STAFF
REPORTS

NO. 1184
FEBRUARY 2026

REVISED
MARCH 2026

Sports Betting Across Borders: Spatial Spillovers, Credit Distress, and Fiscal Externalities

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Federal Reserve Bank of New York Staff Reports, no. 1184
February 2026; revised March 2026
https://doi.org/10.59576/sr.1184

Abstract

Since the 2018 Murphy v. NCAA decision, 38 states have legalized mobile sports betting. We study effects on betting and consumer credit, emphasizing spatial spillovers across state lines. Using consumer spending data and an extended two-way fixed effects framework that separately identifies direct and spillover effects, we find that legalization increases total sportsbook spending roughly tenfold and take-up by 3.1 percentage points. Counties in non-legal states within 15 miles of a legal state experience spillover spending equal to roughly 14 percent of the direct effect, with these spillovers declining to roughly zero by 60 miles. Using the New York Fed Consumer Credit Panel, we find that median credit scores decline by roughly 1 point and overall delinquency rises 0.3 percentage points from a 10.7 percent base, with spillover delinquency rising nearly 0.2 percentage points. Under-40 auto loan delinquency increases by half a percentage point and credit card delinquency by one percentage point, driving the overall increase in delinquency. Scaling the population-level delinquency effect by take-up yields implied delinquency increases of roughly 10 percentage points among induced bettors. We conclude with a policy simulation which reveals that spillovers create a fiscal asymmetry: states that have not legalized bear costs from cross-border betting without capturing tax revenue, giving high exposure states a stronger case for legalization. This incentive is increasing in states that have higher pre-legalization betting activity, population centers near legal states, and a younger population. Methodologically, we show that ignoring spatial spillovers can contribute to attenuated estimates and an under-count of the affected population.

JEL classification: D14, H71, H73, L83
Key words: sports betting, consumer credit, household debt, spatial spillovers, state taxation

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To view the authors' disclosure statements, visit
https://www.newyorkfed.org/research/staff_reports/sr1184.html.

1 Introduction

In the 2018 *Murphy v. NCAA* decision, the Supreme Court struck down a federal law that banned sports betting in the United States. Since then, 38 states have legalized sports betting within their state boundaries, giving rise to a multi-billion dollar mobile sports betting industry (American Gaming Association, 2025). Several large, national sportsbooks have emerged, providing bettors with an opportunity to easily place wagers on sporting outcomes from applications on their mobile devices or web browsers. This ease of use and wide availability has resulted in an explosion of sports wagers, with nearly $520 billion in bets placed between the *Murphy* decision and the first half of 2025 (Bisson, 2025).

At the same time, concerns remain regarding whether nearly frictionless and instant access to sports betting may cause negative outcomes for those who participate. Previous studies have shown that wagering on sports can result in addictive behaviors and reduce consumers' ability to limit their betting activity, leading some individuals to wager more than they would otherwise intend (Gabellini et al., 2023; LaBrie et al., 2007; Meyer et al., 2009; Barone and Graffigna, 2025).

In this paper, we study the impact of legalized sports betting on betting intensity and consumer credit outcomes across U.S. states, with a particular focus on spillovers across state lines into states where betting remains illegal. We exploit the staggered rollout of online sports betting legalization across states following the 2018 *Murphy* decision and implement a difference-in-differences identification strategy.

We begin by estimating the impact of legalization on sportsbook spending as a first-stage result of legalization. Naturally, after a state legalizes sports betting, spending within the state increases dramatically. We estimate that legalization increases total sportsbook spending roughly tenfold, with effects that continue to grow over time. Average quarterly sportsbook spend per person increases by $46 from a pre-treatment mean of $2.50.

Importantly, estimates that ignore spatial spillovers across state lines substantially underestimate the overall impact of legalization. We show that legalization increases betting not only within the legalizing state, but also in neighboring counties located in states where betting is illegal. These spillover effects are substantial, amounting to between roughly 15% of the direct effect depending on the outcome for counties within 15 miles of a legal state, and they decline monotonically with distance to a legal state. Accounting for spillovers increases the estimated first-stage effect of legalization on the share of the population with any sportsbook spending by 6%.

After establishing the first-stage effects and quantifying the extent of cross-state spillovers, we estimate the reduced-form impact of legalization on credit outcomes. We study a broad set of consumer credit outcomes using the New York Fed Consumer Credit Panel, a nationally representative panel of anonymous credit reports from Equifax.

Estimating treatment effects for legal states and spillover counties separately, we find some evidence that legalization lowers credit scores, but the effect is delayed and takes several years to be realized in the overall average. We also find that legalization increases delinquency rates, defined as being at least 90 days past due on any credit product, by 0.31 percentage points in legal counties, relative to a baseline rate of 10.71%. Delinquency rates in spillover counties rise by 0.18 percentage points, or about 58% of the direct effect. Notably, while the first-stage spillover effect on betting intensity is only about 15% as large as the direct effect in legal counties, the spillover effect on delinquency is roughly 60% as large, suggesting that financial distress spreads across borders more strongly than betting activity itself.

These estimates have an intent-to-treat interpretation, reflecting average effects on the full population. However, we estimate that only 3.1% of the population take up active sports betting after legalization, which we define as making at least one sportsbook deposit in a given quarter. We provide a back-of-the-envelope calculation that scales our intent-to-treat estimates by this take-up rate to obtain rough impacts among those induced to bet by legalization. This calculation implies delinquency increases of 10 percentage points among induced bettors—economically large implied effects that underscore the financial risks associated with sports betting.[1]

We further explore additional credit outcomes and heterogeneous effects across age groups. We find that the increase in delinquency is driven primarily by auto loan and credit card delinquencies among younger consumers. For those under 40, auto loan delinquencies increase by 0.55 percentage points (a 5.6% increase) and credit card delinquencies increase by 1.02 percentage

[1] This scaling assumes proportional take-up across demographic groups, that credit impacts operate solely through betting take-up, and that induced bettors are not on systematically different financial trajectories prior to legalization. See Section 5.3 for a complete discussion of these caveats.

points (a 7.9% increase).

Finally, we use our estimates to conduct a back-of-the-envelope cost-benefit simulation for each of the 20 states that had not yet legalized mobile sports betting as of 2025:Q1. We use our estimated first stage and reduced form direct effects and spillover effects as a function of distance from a legal state to project how sportsbook spending, tax revenue, and consumer credit delinquencies would change upon legalization and combine these into a simple metric: the additional tax revenue generated per additional delinquency. The simulation embeds state-level variation in the spatial distribution of the population, the age distribution of the county population, the intensity of sports betting spend before legalization, and the geographic proximity of each county to the nearest legal state. The simulation reveals that states with greater existing spillover exposure face a stronger fiscal case for legalization, because much of the betting-related financial distress is already occurring through cross-border access. Missouri, which has its two largest population centers sitting on the border of legal states and already sees significant sportsbook spending from cross-border activity, would generate substantial new tax revenue while adding relatively few new delinquencies, yielding the highest revenue-per-delinquency ratio of any not-yet-legal state.[2]. Conversely, states like California, where much of the population lives near the coast and far from state borders, face little spillover costs under the status quo, so legalization would represent a larger marginal increase in harm relative to revenue.

This paper contributes to several strands of literature. The first is a longstanding literature on the optimal taxation of goods that generate negative externalities and internalities. Rooted in Pigouvian taxation principles, this framework has been applied extensively to tobacco (Gruber and Kőszegi, 2004), alcohol (Griffith et al., 2019), sugary beverages (Allcott et al., 2019; Dubois et al., 2020), and gambling (Grinols and Mustard, 2006; Kearney, 2005). A central insight from this literature is that optimal corrective taxes must account not only for external harms imposed on others, but also for harms that consumers with self-control problems impose on their future selves (O'Donoghue and Rabin, 2006). We contribute to this literature by providing causal evidence on how legalized sports betting affects household financial outcomes, directly measuring welfare-relevant consequences that should inform optimal policy design both within states and across

[2]Not-yet-legal is measured as of 2025:Q1. Missouri passed a ballot proposition to legalize sports betting at the end of 2025 (Ballotpedia, 2024).

state borders.

More directly, we contribute to a relatively thin literature examining the impacts of betting legalization, and sports betting in particular, in the post-*Murphy* era. To date, this literature consists of a small number of studies estimating the causal effects of betting access on economic and financial outcomes. Evans and Topoleski (2002) study the social and economic effects of Native American casino openings, documenting impacts on local labor market outcomes, income, and related measures of economic well-being, while Grinols and Mustard (2006) documents increases in crime due to casino openings. Building on this broader gambling literature, recent work has focused specifically on sports betting following the *Murphy* decision. Baker et al. (2024) explores how sports betting legalization affects the consumption and investment decisions of consumers. They find only small effects of sports betting legalization on consumption, but larger crowd-out effects on investment, finding that an additional dollar spent on sports betting reduces deposits into brokerage accounts by nearly a dollar. Hollenbeck et al. (2024) explores the impact of in-person and online sports betting on consumer credit outcomes. They leverage the staggered roll-out of both policies across states to estimate how legalization affects credit scores, delinquencies, bankruptcies and collections. They find worsening credit outcomes after sports betting legalization, especially for online sports betting. This includes reductions in credit scores, increases in auto loan delinquencies and increases in bankruptcies. Taylor et al. (2024) analyze a broader set of public and individual outcomes, including sportsbook revenue, tax revenue, problematic betting behavior, helpline calls, and suicides. They find that legalization mechanically raises tax and sportsbook revenue and document increases in irresponsible betting behavior driven by lower income households.

We extend this literature in several important ways. First, we incorporate a large, aggregated dataset of consumer spending with substantial geographic variation at the county level. This spatial richness allows us to construct comprehensive measures of the intensive and extensive margins of mobile sports betting over space. Second, we directly estimate spatial spillovers of sports betting from legal states to states where betting remains illegal. This stands both as a novel mechanism of the policy change and as a factor impacting estimates of any staggered difference-in-differences design with spatial spillovers across borders. Because non-legal states serve as control units in a typical difference-in-differences design, cross-border spillovers into these control units lead to attenuation of estimated treatment effects and contamination of pre-trends. We show that naive specifications that do not account for spatial spillovers underestimate the direct effect of legal sports betting on the share of the population with sportsbook spend by 6%. We further examine a range of credit outcomes and document heterogeneous effects across age groups, with larger increases in delinquency among younger individuals, particularly for credit card and auto loan debt.

Finally, this paper contributes to a broader literature on the political economy and public finance of cross-border economic activity. Our analysis relates to classic models of tax competition with cross-border shopping, in which jurisdictions may bear social costs without capturing associated tax revenue (Kanbur and Keen, 1993). Empirically, our approach aligns with work that exploits borders as economically meaningful sources of variation (Holmes, 1998), as well as studies documenting cross-border responses to policy differences, including recreational marijuana legalization (Hansen et al., 2020), minimum legal drinking age laws (Lovenheim and Slemrod, 2010), cigarette taxation and smuggling (Lovenheim, 2008; Merriman, 2010; Harding et al., 2012; DeCicca et al., 2013), and strategic tax competition at state borders (Agrawal, 2015).

We provide further evidence that legalization of goods and services in one jurisdiction can induce consumption in neighboring jurisdictions where such activity remains illegal. Critically, we show that these spillovers generate a fiscal asymmetry: states that do not legalize bear some of the social costs of betting through cross-border activity while capturing none of the associated tax revenue, whereas neighboring legal states collect increased revenue from diverted spending. This asymmetry may push states toward legalization even when the social costs of betting may exceed tax revenues in isolation, because each state compares legalization to a status quo in which it already bears costs without revenue. More broadly, our findings highlight how ignoring spatial spillovers in staggered-adoption designs leads to meaningfully attenuated estimates of policy effects and an under-count of the affected population. This critique applies to other policy analysis in which states independently regulate goods or activities that generate either cross-border benefits or externalities (e.g. marijuana legalization, minimum wage laws, paid leave mandates, firearm regulations, etc.). We provide an empirical framework for estimating such spillovers when spatially granular data are available.

The paper continues as follows. Section 2 describes the context of state-level legalization in the United States after *Murphy* along with the most relevant papers on post-*Murphy* legalization of sports betting. Section 3 describes the data we use for the analysis. Section 4 discusses our empirical strategy. Section 5 details our main findings, including our first-stage results, reduced-form results, and the separation of spillover effects from direct effects. In Section 6, we discuss how we use the results of our estimation to examine the legalization decision in the presence of spatial spillovers. Section 7 concludes the paper.

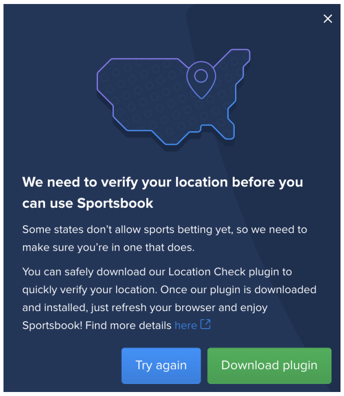
2 Background

The Professional and Amateur Sports Protection Act (PASPA), enacted in 1992, effectively prohibited sports betting nationwide, with limited exceptions for states such as Nevada that were grandfathered under the law. In 2011, New Jersey voters approved a constitutional amendment to legalize sports betting within the state. The state was subsequently sued under PASPA and initially lost due to existing statutes that conflicted with the new amendment. After enacting revised legislation, New Jersey challenged PASPA once again. In *Murphy v. NCAA (2018)*, the U.S. Supreme Court ruled that PASPA violated the Tenth Amendment, thereby granting states the authority to regulate sports betting independently.

In the aftermath of the *Murphy* decision, several states moved to legalize sports betting. While most have authorized mobile betting (typically conducted through smartphone applications or web platforms), several states continue to require in-person wagering. As of this writing, 38 states permit mobile sports betting, and several additional states have active legislation under consideration. In 2024, total wagering volume exceeded $150 billion, and a substantial industry has emerged around sports betting, including commercial partnerships between sportsbooks and both professional and collegiate athletic organizations.

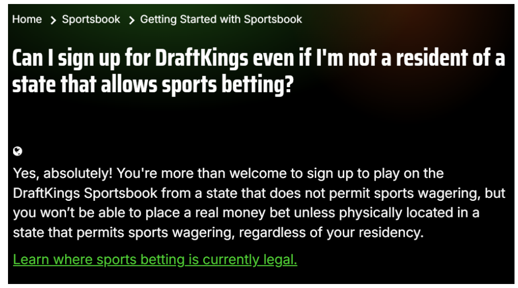
To participate in mobile sports betting, individuals simply need to download a sportsbook application to their smartphone. Regulations vary slightly by state, but bettors must typically complete an identity verification to confirm their age and a geolocation check to verify their physical

(a) Example of geolocation restrictions with mobile sports betting



Source: FanDuel Sports.

(b) Leading sportsbook FAQ regarding out-of-state betting



Source: DraftKings.

location at the time of betting. Importantly, geolocation is based on a bettor's real-time location rather than their state of residence (see Figure 1a). This feature creates a natural channel for spatial spillovers. Residents of states where betting remains illegal can travel to a legal state to satisfy the geolocation requirement when opening an account, and can subsequently return to a legal state whenever they wish to place additional bets. In fact, Figure 1b shows how sportsbooks even advertise this feature. This structure imposes a travel cost that increases with distance to the nearest legal-state border. In Section 4, we show that the estimated spillover effects decline nearly linearly with this distance.

Advocates for the legalization of mobile sports betting commonly cite increased tax revenues as the primary benefit. States vary widely in both the level and structure of sports betting taxation. Most states impose an ad valorem tax on sportsbook revenues, typically defined as gross gaming revenue (GGR), or sportsbook profitability from gaming activity prior to operating expenses. Some states instead tax the total betting handle, and Illinois has recently enacted an additional per-bet tax. In Section 6, we report comparable implied GGR tax rates across states, as compiled by Hoffer and Macumber-Rosin (2025).

The rapid expansion of the industry has prompted growing concern about its potential negative externalities, particularly in an era of ubiquitous digital access. Despite the industry's scale and visibility, there remain only a few causal studies on the effects of legalized sports betting on consumer behavior and welfare. First, Baker et al. (2024) uses the staggered rollout of online sports betting across states to explore its effect on consumption and investment decisions. They use data from an undisclosed financial aggregation platform that combines accounts within a household to include banks, credit card firms, and FinTechs to create a comprehensive view of an individual's financial portfolio. They limit their sample to a random 10% sample of users who rank the highest on the platform's completeness and tenure measure in order to create a panel of users for whom they most likely have a complete view of their finances. Additionally, Baker et al. (2024) acknowledges that spatial spillovers across state borders are problematic for estimation, so they omit border counties from the sample.[3]

Hollenbeck et al. (2024) also exploits the staggered rollout of legalization over time, but instead focuses on the impact of legalization on consumer credit outcomes. They aggregate credit outcomes to a county-quarter level and weight counties by the average numbers of individuals in 2015. For outcomes, they focus on credit scores as a summary measure of financial health as well as several other measures of credit health such as the number of bankruptcies, the presence of debt in collections, and the presence of credit card and auto delinquencies. For evaluating the policy, Hollenbeck et al. (2024) explore separately the impact of any sports betting legalization and online sports betting, noting that online access will likely have different effects than states where bets must be placed in-person.

Third, Taylor et al. (2024) explores the impact of legalized sports betting on problematic betting behavior (among other outcomes). Unlike Baker et al. (2024) and Hollenbeck et al. (2024), Taylor et al. (2024) does not exploit the staggered rollout of sports betting legalization in their empirical strategy. Instead, they employ a Generalized Synthetic Control framework where each legalizing state is paired with a constructed "control" state where the comparison state is formed by a weighted average of many not-legal states with weights derived from the evolution of pretreatment variables.

We differ from these three papers in two key dimensions which enhance our collective understanding of the impacts of post-*Murphy* sports betting legalization and help us to better characterize the costs and benefits of legalizing sports betting in the presence of spatial spillovers. First, and most importantly, we acknowledge and explicitly estimate the effect of cross-border spillover

[3]It is unclear whether border counties that ultimately adopt later in the panel are omitted from entire panel or only prior to adoption.

of sports betting activity. While Baker et al. (2024) acknowledges cross-border spillovers, their solution is to drop counties that border a legal state. While this will reduce attenuation bias from spatial spillovers, it will not eliminate it.[4] Neither Hollenbeck et al. (2024) nor Taylor et al. (2024) directly account for spatial spillovers in their empirical framework. For Taylor et al. (2024), this is not a concern for tax revenue or spending outcomes as each of these is measured within the legal state.[5] However, estimates from outcomes that include spillovers into not-yet-legal states used as controls will likely lead to an underestimate of the effects of legalized sports betting. Our explicit modeling of spatial spillovers helps to reduce bias in the estimation of the effects of sports betting legalization and it stands as a novel policy mechanism that helps inform the relative costs and benefits of a state's legalization decision.

Second, we use a comprehensive transaction-level dataset covering more than 80 million financial accounts to estimate the first stage impact of legalization on betting activity within the legal state and across state lines. Transactions are linked to household identifiers allowing us to explore and disentangle both the extensive and intensive margin of sportsbook spending. We construct the extensive margin of spending by computing the share of households in the sample in a county-quarter that has any online sportsbook transactions. We also measure the intensive margin at the county-quarter level with the (log of) total sportsbook spending and at the individual level by computing the average quarterly spend (both conditional and unconditional on any betting transactions) at the county level. These various constructions allow us to comprehensively detail how betting activity changes after legalization, the extent to which betting activity spills across state lines, and how quickly it declines as distance from a legal state increases.

3 Data

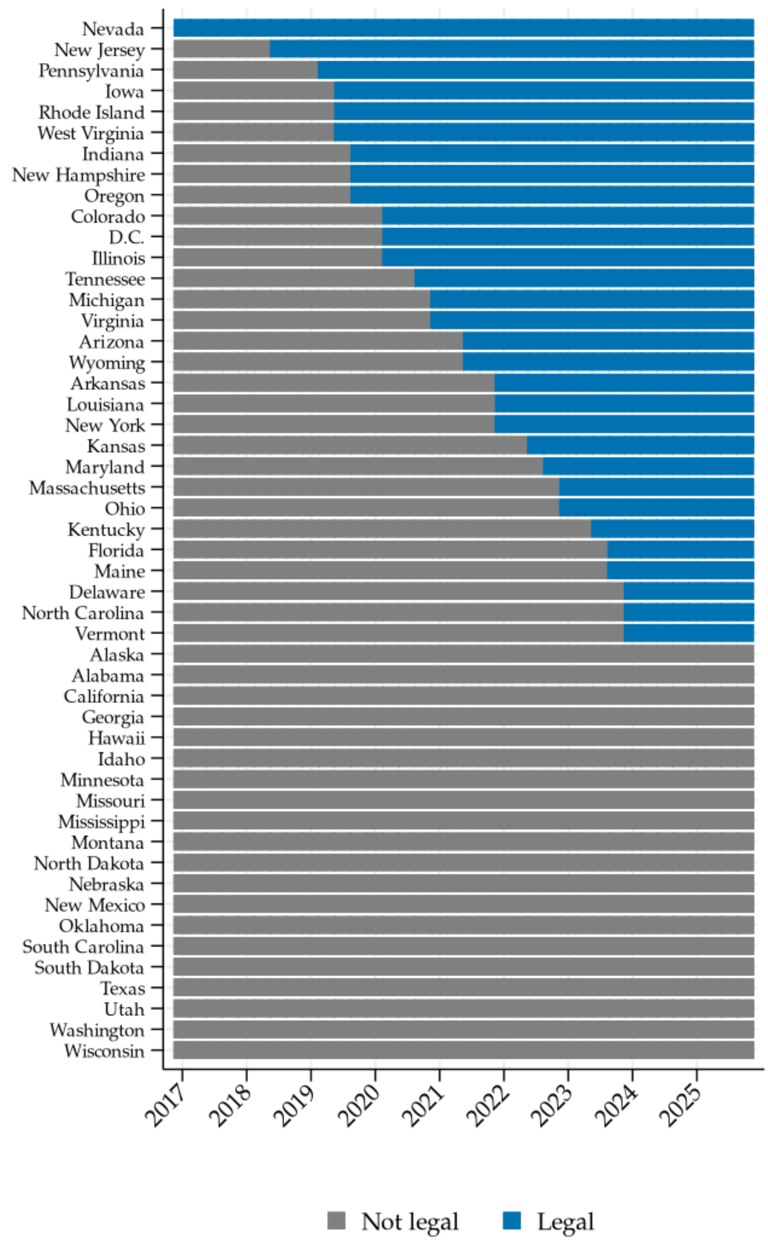
We use three primary sources of data for our analysis. First, we use hand collected dates of the first legal instance of mobile sports betting for each state since the *Murphy v. NCAA* decision in 2018. Figure 2 summarizes the states that passed legislation legalizing mobile sports betting,

[4]For instance, this omission will remove Jackson, Clay, and Platte Counties in Missouri, which contain Kansas City and show large spillover effects from Kansas's legalization. Additionally, this would remove New York County due to its bordering of New Jersey. Figure 3 shows the share of the adult population living near legal states in each quarter.

[5]For instance, spending by a resident of St. Louis within Illinois is coded as spending in Illinois.

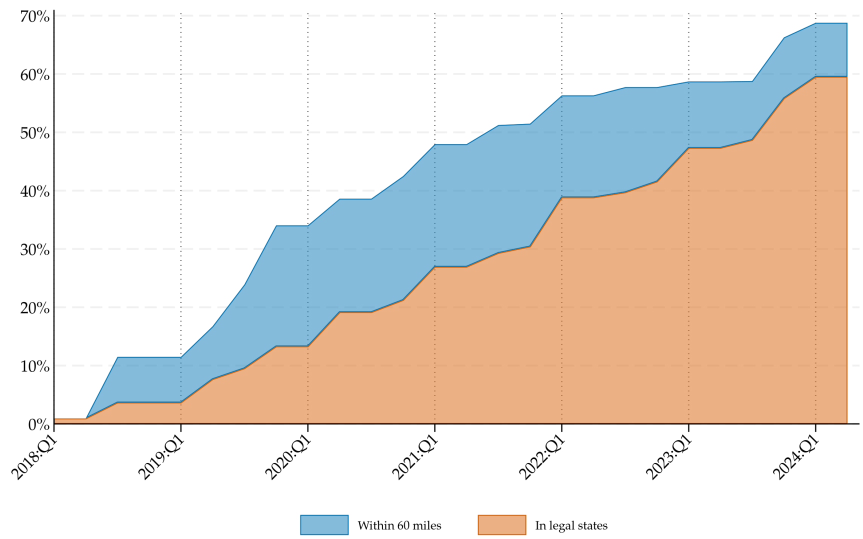
sorted by the effective date of legalization. Additionally, Figure 3 shows the share of the adult population residing in a legal state (in red) and the share of the adult population either living in a legal state or living in a county whose population-weighted centroid is within 60 miles of a legal state (in blue). Here we see that between 2020 and 2023, a substantial share of the population lived within 60 miles of a legal state, but only around half of them actually resided within a legal state.

Figure 2: Mobile sports betting legal periods by state



Notes: Panel A: Sports betting has been legal in Nevada since 2010, however offsite sports betting requires users to use an application associated with an established casino, register in-person at the casino, and place their first deposit in-person and in cash at the casino.
Source: Author collected dates.

Figure 3: Share of adult population with access to mobile sports betting



Notes: The figure above shows the share of the adult population living in states where mobile sports betting is legal (in red) by quarter along with the additional share of the adult population living in a county whose population-weighted centroid is within 60 miles of a legal state (in blue). Source: American Community Survey.

3.1 Earnest Analytics: Consumer Spending Data

The next primary data source contains transaction-level anonymized consumer spending data provided by Earnest Analytics. The data is sourced from anonymized account statements linked to the same households and spans over 80 million credit, debit, and checking accounts. Each row in the data is a transaction and includes an anonymized household identifier, the amount and type of transaction, the merchant and location of the transaction, whether the transaction was online or in-person, and the retail category and subcategory of the merchant. Additionally, the data include location information on the household down to the Census Place level, which corresponds to incorporated cities, towns, and Census Designated Places (CDPs). We geocode each location to its centroid coordinates and spatially join these points to county boundaries to assign each household to a county. We then aggregate transaction-level data to the county-quarter level for analysis. We mark a transaction as online mobile sports betting if the transaction type is online (as opposed to in-person) and it comes from one of the major U.S. sportsbooks: BallyBet, Bet365, BetMGM, BetRivers, Caesar's Sportsbook, DraftKings, ESPN Bet, FanDuel, HardRock Bet, PointsBet, The Score Bet, Underdog Sports, and Wynn Bet. We note, however, that our measure of sportsbook spend is an underestimate of the scale of total wagers since bettors can use winnings towards future bets. Instead, we only observe new deposits into mobile betting sportsbooks and do not observe subsequent wagers placed via winnings.

The data identify each spender's place of residence rather than their physical location at the time of spending. To construct our spillover measures, we therefore assume that any spending by residents of states in which sports betting remains illegal is transacted in the geographically nearest legal state.

The county-quarter panel begins in the first quarter of 2018 and spans through the third quarter of 2025. To ensure reliable estimates, we restrict the analysis sample to counties with an average of at least 75 unique households with any transactions per quarter. This threshold balances the need for statistical precision against maintaining broad geographic coverage. The resulting sample contains a balanced panel with 1,690 counties across 31 quarters.

From the raw aggregates, we construct four primary outcome measures:

1. **Total transaction amount**: The sum of all online sportsbook spending within a county-quarter. For regression analysis, we use the natural logarithm of one plus this amount.

2. **Share with any sportsbook spend**: The percentage of unique households with any transactions in a county-quarter that had at least one online sportsbook transaction. This extensive margin measure captures the take-up and prevalence of betting activity.

3. **Quarterly spend per person**: Total county-quarter sportsbook spending divided by the number of unique households with any transactions in that county-quarter. This measure captures average betting exposure across the full population.

4. **Quarterly spend per bettor**: Total county-quarter sportsbook spending divided by the number of unique households with sportsbook transactions.[6] This intensive margin measure captures spending conditional on participation.

Table 1 reports averages for each of the spending outcome variables split into two populations separately toward the beginning and end of our analysis. The first population consists of states that legalized mobile sports at any point prior to 2025:Q1, while the second consists of states that

[6]For the spend per bettor measure, we set values to zero for county-quarters with no sportsbook transactions to avoid division by zero.

Table 1: Descriptive statistics for online sports betting spend by legalization status

	2018Q1		2025Q1	
	Eventually Legal	Never Legal	Eventually Legal	Never Legal
Total Sportsbook Spend ($)	47,144	10,137	8,000,906	535,993
Share with Sportsbook Spend (%)	0.433	0.290	5.63	1.28
Average Spend per Person ($)	1.21	0.892	87.76	13.30
Average Spend per Bettor ($)	272	295	1,430	846

Notes: Eventually Legal states are those that legalized mobile sports gambling by 2025Q1. Never Legal states had not legalized by 2025Q1. Total Sportsbook Spend is the sum of all online spending at sportsbooks within a county-quarter. Share with Betting Spend is the percentage of unique households with any transactions that had at least one online transaction with a sportsbook. Average Spend per Person is total online sportsbook spending divided by the number of unique households with any transactions. Average Spend per Bettor is total online sportsbook spending divided by the number of unique households with an online sportsbook transaction. All statistics are population-weighted county-quarter averages.
Source: Earnest Analytics.

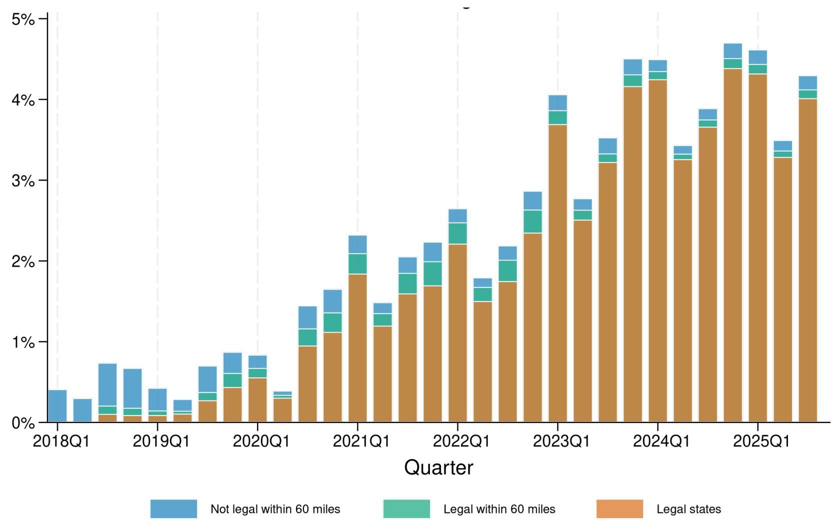
had not legalized by that date. In 2018:Q1, prior to widespread legalization, sportsbook spending was minimal and similar across both groups, reflecting the limited legal avenues for online sports betting.[7] By 2025:Q1, the eventually-legal states show substantially higher spending levels across all measures, with a marked increase in the share of households with any betting activity and a large rise in average spending per bettor. Never-legal states also experience modest increases, consistent with the cross-border spillover effects documented in our empirical analysis.

Figure 4 shows the evolution of the share with any sportsbook spend in a quarter for each quarter broken out by whether the household lived in a legal state, a not-legal county within 60 miles of a legal state, or a not legal county further than 60 miles from a legal state. We see some online sportsbook spend prior to *Murphy*, primarily driven by Daily Fantasy Sports (DFS). By 2020, however, the vast majority of the growth in the population with online sportsbook spend was located in legal states or not legal counties within 60 miles of a legal state. By the end of the series, the share of the population with any sportsbook transaction in a quarter was between 4-5%. The time series demonstrates considerable seasonality as well, with peaks in Q4 and Q1 coinciding with the NFL regular season and playoffs, respectively.

In addition to the Earnest Analytics data, we also replicate our results on an alternate spending dataset in Section Online Appendix A.5.

[7]Many of the sportsbooks in our sample previously operated primarily as Daily Fantasy Sports platforms, which allow users to assemble teams of athletes and compete against other players based on the fantasy points generated by those athletes' on-field performance.

Figure 4: Share with any online sportsbook spend by legal status and distance to legal states



Notes: The bars in the figure show the total share of the population of Earnest households with at least one online transaction with a sportsbook each quarter, separately for three groups. In blue are households that live in a county further than 60 miles from a legal state. In orange are households that live within 60 miles of a legal state but not within a legal state. In red are households that live within a state that is legal in that quarter.
Source: Earnest Analytics.

3.2 New York Fed Consumer Credit Panel: Credit Outcomes

Lastly, we use the New York Fed Consumer Credit Panel, a nationally representative 5% sample of anonymous credit reports from Equifax, to construct credit outcome measures. It includes credit details such as balances, new originations, delinquency status, bankruptcies, and foreclosures as well as borrower age and location. To construct outcome data, we aggregate outcomes to the county–quarter level for the full sample and also create subsamples of county–quarter observations for three broad age groups: 1) under 40, 2) 40 to 64, and 3) Over 65. We require counties to have an average population of at least 15 individuals across all quarters to ensure reliable rate calculations. Within each age group, we retain only counties that appear in all quarters of the panel to create a balanced sample. This ensures that changes over time reflect changes in outcomes rather than compositional shifts in the sample. We prefer the county construction in order to match the data structure and geographic variation in the first-stage. However, we also construct an analogous ZIP-code sample which has finer spatial variation and a larger sample size. We use this sample for increased precision in some heterogeneity analysis.

We construct the following credit outcome measures:

Credit Scores: We use the median Equifax Risk Score 3.0 for each county-quarter-age group cell. Credit scores are calculated with proprietary algorithms and use attributes within credit reports to calculate default risk and map (the inverse of) default risk into a score ranging from 350 to 850 where higher scores translate to lower expected default probability.

Delinquency Rates: For each debt product type, we compute the delinquency rate as the number of individuals with any account 90 or more days past due divided by the number of individuals with any account of that type, multiplied by 100 to express results as a percentage.[8] We construct separate delinquency rates for: 1) credit cards, 2) auto loans, 3) mortgages, 4) student loans, and 5) any debt product (overall delinquency rate).

Financial Distress Rates: We compute the share of individuals with either a bankruptcy or a foreclosure as a percentage of the county credit report population to measure severe adverse credit events.

Table 2 reports averages for each of the credit outcome variables split into two populations separately toward the beginning and end of our analysis. Again, the first population consists of states that eventually legalized mobile sports betting by 2025:Q1, while the second consists of states that had not legalized by that date. At baseline in 2018:Q1, the two groups exhibit similar credit profiles, with comparable average credit scores, delinquency rates, and financial distress rates. This similarity supports the parallel trends assumption underlying our difference-in-differences design. By 2025:Q1, both groups show improvements in credit scores, but delinquency trends vary. Credit card and auto loan delinquency has increased while student loan and mortgage delinquency has declined.[9]

Our empirical strategy isolates the causal effect of legalization from these secular trends by comparing the differential changes between the two groups while accounting for the staggered timing of legalization across states. Because the CCP does not allow us to identify which individuals actually place sports bets, we cannot condition on betting participation. As a result, the treatment effects we estimate using the CCP reflect the impact of being exposed to legal access

[8]For county-quarters where no individuals hold a particular debt product, we set the delinquency rate to zero rather than missing.

[9]Student loan delinquency rates dropped to below one percent during the pandemic due to the administrative forbearance on federal student loans. Delinquencies began reporting again in early 2025, driving these rates higher again. However, rates remained artificially low during most of our analysis period. See Haughwout et al. (2025) for more on these dynamics.

Table 2: Descriptive statistics for credit outcomes by legalization status

	2018Q1		2025Q1	
	Eventually Legal	Never Legal	Eventually Legal	Never Legal
Median Credit Score	726.7	719.5	742.8	737.0
Delinquency Rate, Any Product (%)	11.13	12.17	13.01	13.71
Delinquency Rate, Credit Card (%)	8.19	8.53	12.23	12.33
Delinquency Rate, Auto Loan (%)	7.31	7.86	8.38	8.71
Delinquency Rate, Mortgage (%)	1.77	1.40	1.33	1.25
Delinquency Rate, Student Loan (%)	17.60	18.22	12.65	13.41
Bankruptcy Rate (%)	4.06	3.92	1.78	1.60
Foreclosure Rate (%)	0.66	0.59	0.17	0.14

Notes: Eventually Legal states are those that legalized mobile sports gambling by 2025Q1. Never Legal states had not legalized by 2025Q1. Median Credit Score is the Equifax Risk Score 3.0. Delinquency rates are computed as the share of the credit population in each county-quarter with any account 90 or more days past due, conditional on having an account of that product type for product-specific rates. Bankruptcy and foreclosure rates are the share of the credit population with any such flag on their credit report. All statistics are population-weighted county-state-quarter averages for all age groups.
Source: New York Fed Consumer Credit Panel/Equifax.

to mobile sports betting in one's state, not the effect of betting itself. These estimates therefore have an Intent-to-Treat (ITT) interpretation with respect to the legalization policy. We then combine these ITT credit effects from the CCP with take-up and spending responses estimated in the Earnest data, where individual betting behavior is observed, to infer the implied treatment effects for induced bettors. The empirical strategy is described more completely in the next section.

4 Empirical Strategy

Estimating the causal effect of sports betting legalization on consumer financial outcomes presents two key challenges. First, states legalized mobile sports betting at different times following the 2018 *Murphy v. NCAA* decision, creating a staggered adoption design. Recent econometric literature has shown that standard two-way fixed effects (TWFE) estimators can produce biased estimates in staggered settings when treatment effects vary across cohorts or over time (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021). Second, as Figure 3 and Figure 4 illustrate, residents of counties near state borders can access sports betting in neighboring legal states, creating spatial spillovers that contaminate the comparison group. Ignoring these spillovers would bias estimates of both direct treatment effects and the counterfactual outcomes for not-yet-treated units.

To address both challenges simultaneously, we follow the extended two-way fixed effects (ETWFE) approach developed in Wooldridge (2025) and extended to accommodate spillovers in Fiorini et al. (2024).[10] Our approach treats exposure to sports betting as a two-dimensional treatment vector, allowing us to separately identify the direct effects of legalization and cross-border spillover exposure.

Let i index counties and t index quarters.[11] Each county is characterized by two potential sources of exposure to legal sports betting. Let $G_i \in \{2018q1, 2018q2, \ldots, 2025q4, \infty\}$ denote the quarter in which county i's own state first legalizes mobile sports betting, with $G_i = \infty$ for states that never legalize during the sample period. Second, let $Q_i \in \{2018q1, 2018q2, \ldots, 2025q4, \infty\}$ denote the quarter in which county i first becomes exposed to spillovers: the first quarter in which at least one neighboring state within a distance threshold of the county i's (population-weighted) centroid has legalized, conditional on the county's own state not yet having legalized. For counties never within range of a legal state, or whose own state legalizes before any neighboring state, $Q_i = \infty$.

This two-dimensional treatment structure generates four mutually exclusive exposure paths:

0. **Never treated** ($Q_i = \infty, G_i = \infty$): Counties that never experience spillover exposure and whose own state never legalizes. These counties in the interior of persistently non-legal states serve as the primary control group.

1. **Direct to legal** ($Q_i = \infty, G_i = g$): Counties whose own state legalizes before any neighboring state, so they transition directly from untreated to directly treated without an intervening spillover period.

2. **Spillover to legal** ($Q_i < G_i < \infty$): Counties that first experience spillover exposure when a neighbor legalizes, then later transition to direct treatment when their own state legalizes.

3. **Spillover only** ($Q_i = q, G_i = \infty$): Counties that become exposed to a neighboring legal state

[10] The estimator proposed in Wooldridge (2025) is functionally equivalent to the 2x2 estimators proposed in Callaway and Sant'Anna (2021) with a more flexible implementation.

[11] We measure spending outcomes at the county level and credit outcomes at either the county or the ZIP code level. We refer to counties here for ease of notation.

but whose own state never legalizes during the sample period.

We summarize time-varying exposure using two mutually exclusive indicators. The direct treatment indicator,

$$D_{it} = \mathbb{1}[t \geq G_i],$$

equals one once county i's own state has legalized sports betting. The spillover indicator,

$$S_{it} = \mathbb{1}[G_i > t \geq Q_i],$$

equals one during the period in which county i is exposed to a neighboring legal state but before own-state legalization. By construction, D_{it} and S_{it} are mutually exclusive: once a county's own state legalizes ($D_{it} = 1$), the spillover indicator turns off ($S_{it} = 0$). A county's first treatment of any kind occurs at $\min\{Q_i, G_i\}$.

To facilitate interpretation and pre-trend testing, we also define two time-invariant indicators capturing eventual treatment status. Let $\tilde{D}_i = \mathbb{1}[G_i < \infty]$ indicate that a county's state legalizes at some point during the sample period, and let $\tilde{S}_i = \mathbb{1}[Q_i < G_i]$ indicate that a county will experience spillover exposure prior to its own-state legalization.

4.1 Identifying Assumptions

Identification relies on a set of standard assumptions for difference-in-differences with staggered adoption and spillovers, adapted from Fiorini et al. (2024). First, in the absence of both own-state legalization and exposure to neighboring-state legalization, counties would have followed parallel trends in outcomes, conditional on fixed effects. Second, there is no anticipation: outcomes do not respond in advance of either own-state or neighboring-state legalization. Third, current outcomes depend only on current and past treatment status, not on the timing of future policy changes. These conditions are evaluated using pre-period event-study coefficients.

The key additional assumption required to accommodate spillovers is "immunity": any spillover treatment becomes null once the unit is fully treated. In this setting, this requires assuming that once a county's own state legalizes, spillover effects from neighboring states no longer operate. Spillovers arise from cross-border travel to place legal bets; once in-state legal access becomes available, such travel is no longer necessary. The convenience of in-state mobile access therefore effectively eliminates the demand for cross-border betting that drives the spillover.

4.2 Extended Two-way Fixed Effects (ETWFE) Specification

Following the ETWFE approach of Wooldridge (2025), we estimate a fully saturated model with cohort-specific coefficients for each event-time period, jointly identifying the direct effects of own-state legalization and the spillover effects of neighbor-state legalization. We then aggregate these cohort-level parameters by event time to construct a combined event-study profile, and by overall treatment status to report difference-in-differences estimates. Let $\mathcal{C}$ denote the set of all treatment cohorts (q, g), excluding the never-treated cohort (∞, ∞). For each cohort, event time is defined as $e = t - \min(q, g)$, which measures time relative to the first occurrence of any treatment.

Let $\underline{p}$ and $\bar{p}$ denote the lower and upper bounds of the event-time window used in the event study. In our preferred specification, we set $\underline{p} = -6$ and $\bar{p} = 12$, and we bin event times outside this window into the corresponding endpoint categories.

The estimating equation is:

$$\begin{aligned} Y_{it} = \alpha_i + \lambda_t + \sum_{(q,g)\in\mathcal{C}} \mathbf{1}[(Q_i, G_i) = (q, g)] \Bigg[& \gamma_{qg}^{<\underline{p}} \cdot \mathbf{1}[e_{it} < \underline{p}] + \gamma_{qg}^{>\bar{p}} \cdot \mathbf{1}[e_{it} > \bar{p}] \\ & + \sum_{\substack{e=\underline{p} \\ e\neq -1}}^{\bar{p}} \gamma_{qg,e} \cdot \mathbf{1}[e_{it} = e] \Bigg] + \varepsilon_{it} \end{aligned} \tag{1}$$

where $e_{it} = t - \min(q, g)$ is the event time for county i in period t, and $e = -1$ is omitted as the reference period for each cohort. α_i and λ_t are county and quarter fixed effects, respectively. Next, $\gamma_{qg}^{<\underline{p}}$ and $\gamma_{qg}^{>\bar{p}}$ are cohort specific binned endpoints for periods before and after the event time in the ultimate event study, respectively. The parameters of interest are the set of $\gamma_{qg,e}$, which measure the cohort-event-time specific coefficients for cohort (q, g) at event time e. Standard errors are clustered at the state-by-treatment-cohort level (q, g), so states that experience spillover exposure at multiple distinct dates are split into multiple state-cohort clusters.

To clarify how we construct separate event studies for direct treatment and spillover effects, we decompose the inner sum by treatment status. Within the event window $[\underline{p}, \bar{p}]$, we partition the event times for each cohort into three mutually exclusive sets based on treatment status at that event time: pre-treatment periods ($e < 0$), periods under direct treatment ($D_{it} = 1$), and periods under spillover exposure ($S_{it} = 1$).

For each cohort (q, g), define:

- $\mathcal{E}_{qg}^{pre} = \{e : \underline{p} \leq e < 0, e \neq -1\}$: the set of pre-treatment event times within the estimation window
- $\mathcal{E}_{qg}^{D} = \{e : 0 \leq e \leq \bar{p}, D_{it} = 1\}$: the set of event times under direct treatment
- $\mathcal{E}_{qg}^{S} = \{e : 0 \leq e \leq \bar{p}, S_{it} = 1\}$: the set of event times under spillover exposure

We can then rewrite Equation (1) as:

$$\begin{aligned} Y_{it} = \alpha_i + \lambda_t + \sum_{(q,g)\in\mathcal{C}} \mathbf{1}[(Q_i, G_i) = (q, g)] \Bigg[& \gamma_{qg}^{<\underline{p}} \cdot \mathbf{1}[e_{it} < \underline{p}] + \gamma_{qg}^{>\bar{p}} \cdot \mathbf{1}[e_{it} > \bar{p}] \\ & + \sum_{e\in\mathcal{E}_{qg}^{pre}} \gamma_{qg,e}^{pre} \cdot \mathbf{1}[e_{it} = e] \\ & + \sum_{e\in\mathcal{E}_{qg}^{D}} \gamma_{qg,e}^{D} \cdot \mathbf{1}[e_{it} = e] \\ & + \sum_{e\in\mathcal{E}_{qg}^{S}} \gamma_{qg,e}^{S} \cdot \mathbf{1}[e_{it} = e] \Bigg] + \varepsilon_{it} \end{aligned} \tag{2}$$

This decomposition makes explicit how the cohort-event-time coefficients map to the two event studies of interest:

- For **direct-to-legal** cohorts ($q = \infty, g < \infty$): $\mathcal{E}_{qg}^{S} = \emptyset$ (no spillover periods), and all post-treatment coefficients belong to $\mathcal{E}_{qg}^{D}$.
- For **spillover-only** cohorts ($q < \infty, g = \infty$): $\mathcal{E}_{qg}^{D} = \emptyset$ (no direct treatment periods), and all post-treatment coefficients belong to $\mathcal{E}_{qg}^{S}$.
- For **spillover-to-legal** cohorts ($q < g < \infty$): Both sets are non-empty. Event times $e \in \{0, 1, \ldots, g - q - 1\}$ (when $S_{it} = 1$) belong to $\mathcal{E}_{qg}^{S}$, while event times $e \geq g - q$ (when $D_{it} = 1$) belong to $\mathcal{E}_{qg}^{D}$.

$\hat{\gamma}^{D}_{qg,e}$ and $\hat{\gamma}^{S}_{qg,e}$ then estimate coefficients relating to the direct and spillover effects, respectively. The pre-period estimates $\hat{\gamma}^{pre}_{qg,e}$ are used to test the parallel trends assumption and are not included in the treatment effect aggregation.

Note that we can recover the standard ETWFE estimates (without spillovers) by setting $Q_i = \infty\ \forall i$. In this case, the spillover periods for spillover-to-legal counties become the pre-periods for their legalization while the spillover periods for spillover-only counties become never-treated counties. To benchmark the extent of potential bias caused by spatial spillovers, we also estimate this "naive" specification that ignores spillovers and compare the estimated effects of legalization $\hat{\gamma}^{D}_{qg,e}$ between the naive specification and the specification that explicitly accounts for spatial spillovers.

4.3 Aggregating to Event Study Estimates

We construct two event studies from the ETWFE estimates: one for the direct effect (event time relative to own-state legalization g) and one for spillover effects (event time relative to first spillover exposure q). Because the specification accommodating spillovers defines event time relative to first treatment of any kind, $e = t - \min(q, g)$, aggregating to these event studies requires careful attention to which cohort-time estimates contribute to each event study and how event times must be remapped.

Direct Effect Event Study. The direct effect event study estimates dynamic treatment effects relative to own-state legalization, with event time defined as $e^D = t - g$. This event study draws on estimates from cohorts that experience either form of own-state legalization: direct-to-legal ($q = \infty, g < \infty$) and spillover-to-legal ($q < g < \infty$). For direct-to-legal cohorts, first treatment occurs at own-state legalization, so the ETWFE event time equals the legalization event time: $e = t - g = e^D$. Both pre-period and post-period estimates map directly into the legalization event study without remapping. For spillover-to-legal cohorts, the ETWFE event time is defined relative to first spillover exposure: $e = t - q$. To convert to legalization event time, we subtract the duration of the spillover period:

$$e^D = e - (g - q) \tag{3}$$

where $(g - q)$ is the number of quarters between first spillover and own-state legalization.

Only the post-legalization estimates from spillover-to-legal cohorts (i.e., estimates with $e \geq g - q$, corresponding to $e^D \geq 0$) are included in the direct event study. We exclude the pre-spillover periods from these cohorts because these estimates are temporally separated from the own-state legalization and thus do not provide an appropriate test for parallel trends in the periods immediately preceding own-state legalization.

For each direct event time e^D, we aggregate across contributing cohorts weighting parameters by the number of counties in each cohort:

$$\hat{\gamma}^D_{e^D} = \sum_{(q,g) \in \mathcal{C}^D_{e^D}} \omega^D_{qg,e^D} \cdot \hat{\gamma}_{qg,e} \tag{4}$$

where $\mathcal{C}^D_{e^D}$ is the set of cohorts contributing to legalization event time e^D, and ω^D_{qg,e^D} weights each cohort by the share of total counties within each cohort within each period of event time. For the pre-period ($e^D < 0$), only direct-to-legal cohorts contribute. For the post-period ($e^D \geq 0$), both direct-to-legal and spillover-to-legal cohorts contribute, with the spillover-to-legal estimates remapped via the equation above.

Spillover Effect Event Study. The spillover event study estimates dynamic effects relative to first spillover exposure, with event time defined as $e^S = t - q$. This event study draws on estimates the spillover-only cohorts ($q < \infty, g = \infty$) and spillover-to-legal cohorts ($q < g < \infty$). For spillover-only cohorts, first treatment occurs at spillover exposure, so the ETWFE event time equals the spillover event time: $e = t - q = e^S$. Both pre-period and post-period estimates are included in the spillover event study. For spillover-to-legal cohorts cohorts, the ETWFE event time is also relative to first spillover: $e = t - q = e^S$, so no remapping is required. Pre-period estimates from all spillover-to-legal cohorts are included regardless of spillover duration.

For each spillover event time e^S, we aggregate across contributing cohorts weighting parameters by the number of counties in each cohort:

$$\hat{\gamma}^S_{e^S} = \sum_{(q,g) \in \mathcal{C}^S_{e^S}} \omega^S_{qg,e^S} \cdot \hat{\gamma}_{qg,e} \tag{5}$$

where $\mathcal{C}^S_{e^S}$ is the set of cohorts contributing to spillover event time e^S, and the weights ω^S_{qg,e^S}.

4.4 Aggregating to Difference-in-Differences Estimates

Following Wooldridge (2025), we aggregate the event study estimates into summary difference-in-differences treatment effects by averaging over post-period event times.

Direct Treatment Effect ($\hat{\tau}^D$):

$$\hat{\tau}^D = \sum_{e^D=0}^{\bar{p}^D} \omega^D_{e^D} \cdot \hat{\gamma}^D_{e^D} \tag{6}$$

where $\bar{p}^D$ is the maximum post-period event time in the legalization event study, and $\omega^D_{e^D}$ weights each event time by the share of counties in each cohort-event time cell out of the total of all county-quarter post period units. We compute the **Spillover Effect ($\hat{\tau}^S$)** similarly:

$$\hat{\tau}^S = \sum_{e^S=0}^{\bar{p}^S} \omega^S_{e^S} \cdot \hat{\gamma}^S_{e^S}. \tag{7}$$

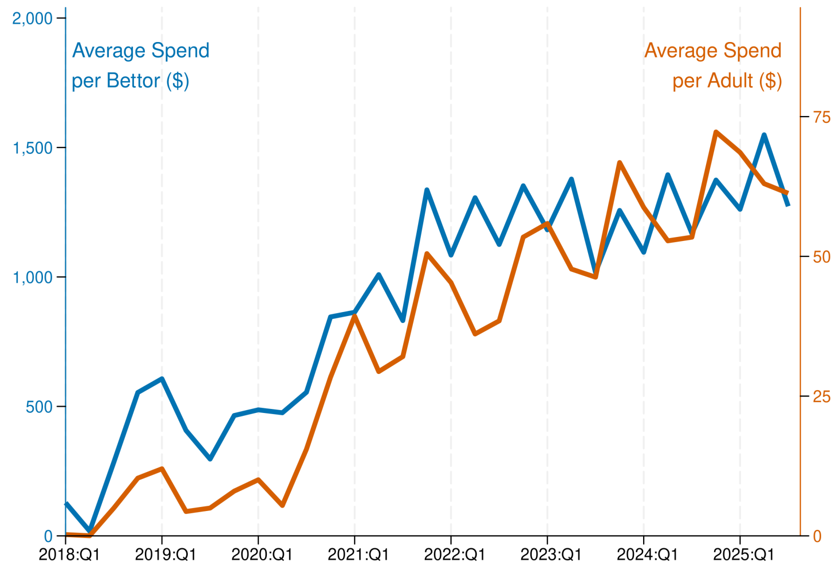
These aggregated estimates have a natural interpretation: $\hat{\tau}^D$ measures the average effect of own-state legalization relative to the period immediately prior to legalization, while $\hat{\tau}^S$ measures the average effect of cross-border spillover exposure relative to the period immediately prior to first spillover. Standard errors for these aggregated parameters are computed via the delta method using the variance-covariance matrix from the ETWFE specification.

4.5 Implementation

In this section, we discuss the choice of parameter values and restrictions to implement the empirical strategy above for our specific context. First, we make two restrictions related to the timing of the COVID-19 pandemic. Our baseline specification omits treatment cohorts whose own-legalization event occurs prior to the first quarter of 2020.[12] Because the market of legal states was initially small and sportsbooks had access to only a limited set of potential customers, betting activity at many platforms did not meaningfully ramp up until after the COVID-19 pandemic.

[12] Note that these parameters are still estimated in the ETWFE regressions, but we set ω weights to zero for the event study and DD estimates.

Figure 5: Average sportsbook deposits per quarter in legal states.



Notes: The figure plots quarterly average sportsbook spending per bettor (left axis) and per adult (right axis) in states with legal mobile sports betting. Average spend per bettor is total online sportsbook deposits in a county-quarter divided by the number of unique households with at least one sportsbook transaction. Average spend per adult divides by the number of unique households with any transaction in the Earnest sample. Both series are unweighted averages across all counties in states where mobile betting is legal in the given quarter.
Source: Earnest Analytics.

Figure 5 shows time series of average per-capita spending and average spending per bettor in legal states by quarter. Both series display sharp increases following the onset of the pandemic. Average deposits by bettors were below $500 per quarter in December 2019, but exceeded $1,000 by June 2021. This pattern is consistent with large fixed costs of entry for sportsbooks, most notably the development of mobile platforms and substantial advertising expenditures, which many firms had not yet incurred prior to the pandemic.

Additionally, we exclude event study parameters corresponding to 2020:Q2 since most American sports were canceled during the early portion of the pandemic and betting activity dropped dramatically. This drop can be seen in Figure 4 and Figure 5. We view the 2020:Q2 parameters and the pre-pandemic adopting cohorts as representing a substantively different treatment environment that is less representative of the policy context faced by states today, and we therefore set the ω weights to zero for these parameters in our preferred specification. For completeness, we report results with no restrictions in Section Online Appendix A.4.

To motivate our choice of the threshold distance used to define spillover areas, we estimate $\hat{\tau}^D$

and $\hat{\tau}^S$ for changes in the log of total sportsbook transaction volume as we vary the distance cutoff used to classify counties as spillover-exposed, where distance is measured from the population-weighted centroid of a county to the border of the nearest state with legal mobile betting.[13] We vary this threshold from 10 to 100 miles. Figure 6 plots $\hat{\tau}^D$, $\hat{\tau}^S$ and 95% confidence intervals for each estimate as a function of the varying spillover threshold.

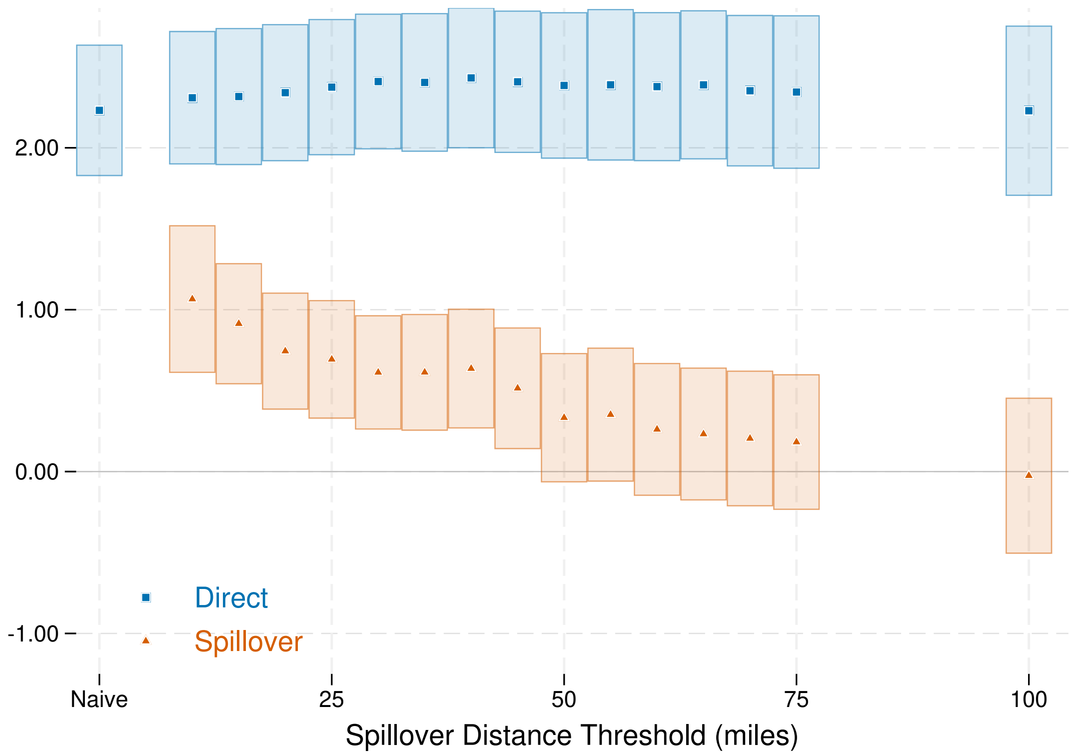
First, we note that the estimate for the direct effect increases slightly as we expand the definition of spillover counties, suggesting that spillover effects indeed attenuate the estimation of the direct effect on spending. The figure further shows that the estimate for spillovers, $\hat{\tau}^S$, declines sharply and nearly monotonically as the distance threshold increases. With spillover counties defined as counties with a population-weighted centroid 10 miles or closer to a legal state, spillovers are nearly 50% the magnitude of the direct effect, but the proportional effect is roughly 30% at 20 miles and falls to effectively zero around 60 miles. This spatial decay provides direct support for interpreting spillovers as arising from cross-border travel.

To estimate unattenuated Direct effects, we must assume a cutoff at which spillovers become negligible. Using Figure 6 as evidence, we choose that cut-off to be 60 miles and present the estimates of the direct effect, $\hat{\tau}^D$, in the next section using that threshold. For spillovers, we choose a threshold of 15 miles, before the effect significantly fades but after a significant sample of counties satisfies the threshold. We choose these two distances with policy relevance in mind, to estimate the direct effect of legalization clean of spillover contamination and to estimate the scope of spillovers within a reasonable distance from a legal state.

In Figure 7, we show the treatment status of counties in select years using the 15 mile and 60 mile distance thresholds, respectively. The orange counties in the left panel show the counties that are not themselves legal but whose population-weighted centroids are within 15 miles of a legal state. These places have relatively easy access to legalized sports betting and serve as our units for estimating the spillover effects. The orange counties in the right panel are not themselves legal but their population-weighted centroids are within 60 miles of a legal state. They are separated from the treated and control units to reduce the potential contamination of legalized sports betting in the estimation of the direct effect. The gray counties in the right panels then represent our "clean

[13] We discuss the construction of these population-weighted centroids in detail in Section Online Appendix A.1.

Figure 6: Difference-in-differences estimates for τ_D and τ_S on Log(Transaction Amount), varying thresholds of spillover definition

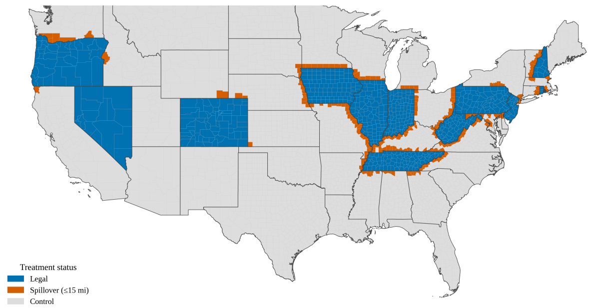


Notes: The figure above plots the estimates and 95% confidence intervals for $\hat{\tau}_D$ (in blue) and $\hat{\tau}_S$ (in orange) for various definitions of spillover counties as a function of distance from the nearest legal state. The outcome variable is the log of (one plus) the total county-level transaction amount from online sportsbooks in a county-quarter.
Source: Earnest Analytics

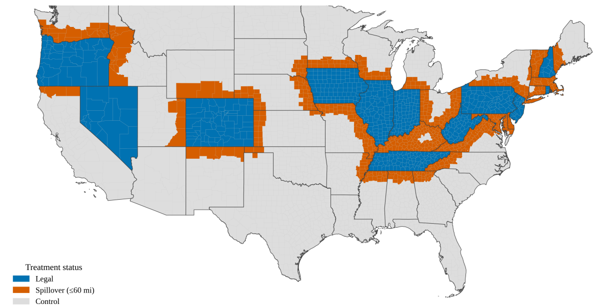
controls" for the direct effects, either never-treated or not-yet-treated counties. For the majority of the sample period, there are a large number of control counties in each Census region and contained in a variety of different states. However, by the end of 2024, all of the Northeast Census region had legalized mobile sports betting and the "never treat" counties largely come from a variety of states in the West, South, and Midwest regions.

Figure 7: U.S. counties by treatment status

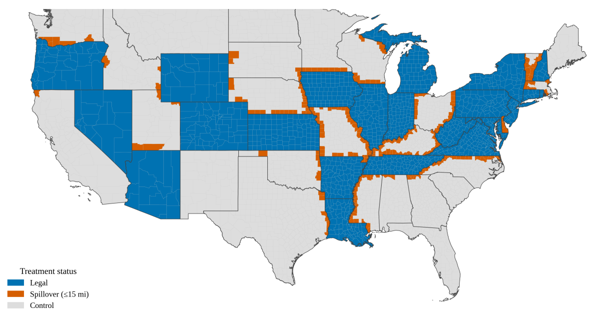
(a) Status as of 2020:Q4 (within 15 miles)



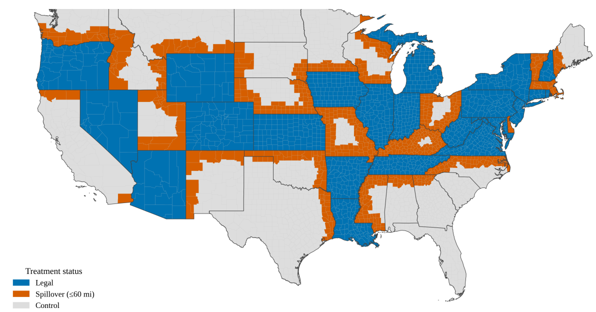
(b) Status as of 2020:Q4 (within 60 miles)



(c) Status as of 2022:Q4 (within 15 miles)



(d) Status as of 2022:Q4 (within 60 miles)



Notes: Since Nevada's gambling status pre-dated the 2018 *Murphy* decision, we do not consider counties near Nevada to be spillover counties. The panels on the left (A and C) show legal counties in green and counties whose population-weighted centroid is within 15 miles from a legal state in red. We use variation in spillover timing for this set to estimate τ^S. The panels on the right (B and D) replicate these panels except the red counties denote counties whose population-weighted centroid is within 60 miles of a legal state. We use this definition of spillover counties to isolate spillovers to estimate τ^D

5 Results

5.1 First stage impact of legalization on spending

In Figure 8 we present event study estimates for the impact of sports betting legalization relative to event time for four betting outcomes, separately for legal counties and spillover counties. We use a spillover threshold of 60 miles to estimate the direct effect clean of potential bias from spillovers, and we use a 15 mile threshold for estimating the spillover effects. For all four outcomes, we observe four distinct patterns. First, the periods prior to legalization show little evidence of a pre-trend. Panels A and B show that the five estimates prior to the event are jointly statistically indistinguishable from zero.[14] Panels B and C show small increases 5 quarters prior to

[14]Note, the pre-periods for the direct effect include only those direct-to-legal treatment types since spillover-to-legal pre-periods are contaminated and induce bias.

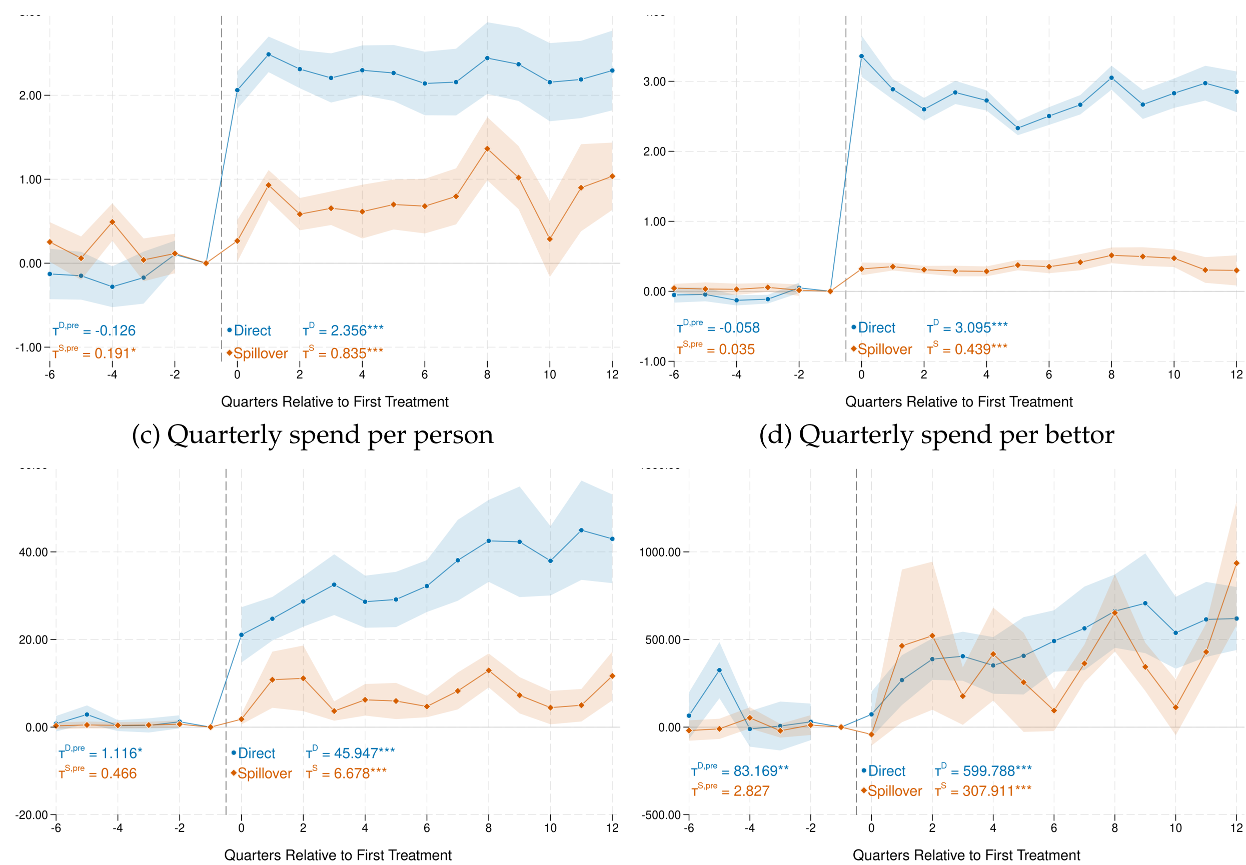
Figure 8: First-stage impact of sports betting legalization on betting outcomes

(a) Log(total transaction amount)

(b) Share with any sportsbook spend

(c) Quarterly spend per person

(d) Quarterly spend per bettor



Notes: Each panel above reports coefficient estimates (markers) and 95% confidence intervals (vertical bars) for a separate event study specification for the outcome variable denoted in the panel title. Blue circles report $\hat{\gamma}^{D}_{e^D}$ from Equation (4) where the spillover threshold distance is 60 miles. Orange triangles report $\hat{\gamma}^{S}_{e^S}$ from Equation (5) where the spillover threshold distance is 15 miles. Log(total transaction amount) is (one plus) the total amount of quarterly online sportsbook spend in a county-quarter. Share with any sportsbook spend is the share of unique members with any transactions in a county-quarter that had at least one online sportsbook transaction. Quarterly spend per person is the total amount of county-quarter spend divided by the number of unique members with any transactions in a county-quarter. Quarterly spend per bettor is the total quarterly spend divided by the number of unique members with any betting spend in a quarter. Specifications are described completely in Section 4 and variable constructions are described in Section 3.
Source: Earnest Analytics.

legalization but the other parameters are not statistically different from zero and none show any pre-trends in outcomes leading up to legalization. Second, there is an immediate and significant increase in each outcome variable in the first quarter of legalization. Third, the impact of legalization on average spending per bettor and per person increases steadily over time, continuing to grow as the time since legalization lengthens. Lastly, there are sizable spillover effects for each outcome in not-legal counties within 15 miles of a legal state.[15]

[15]Spillover effects measured at 10 miles are larger but noisier than the 15 mile baseline estimates.

Figure 8a presents event-study estimates for the log of total online sportsbook transaction volume. Spillover counties exhibit the largest relative effects in this outcome, with spending responses on the order of 36% of the direct legalization effect in log points. Figure 8b reports effects on the share of county residents with any online sportsbook spending in a given quarter. Direct legalization increases this share by about 3 percentage points in the first post-legalization quarter, followed by a brief decline and then a gradual rise. Spillover take-up is smaller, below 1 percentage point, but similarly persistent over time. This pattern is consistent with participation in spillover counties being concentrated among a relatively small but dedicated group of bettors willing to incur the travel cost.Figure 8c reports effects on average online sportsbook spending per resident in a county-quarter. This outcome exhibits the strongest growth over event time. In the first post-legalization quarter, average spending rises by roughly $20 from a near-zero baseline, and then increases almost linearly, reaching about $40 after three years with no clear evidence of saturation. In spillover counties, average spending per resident remains low, around $5 per quarter, reflecting the limited take-up documented in Figure 8b. Figure 8d conditions on participation and shows that, among those who do place bets online, spillover bettors have average initial quarterly spending levels at the same level (and sometimes larger) than those with direct legal access, albeit with noisier estimates.

Next, we present our difference-in-differences estimates from Equation (6) and Equation (7) for the main betting outcomes in Table 3. Odd columns report the direct effect estimates for the naive specification that ignores spillovers ($Q_i = \infty \, \forall i$) while even numbered columns report the preferred specification that separately estimates direct and spillover effects. The first two columns report results for the log of total transaction amount for a county-quarter. The naive specification yields a coefficient of 2.20, while the spillover-adjusted model in column (2) estimates a direct effect of 2.36, an increase of 7% over the naive specification, suggesting minor attenuation bias in the first stage. Since the dependent variable is in logs, this implies that legalization multiplies total sportsbook spending by roughly tenfold ($e^{2.36} \approx 10.6$). The estimated spillover effect corresponds to an increase of about 132% in total spending in counties exposed only through neighboring-state legalization, roughly 14% the size of the direct effect.

In columns (3) and (4), we report estimates for the share of the county with any online sports-

Table 3: Difference-in-differences estimates of the impact of legalized mobile sports betting on spending outcomes.

	Log(Betting Spend)		Share Bettors		Average Spend (All)		Average Spend (Bettors)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Direct Effect	2.20***	2.36***	2.92***	3.09***	44.86***	45.95***	612.14***	599.79***
	(0.17)	(0.21)	(0.13)	(0.11)	(4.87)	(3.56)	(87.06)	(70.93)
Spillover Effect		0.84***		0.44***		6.68***		307.91***
		(0.15)		(0.04)		(1.14)		(37.92)
Pre-treat mean	6.88	6.09	0.83	0.63	4.85	2.50	420.76	313.93
Model	Naive	Spillover	Naive	Spillover	Naive	Spillover	Naive	Spillover
Observations	52,390	52,390	52,390	52,390	52,390	52,390	52,390	52,390
# Counties	1,690	1,690	1,690	1,690	1,690	1,690	1,690	1,690
# Clusters	50	118	50	118	50	118	50	118

Notes: Observations are county-quarters. Spillover effects are estimated for counties within 15 miles of a legal state and Direct effects are estimated by considering spillovers to fade to zero after 60 miles from a legal state. All specifications include treatment-cohort and quarter fixed effects. The specification is described in its entirety in Section 4. Pre-treatment means are computed over distinct pre-treatment windows: in the spillover specification, the pre-treatment period ends at the onset of spillover exposure from a neighboring legal state, whereas in the naive specification it ends at the date of own-state legalization. Standard errors are clustered at the state-by-treatment-cohort level.
Source: Earnest Analytics.

book transactions in a quarter. The naive specification estimates a 2.92 percentage point increase while the estimate eliminating spillovers is 3.09, around 6% larger. The increase in counties within 15 miles of a legal state is 0.44 percentage points, or nearly 14% the magnitude of the direct effect. Columns (5) and (6) report the DD estimate for the average spend across all spenders in a county, which shows roughly similar estimates around $45-46 across the two specifications with nearly $7 per quarter in spillover counties. Lastly, we compute average sportsbook spend among those in the population with online sportsbook spend and report the estimates in columns (7) and (8). We find an increase of roughly $600 per quarter in the conditional average, with a spillover estimate of greater than $308 per quarter. These results provide additional evidence of the presence of significant spillover effects across state boundaries with residents in not-legal states crossing state boundaries to place mobile sports bets.

The results from this section help us better understand how mobile sports betting legalization translates to betting activity over time and space. First, we confirm that significant betting activity occurs by residents living across borders where sports betting is not legal. The effect on spending in counties with a population-weighted centroid within 15 miles of a legal state is 14% the size of the impact in counties that are legal within their boundaries. Second, we find that average betting spend increases steadily over time and that spending per person in legal areas may have not yet reached its saturation point even three years after legalization. Third, we find that legalization induces a relatively small share of the population to take up sports betting. The treatment effect is 3.1 percentage points off a pre-treatment base of roughly 0.6 percentage points. Consistent with Figure 4, this represents roughly 5% of the population with a sportsbook transaction in legal states in a given quarter.

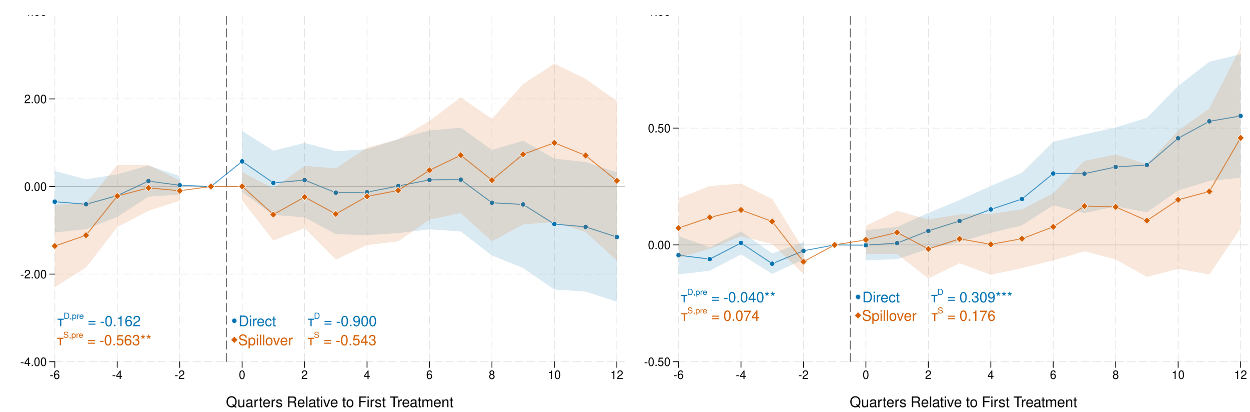
These findings help to guide our analysis in the next section on the reduced form impact of legalized sports betting. First, because betting intensity increases over time and credit delinquencies also emerge with a lag, we estimate our difference-in-differences effects, $\hat{\tau}^D$ and $\hat{\tau}_S$, excluding the first four post-legalization quarters to allow for credit outcomes to evolve alongside the buildup in betting activity. Second, given the demonstrated cross-border take-up in sportsbook spending, we also test for corresponding credit deterioration in spillover areas. As discussed in Section 3, our reduced form estimates using the CCP have an Intent to Treat (ITT) interpretation, capturing the average effect of legalization on the full population. Since a relatively small portion of the population take-up betting after legalization, we can expect relatively small population effects. In Section 5.3 we benchmark the likely scaling factor to translate our ITT estimates to a treatment effect for those who take up betting.

5.2 Reduced form impact of legalization on credit outcomes

We begin our analysis of the impact of legalized mobile sports betting on credit outcomes by looking at credit scores and overall delinquency rates. Credit scores are Equifax Risk Score 3.0 and our preferred delinquency rate is the share of individuals in a county-quarter with any balance 90 or more days past due on any debt product. Figure 9 presents the event study estimates for these two outcomes. Panel A shows the estimates for credit scores, which are relatively flat for both the direct and spillover effects until 2 years after legalization, after which they begin to decline. By three years after legalization, the median credit score for a legal county has declined by one point. Panel B reports estimates for the delinquency rate. Consistent with the prediction of delayed impacts to credit outcomes, delinquency begins ramping up around one year after legalization and continues to grow throughout with no signs of stabilization, reaching half a percentage point increase by three years after legalization. Delinquencies in spillover counties also increase over

Figure 9: Reduced form impact of sports betting legalization on credit outcomes

(a) Median Riskscore (b) Delinquency Rate



Notes: Each panel above reports coefficient estimates (markers) and 95% confidence intervals (vertical bars) for a separate event study specification for the outcome variable denoted in the panel title. Blue circles report $\hat{\gamma}^{D}_{e^D}$ from Equation (4) where the spillover threshold distance is 60 miles. Orange triangles report $\hat{\gamma}^{S}_{e^S}$ from Equation (5) where the spillover threshold distance is 15 miles. Credit Scores are Equifax Risk Score 3.0. Delinquency rates are the share of credit reports with any account 90 or more days past due. Specifications are described completely in Section 4 and variable constructions are described in Section 3.
Source: New York Fed Consumer Credit Panel/Equifax.

time and by three years after legalization, the increase in the delinquency rate is on par with legal counties.

Table 4 reports the $\hat{\tau}^D$ estimates from the naive specification that ignores spatial spillovers in columns (1) and (3) for median credit score and the overall delinquency rate, respectively, while columns (2) and (4) report $\hat{\tau}^D$ and $\hat{\tau}^S$ from the model that explicitly accounts for spatial spillovers. For the naive specification, we estimate a reduction of 1.08 points in the median credit score but the estimate is not statistically different from zero. After accounting for spatial spillovers, column (2) reports a 0.90 point reduction in the median credit score, and there is a 0.54 point reduction in areas that are not legal but within 15 miles of a legal state, however neither of these estimates is statistically different from zero. We explore the impact of legalized sports betting on the delinquency rate in columns (3) and (4), where column (3) presents the results from the naive specification and column (4) reports the results with spillovers. Here we find an increase in the prevalence of delinquencies of 0.30 percentage points in the naive specification and 0.31 percentage points after accounting for spillovers. Further, the spillover estimate is 0.18 percentage points, roughly 58% of the direct effect, but is not statistically different from zero.

Next, we explore heterogeneity in the impact of legalized sports betting on credit scores in

Table 4: Difference-in-differences estimates of the impact of legalized mobile sports gambling on average credit score and delinquency rate.

	Median Credit Score		Delinquency Rate	
	(1)	(2)	(3)	(4)
Direct Effect	–1.08	–0.90	0.30***	0.31***
	(0.68)	(0.76)	(0.10)	(0.11)
Spillover Effect		–0.54		0.18
		(0.82)		(0.14)
Pre-treat mean	732.21	732.03	10.76	10.71
Model	Naive	Spillover	Naive	Spillover
Observations	112,572	112,572	112,572	112,572
Geographies	3,127	3,127	3,127	3,127
# Clusters	50	129	50	129

Notes: Credit Scores are Equifax Risk Score 3.0. Observations are county-quarter cells. Spillover effects are estimated for counties within 15 miles of a legal state and Direct effects are estimated by considering spillovers to fade to zero after 60 miles from a legal state. All specifications include treatment-cohort and quarter fixed effects. The specification is described in its entirety in Section 4. Pre-treatment means are computed over distinct pre-treatment windows: in the spillover specification, the pre-treatment period ends at the onset of spillover exposure from a neighboring legal state, whereas in the naive specification it ends at the date of own-state legalization. Standard errors are clustered at the state-by-treatment-cohort level.
Source: New York Fed Consumer Credit Panel/Equifax.

Table 5, reporting estimates for median credit score by age. However, to increase precision in estimates, we use a ZIP-code aggregation of credit outcomes.[16] Using ZIP-codes as the unit of observation yields a similar point estimate for median credit scores for the full sample, but the estimate is now statistically different from zero, while the spillover estimate is roughly one-quarter the size but not precisely estimated. While the Under 40 group in column (2) reports an imprecise decline in credit scores, the 40 to 64 group in column (3) and the Over 65 group in column (4) show declines in median credit scores of 1.65 points and 1 point respectively. Spillover effects are negative but imprecise for 40 to 64 but large and statistically significant for the Over 65 group. This age pattern likely reflects the fact that credit scores are generally increasing across the age profile (the pre-treatment means are 677 for under 40, 730 for 40-64, and 789 for over 65) and the impact of adverse credit events on credit scores is larger for higher credit score borrowers.

Next, we explore the impact of legalization on delinquency by product type and age. Table 6 reports difference-in-differences estimates for auto loan, credit card, mortgage, and student loan delinquency rates, for the full sample (Panel A) and separately for three age groups: under 40

[16] While we can estimate outcomes at ZIP-code level with the CCP data, we prefer county-level data to match the geography for the first stage outcomes for use in the application in Section 6. Additionally, the sample size for ZIP-codes is roughly ten times that of counties which dramatically increases computational requirements.

Table 5: Difference-in-differences estimates of the impact of legalized mobile sports gambling on median credit score, by age group

	(1)	(2)	(3)	(4)
	All	Under 40	40 - 64	Over 65
Direct Effect	−1.07**	−0.46	−1.65***	−1.00***
	(0.54)	(1.23)	(0.53)	(0.34)
Spillover Effect	−0.22	0.49	−0.45	−1.20***
	(0.57)	(1.02)	(0.63)	(0.38)
Pre-treat mean	733.95	677.41	730.80	789.33
Model	Spillover	Spillover	Spillover	Spillover
Observations	1,001,628	729,360	832,572	811,944
Geographies	27,823	20,260	23,127	22,554
# Clusters	142	141	142	142

Notes: Credit Scores are Equifax Risk Score 3.0. Observations are zipcode-state-quarter cells. Spillover effects are estimated for ZIP-codes within 15 miles of a legal state and Direct effects are estimated by considering spillovers to fade to zero after 60 miles from a legal state. All specifications include treatment-cohort and quarter fixed effects. Standard errors are clustered at the state-by-treatment-cohort level.
Source: New York Fed Consumer Credit Panel/Equifax.

(Panel B), 40 to 64 (Panel C), and over 65 (Panel D). For the full sample, we find statistically significant increases in delinquency for auto loans and student loans with imprecise but positive impacts for the direct effect on credit cards and the spillover estimate for auto loans, credit cards, and student loans. Across all age groups, we find no evidence of changes to mortgage delinquency in the direct effect or the spillover effect.

We report results for student loans for completeness, but note that they may be confounded by major policy changes during our sample period. At the onset of the pandemic, student loan delinquencies fell dramatically when federal student loans were placed in forbearance, and the delinquency fell to below 1% during this time. Beginning in 2025:Q1, delinquent loans were again reported to credit reports and delinquency returned to pre-pandemic levels by 2025:Q2.[17] Taken together, the scope for delinquency transitions in our data is substantially constrained, so estimated increases on the order of one percentage point should be interpreted as economically large effects, but potentially confounded by the return to repayments.

For those under 40 (Panel B), we find significant increases in auto loan delinquencies (0.55 percentage points off a base of 9.80%, a 5.6% increase) and credit card delinquencies (1.02 percentage points off a base of 12.85%, a nearly 8% increase). Notably, the spillover effects for this age group

[17]Moreover, 62% of student loan borrowers are under age 40 (Mangrum et al., 2022), implying that borrowers over 40 are a relatively selected group, consisting primarily of individuals with older outstanding balances or Parent PLUS loans held by parents on behalf of their children.

Table 6: Difference-in-differences estimates of the impact of legalized mobile sports gambling on delinquency rates by product type and by age group

	(1)	(2)	(3)	(4)
	Auto	Credit Card	Mortgage	Student Loan
A. All				
Direct Effect	0.48**	0.26	–0.07	1.24*
	(0.25)	(0.27)	(0.08)	(0.68)
Spillover Effect	0.26	0.36	–0.09	0.65
	(0.23)	(0.25)	(0.09)	(0.71)
Pre-treat mean	7.49	9.02	1.21	12.59
Geographies	3,127	3,127	3,127	3,127
B. Under 40				
Direct Effect	0.55*	1.02***	0.02	1.30*
	(0.31)	(0.28)	(0.10)	(0.67)
Spillover Effect	0.78**	1.25***	0.10	1.11
	(0.33)	(0.35)	(0.14)	(0.70)
Pre-treat mean	9.80	12.85	1.05	12.50
Geographies	3,060	3,060	3,060	3,060
C. 40–64				
Direct Effect	0.51*	0.09	–0.04	1.27*
	(0.27)	(0.34)	(0.06)	(0.71)
Spillover Effect	0.19	0.43	–0.14	0.37
	(0.24)	(0.33)	(0.15)	(0.83)
Pre-treat mean	7.00	9.96	1.30	13.52
Geographies	3,093	3,093	3,093	3,093
D. 65+				
Direct Effect	0.06	–0.21	–0.02	0.04
	(0.31)	(0.21)	(0.13)	(0.69)
Spillover Effect	0.17	–0.04	0.03	0.44
	(0.28)	(0.18)	(0.15)	(0.93)
Pre-treat mean	4.87	4.76	1.22	7.87
Geographies	3,093	3,093	3,093	3,093
Model	Spillover	Spillover	Spillover	Spillover
Observations	112,572	112,572	112,572	112,572
Geographies	3,127	3,127	3,127	3,127
# Clusters	129	129	129	129

Notes: Each delinquency rate is computed as the share of the credit population in each county-quarter with any past due balance for a particular loan type divided by the population with any accounts of that type. Observations are county-quarter cells. Spillover effects are estimated for counties within 15 miles of a legal state and Direct effects are estimated by considering spillovers to fade to zero after 60 miles from a legal state. All specifications include treatment-cohort and quarter fixed effects. Standard errors are clustered at the state-by-treatment-cohort level.
Source: New York Fed Consumer Credit Panel/Equifax.

are larger than the direct effects for both auto loans (0.78 percentage points) and credit cards (1.25 percentage points). While this result seems counter-intuitive, legal states often devote tax revenues from sports betting toward mitigating the negative effects of sports betting. These include state-level phone lines for those struggling with addiction, supports groups, education, and advertising. If these resources are available in legal states but not in spillover counties, residents of the latter might be more prone to financial distress.

For those aged 40 to 64 (Panel C), the pattern shifts. Auto loan delinquencies still increase significantly (0.51 percentage points), but credit card delinquencies show no significant effect. The spillover effects for this age group are not significant for auto loans (0.19 percentage points) or credit cards (0.43 percentage points). In Panel D, no estimate is statistically different from zero suggesting that there is not a meaningful impact of legalized sports betting for those over 65. Overall, the results suggest that the delinquency effects of sports betting are concentrated among younger and middle-aged adults, primarily affecting auto loans and, for the youngest group, credit cards. This is consistent with surveys showing that sports betting take-up is concentrated among those under 50 (Siena College Research Institute and St. Bonaventure University, Jandoli School of Communication, 2024; National Council on Problem Gambling, 2021).

Lastly, we examine whether legalized sports betting affects more severe credit events: bankruptcies and foreclosures. Table 7 reports these results, however there does not seem to be consistent evidence that sports betting increases either bankruptcies or foreclosures. These null results suggest that while sports betting legalization affects delinquencies, it does not lead to a measurable increase in more serious adverse credit events, at least within the time horizon of our sample. Bankruptcies are relatively rare events that typically occur after extended periods of financial distress, so the null result may also reflect insufficient time for effects to materialize. Additionally, these results suggest that sports betting legalization does not lead to a meaningful increase in home loss, consistent with the broader pattern that effects are concentrated in unsecured debt products like auto loans and credit cards rather than secured debt like mortgages.

Taken together, the reduced form results paint a nuanced picture of how sports betting legalization affects consumer credit outcomes. Credit scores decline modestly but only several years after legalization.Delinquencies increase, but the effects are concentrated in auto loans (for those

Table 7: Difference-in-differences estimates of the impact of legalized mobile sports gambling on bankruptcy and foreclosure rates

	(1)	(2)
	Bankruptcy Rate	Foreclosure Rate
Direct Effect	–0.25	–0.02
	(0.18)	(0.02)
Spillover Effect	–0.24*	–0.00
	(0.13)	(0.02)
Pre-treat mean	2.99	0.35
Model	Spillover	Spillover
Observations	112,572	112,572
Geographies	3,127	3,127
# Clusters	129	129

Notes: The bankruptcy rate is the share of the credit population with any bankruptcy flag on their credit report. The foreclosure rate is the share of the credit population with any foreclosure flag on their credit report. Observations are county-quarter cells. Spillover effects are estimated for counties within 15 miles of a legal state and Direct effects are estimated by considering spillovers to fade to zero after 60 miles from a legal state. All specifications include treatment-cohort and quarter fixed effects. The specification is described in its entirety in Section 4. Standard errors are clustered at the state-by-treatment-cohort level.
Source: New York Fed Consumer Credit Panel/Equifax.

under 65) and credit cards (primarily for younger adults). More severe outcomes like bankruptcies and foreclosures show no clear pattern of increase, suggesting that while sports betting may push some consumers into financial distress, it does not (at least within our sample period) lead to widespread severe credit deterioration. The spillover effects provide additional evidence that cross-border betting has real financial consequences, particularly for younger adults living near legal states.

5.3 Implied Treatment Effects Among Induced Bettors

As discussed above, the estimated credit impacts are measured for the population as a whole, rather than for individuals who actually take up mobile sports betting. Accordingly, these estimates have an intent-to-treat interpretation. In this subsection, we use our first-stage estimates of betting take-up, benchmark them against external survey measures, and combine them with the reduced-form credit impacts to obtain rough estimates of treatment effects among those induced to bet.

We estimate that the legalization of mobile sports betting increases the share of the population making any deposit at a sportsbook within a quarter by 3.1 percentage points, from a near-zero baseline. A directly comparable quarterly statistic is not available in external data, but surveys from Pew Research Center (2025) and S&P Global Market Intelligence (2025) estimate that 10% and 15% of adults, respectively, placed a bet at an online sportsbook *within the past year.*

These surveys also indicate that betting activity is concentrated among young men. In S&P Global Market Intelligence (2025), 67% of bettors are age 44 or younger and 68% are men, while Siena College Research Institute and St. Bonaventure University, Jandoli School of Communication (2024) reports that 48% of men aged 18–49 have an account at an online sportsbook. Our reduced-form credit impacts measured in the overall population are therefore likely driven primarily by younger male borrowers. While we cannot directly split our estimates by gender, we find that the increases in delinquency on auto loans and credit cards are largest among individuals under age 40.

Dividing our reduced-form estimate of the overall delinquency impact (0.31) by the quarterly increase in betting take-up of 3.1 percentage points provides a rough estimate of the treatment effects among individuals induced to actively place bets by legalization. This back-of-the-envelope calculation implies an increase in overall delinquency of 10 percentage points for bettors of all ages. If we do the same for the reduced form impact for those under 40 (0.81) and for those 40 to 64 (0.23), we get average treatment effects for those induced of 26 percentage points for those under age 40, and 7.4 percentage points for those aged 40 to 64.

However, these estimates should be taken with caution due to the inherent assumptions required. First, the calculations assume proportional take-up across age groups. As a result, the implied effects are likely overstated for younger individuals, whose take-up is higher, and understated for those aged 40 to 64, whose take-up is lower. They also rely on an exclusion restriction, namely that the observed credit impacts operate only through betting take-up, and that individuals induced to bet are not on systematically different financial trajectories at the time of legalization. Nonetheless, the same calculation implies an increase of 10 percentage points in delinquency on any credit product among individuals induced to bet: an economically meaningful effect with potentially lasting consequences for borrowers' credit profiles.

6 Spatial Spillovers and the Political Economy of Sports Betting Legalization

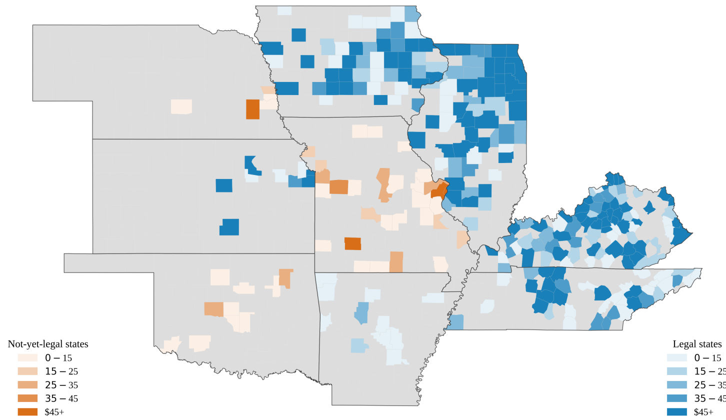
Our empirical results establish two facts that are directly relevant for states considering whether to legalize mobile sports betting. First, legalization increases sportsbook spending and worsens consumer credit outcomes within the legalizing state. Second, significant spillovers flow across state lines: residents of not-yet-legal states near legal states already bet and already experience adverse credit effects. Together, these facts imply that the net impact of legalization differs across states depending on how much spillover exposure they already face. In this section, we use our estimates to conduct a back-of-the-envelope cost-benefit simulation for each state that has not yet legalized mobile sports betting.

Missouri illustrates why spatial spillovers are central to the legalization calculus. As of 2025:Q1, Missouri had not legalized mobile sports betting despite being bordered by six states that had: Iowa, Illinois, Kentucky, Tennessee, Arkansas, and Kansas.[18] Critically, Missouri's two major population centers sit directly on legal state borders (Kansas City borders Kansas to the west and St. Louis borders Illinois to the east), giving it a population-weighted average distance to legal sports betting of just 20.4 miles, the second lowest in our data. Figure 10 maps average sportsbook spending per person by county for Missouri and its neighboring states, illustrating the elevated spending in Missouri counties nearest to Kansas and Illinois. The consequence is that Missouri already bears significant social costs of betting, as its residents actively cross state lines to place bets, while the tax revenue from that activity accrues to Kansas, Illinois, and other neighboring states. Missouri thus faces an acute version of the fiscal asymmetry that motivates this section's analysis: the state absorbs costs from betting spillovers but captures none of the associated revenue.

Our exercise simulates how sportsbook spending, tax revenue, and consumer credit delinquencies would change in each not-yet-legal state if it were to legalize, and combines these to create an illustrative cost-benefit metric. The exercise proceeds in three steps: (i) simulated tax revenue gains for newly legalizing states, (ii) simulated increases in consumer delinquencies for newly legalized states, and (iii) simulated tax revenue losses for currently legal states that lose

[18] Missouri's remaining two neighbors, Oklahoma and Nebraska, had also not legalized as of 2025:Q1.

Figure 10: Average sportsbook spending per person by county: Missouri and neighboring states



Notes: The map shows the average quarterly spend at online sportsbooks per household in the Earnest Analytics data by county in 2025:Q1. States with legalized sports betting are colored in green with darker colors representing higher spend. States that had not legalized sports betting by 2025:Q1 are colored in red with darker colors representing higher spend. Counties in gray are not in the final analysis sample as described in Section 3.
Source: Earnest Analytics.

cross-border bettors. We provide the full methodological details of the calculations in Section Online Appendix B and focus on the intuition and key assumptions in this section.

6.1 Simulated tax revenue gains for newly legalizing states

First, we exploit the spatial structure of our first-stage estimates to estimate how spending would evolve in each not-yet-legal state if they were to legalize. Equation (8) formalizes this calculation:

$$\hat{T}_s = r \cdot \phi \cdot \sum_{c \in s} \bar{y}_c \cdot N_c \cdot e^{\Delta_c}. \tag{8}$$

First, we compute the total county level spend before legalization by computing $\bar{y}_c$,the average spend per unique member in the spending data, which we multiply by N_c, the county level population from the ACS. Next, we compute the estimated incremental spend for each county c as a function of the distance from a legal state and the corresponding $\hat{\tau}$ estimates. For counties within 60 miles of a legal state, the change in spending we apply is equal to the estimated difference in the direct effect (measured with a spillover threshold of 60 miles) and spillover effect for that distance, $\Delta_c = \hat{\tau}^D(\bar{d}) - \hat{\tau}^S(d_c)$. Here, $\bar{d}$ denotes the maximum spillover distance (60 miles in our specification), and d_c is county c's distance to the nearest legal state. For counties further than 60 miles away from a legal state, we apply the full direct effect, so that $\Delta_c = \hat{\tau}^D(\bar{d})$. We then sum the total spending over all counties in a state before annualizing quarter spend with ϕ. Lastly, we apply the tax rate. For the baseline simulation, we apply a rate of 10%, but we note that the tax revenue scales linearly with the tax rate.[19]

This calculation requires two assumptions. First, the "top-up" construction of Δ_c assumes that the individuals induced to bet through cross-border access are a subset of those who would bet under direct legalization, so that own-state legalization brings a county's betting activity up to the level observed in directly treated counties. Next, we assume the Earnest per-person average quarterly gambling is representative of the county average. Under these assumptions, we arrive at state level estimates of tax revenue from sports betting legalization as a function of the chosen tax rate r.

6.2 Simulated increases in consumer delinquencies for newly legalized states

Next, we simulate the state level increases in credit card and auto delinquencies as a result of legalization. Equation (9) models the simulation for additional delinquencies of credit product type y in state s:

$$\Delta \mathrm{DQ}_{y,s} = \sum_{c \in s} \sum_{a} \delta_{y,a,c} \cdot N^{\text{acct}}_{y,a,c}. \tag{9}$$

We use a similar incremental "top-up" construction relying on our spillover gradient to compute $\delta_{y,a,c}$, which is the percent change in delinquencies for credit product type y and age group a in county c. We construct this term similarly to Δ_c. For counties further than 60 miles of a legal state, $\delta_{y,a,c} = \hat{\tau}^D_{y,a}(\bar{d})$, where $\bar{d} = 60$. For counties within 60 miles of a legal state,

$$\delta_{y,a,c} = \max\left[\hat{\tau}^D_{y,a}(\bar{d}) - \hat{\tau}^S_{y,a}(d_c), 0\right],$$

such that the increase in delinquencies is equal to the difference in the direct effectand the spillover estimate at d_c. Notably, we construct $\delta_{y,a,c}$ such that legalization *cannot decrease delin-*

[19] Actual state rates range from roughly 7% (Iowa) to over 50% (New York, Oregon).

quencies. This assumption is actually binding for some y and a given the result in Section 5 that the spillover estimate for those under 40 was larger than the direct effect. Given that access to gambling resources can reduce delinquencies, it may be plausible that legalization could reduce delinquencies for those already gambling in spillover states. However, for the simulation, we prevent legalization from reducing delinquencies to not overstate the potential benefits of sports betting legalization. Lastly, we multiply the percent increase in delinquencies $\delta_{y,a,c}$ by the population of borrowers at age a with a y account in county c ($N^{acct}_{y,a,c}$). We then sum these totals across all age groups and across all counties in state s.

Table 8 reports the results for the 20 states that had not legalized mobile sports betting as of 2025:Q1, sorted by population-weighted distance to the nearest legal state. We combine the two components into a benefit-cost ratio: the added annual tax revenue per additional delinquency. This metric provides a rough benchmark for comparing the fiscal benefits of legalization against one dimension of its social costs. A higher value indicates more revenue captured per adverse credit event. We emphasize that this is not a comprehensive welfare calculation; it omits consumer surplus from betting, internalities borne by problem bettors, broader social costs of financial distress, and administrative costs. Nevertheless, it provides an informative comparison across states that highlights how spatial spillovers shape the cost-benefit calculus.

We again caution that the simulation relies on several maintained assumptions: the treatment effects from earlier-legalizing states generalize to not-yet-legal states; the exercise is partial equilibrium, holding other states' legal status fixed; the composition of credit account holders is unaffected by legalization; per-person sportsbook expenditures in the Earnest sample are representative of the broader adult population; and all legalizing states apply a uniform 10% tax rate.

Table 8: Simulated Changes in Sports Betting Spending, Tax Revenue, and Delinquencies for State Sports Betting Legalization for Not-yet-legal States

		Betting Spend Changes				Delinquency Changes		
State	Avg Dist to Legal (miles)	Annual spend pre (millions $)	New spend (millions $)	Annual spend post (millions $)	Added tax revenue (millions $)	Added Auto DQ	Added CC DQ	Tax Revenue per added DQ ($)
NE	18.9	$80.8	+$372.5 (4.6x)	$453.3	+$45.3	750 (+2.9%)	957 (+1.0%)	$26,538
MO	20.4	$347.8	+$1,528.5 (4.4x)	$1,876.3	+$187.6	2,193 (+1.5%)	2,191 (+0.5%)	$42,792
SD	23.6	$12.6	+$76.0 (6.0x)	$88.6	+$8.9	482 (+4.2%)	642 (+1.5%)	$7,918
MS	38.1	$47.1	+$266.4 (5.7x)	$313.5	+$31.4	1,719 (+1.6%)	1,613 (+0.7%)	$9,424
WI	49.2	$151.5	+$916.9 (6.1x)	$1,068.4	+$106.8	4,045 (+5.2%)	5,928 (+2.5%)	$10,709
UT	51.7	$33.2	+$223.8 (6.7x)	$257.1	+$25.7	2,960 (+7.2%)	5,260 (+3.6%)	$3,127
SC	53.0	$233.1	+$1,516.4 (6.5x)	$1,749.5	+$175.0	3,877 (+2.3%)	5,141 (+1.1%)	$19,406
ID	59.4	$7.3	+$41.6 (5.7x)	$48.9	+$4.9	1,585 (+5.2%)	1,980 (+2.1%)	$1,374
AL	68.1	$95.2	+$665.6 (7.0x)	$760.8	+$76.1	4,120 (+2.4%)	5,300 (+1.3%)	$8,079
GA	78.8	$277.1	+$2,467.0 (8.9x)	$2,744.1	+$274.4	10,927 (+3.0%)	20,990 (+2.0%)	$8,597
WA	90.7	$78.4	+$676.2 (8.6x)	$754.6	+$75.5	6,717 (+6.8%)	15,806 (+3.9%)	$3,352
OK	92.9	$87.8	+$810.4 (9.2x)	$898.2	+$89.8	4,062 (+3.7%)	6,146 (+2.2%)	$8,797
MN	102.3	$164.3	+$1,542.5 (9.4x)	$1,706.7	+$170.7	5,516 (+8.7%)	11,943 (+4.6%)	$9,777
MT	114.7	$7.8	+$67.0 (8.6x)	$74.8	+$7.5	938 (+6.5%)	1,817 (+3.5%)	$2,722
NM	129.4	$26.0	+$237.4 (9.1x)	$263.4	+$26.3	2,120 (+3.7%)	3,539 (+2.7%)	$4,647
TX	170.6	$1,026.9	+$9,742.3 (9.5x)	$10,769.2	+$1,076.9	36,971 (+3.9%)	66,536 (+2.6%)	$10,404
CA	222.7	$1,408.9	+$13,445.8 (9.5x)	$14,854.7	+$1,485.5	40,227 (+6.1%)	96,433 (+3.6%)	$10,870
ND	234.0	$7.9	+$75.0 (9.5x)	$82.9	+$8.3	981 (+10.3%)	1,710 (+4.6%)	$3,084
HI	2,401.7	$13.1	+$125.1 (9.5x)	$138.2	+$13.8	1,171 (+8.0%)	2,972 (+4.0%)	$3,331
AK	2,714.1	$3.0	+$28.7 (9.5x)	$31.7	+$3.2	702 (+9.2%)	1,530 (+4.0%)	$1,434

Notes: The table lists the states who had not legalized sports betting by 2025:Q1. The second column computes the population-weighted distance to the nearest legal state in 2025:Q1. The third column computes the simulated annual spend within the state by scaling the county-level average online betting spend per person in the Earnest data in 2025:Q1 to the full county population. The fourth column computes simulated new spend after legalization by applying the estimated treatment effect on the log of total county spending to scale up pre-legalization spending. The fifth column combines the third and fourth columns and the sixth column estimates tax revenue assuming a 10% tax rate. The seventh and eighth columns estimate simulated additional delinquencies after legalization by applying the age specific direct and spillover treatment effects to zipcode-by-age levels of delinquency in 2025:Q1. The last column relates the sixth column to the sum of the seventh and eighth. A more comprehensive discussion of methods is in Section Online Appendix B.
Source: Earnest Analytics, New York Fed Consumer Credit Panel/Equifax, American Community Survey, author's calculations.

Returning to the case of Missouri, the simulation results confirm the fiscal asymmetry suggested by its geography. Missouri's annualized pre-legalization sportsbook spending is already $348 million, largely attributable to cross-border spillovers into Kansas and Illinois among other neighboring legal states, yet the state captures none of the associated tax revenue. If Missouri were to legalize, we estimate that total annual spending would rise from $348 million to roughly $1.88 billion, generating an estimated $188 million in new tax revenue at a 10% tax rate. For reference, New York bettors spent $26 billion in wagers in 2025 (Yogonet, 2026). At the same time, the simulated increase in delinquencies is modest in percentage terms: roughly 2,200 additional auto loan delinquencies (a 1.5% increase) and credit card delinquencies (a 0.5% increase) each. These relatively small marginal increases reflect the fact that some of the betting-related financial distress is already occurring through cross-border access. The resulting tax revenue per additional delinquency is roughly $43,000, the highest of any not-yet-legal state by a large margin (the next largest are Nebraska at $26,000 and South Carolina at $19,000). Perhaps due to these factors, Missouri voters narrowly approved Amendment 2 in November 2024 to legalize sports betting, with just 50.01% of the vote, a margin of roughly 2,961 votes out of 2,954,343 cast (Ballotpedia, 2024).

6.3 Simulated tax revenue losses for currently legal states that lose cross-border bettors

Legalization also diverts tax revenue away from neighboring legal states that currently host cross-border betting. The simulated lost tax revenue for legal state ℓ with state-specific effective tax rate r_ℓ is:

$$L_\ell = r_\ell \cdot \sum_{\substack{c:\ell(c)=\ell, \\ d_c \le \bar{d}}} \bar{y}_c \cdot N_c. \tag{10}$$

For each county c in a not-yet-legal state with $d_c \le \bar{d}$, we attribute its current pre-legalization spending $\bar{y}_c \cdot N_c$ (as defined above) to the nearest legal state $\ell(c)$. We then aggregate across all spillover counties to obtain the total spending currently attributed to each legal state ℓ. We apply each state's approximate tax rate on gross gaming revenue (Hoffer and Macumber-Rosin, 2025). Table 9 reports the simulated revenue losses for each affected legal state. We see that the states benefiting most from spillover tax revenue are Illinois (high tax rate combined with spillover spend

Table 9: Simulated Loss in State Tax Revenues from Neighboring State Sports Betting Legalizations

State	Change in Spillover Spend (millions $)	Previous spillover states	State Betting Tax Rate	Simulated lost state tax revenue (millions $)
Illinois	-$277.8	WI, MO	30%	-$83.4
North Carolina	-$167.6	SC, GA	18%	-$30.2
Tennessee	-$66.1	MS, AL, GA	20%	-$13.0
Kansas	-$101.3	MO, NE, TX, OK	10%	-$10.1
Oregon	-$16.2	ID, CA, WA	51%	-$8.3
Iowa	-$88.2	NE, WI, SD, MO, MN	7%	-$6.0
Louisiana	-$24.4	TX, MS	22%	-$5.2
Arkansas	-$32.7	TX, MO	15%	-$4.9
Florida	-$32.7	MS, GA, AL	10%	-$3.3
Wyoming	-$24.8	MT, UT, SD, ID	10%	-$2.5
Arizona	-$6.3	UT, NM	20%	-$1.3
Michigan	-$8.0	WI	8%	-$0.7
Colorado	-$1.0	NM, UT	10%	-$0.1

Notes: We compute lost tax revenue by assuming all online sportsbook transactions taking place in not-legal states are attributed to the nearest legal state within 60 miles. We then sum over all nearest legal states to arrive at the change in spillover spend if all previous spillover states were to legalize. State Sports Betting Tax Rates are average effective rates from The Tax Foundation.
Source: Earnest Analytics, New York Fed Consumer Credit Panel/Equifax, American Community Survey, author's calculations.

from Wisconsin and Missouri) and North Carolina (moderately high tax rate and spillover spend from Georgia and South Carolina).

7 Discussion

We study the impact of legalized mobile sports betting on betting activity and consumer credit outcomes, with an emphasis on spatial spillovers across state lines. Exploiting the staggered roll-out of state-level legalization following the 2018 *Murphy v. NCAA* decision, we use an extended two-way fixed effects (ETWFE) framework that separately estimates direct treatment effects and cross-border spillover effects. Our first-stage estimates establish that legalization dramatically increases betting activity: average quarterly spending per person rises by roughly $46 from a pre-treatment mean of $2.50, and the share of the population with any sportsbook spending in a quarter increases by 3.1 percentage points. The effects on average spending grow continually over time with no clear evidence of saturation, suggesting the market for mobile sports betting continues to mature years after legalization. At the same time, substantial betting activity occurs in counties where sports betting is not legal but which lie near a legal state, with spillover effects on total spending roughly 14% of the direct effect for counties within 15 miles of a legal state, declining monotonically with distance and approaching zero by 60 miles. These spillovers have real consequences for consumer financial health. Three years after legalization, median credit scores are one point lower and overall delinquency rates increase by 0.31 percentage points following legalization. Since only about 3.1% of the population takes up betting after legalization, these intent-to-treat estimates would imply that those who are induced to bet due to legalization experience delinquency increases of 10 percentage points, effects that are economically meaningful. We find no evidence that legalization increases bankruptcies or foreclosures within our sample period, suggesting that financial harm manifests as early-stage distress in unsecured credit products rather than as severe default events, though our sample period may be too short to fully capture such long-run consequences.

A central contribution of this paper is demonstrating the importance of accounting for spatial spillovers in staggered adoption designs. The standard difference-in-differences framework identifies treatment effects by comparing treated units to not-yet-treated or never-treated controls. When treatment in one jurisdiction spills across borders into control jurisdictions, this comparison is contaminated: the control group is partially treated, and the estimated effect is attenuated toward zero. We show that this attenuation is empirically meaningful: ignoring spatial spillovers underestimates the direct effect of legalization on betting take-up by 6% and understates the extent of the population that is affected. Our approach, building on the ETWFE framework of Wooldridge (2025) and the spillover extension of Fiorini et al. (2024), treats exposure as a two-dimensional treatment vector that separately captures own-state legalization and neighboring-state legalization, offering a practical template for other settings in which state-level policies generate cross-border externalities. The key identifying assumption, that spillover effects become null once a unit's own jurisdiction adopts, is plausible when the policy eliminates the need for the behavior generating the spillover, as mobile sports betting does, though it would be less tenable for policies where neighboring-state legalization continues to affect behavior even after own-state adoption. More broadly, our findings serve as a cautionary note for the growing literature exploiting staggered state-level policy adoption. Many state policies, including marijuana legalization, minimum wage laws, paid leave mandates, and firearm regulations, are likely to generate spatial spillovers, and naive specifications that ignore them may systematically understate the effects of these policies.

These methodological concerns have direct consequences for the policy debate over sports betting. As of this writing, 12 states have not yet legalized mobile sports betting, and our results provide evidence that should inform their decisions. Legalization produces measurable harm to consumer financial health: while the population-average effects are modest, they reflect the dilution of concentrated effects among a small share of the population that takes up betting, and the implied treatment effects on induced bettors are substantial. At the same time, not legalizing does not insulate a state from these consequences. Our first-stage estimates show that residents of not-yet-legal states near legal states already bet at significant rates, and our reduced-form estimates confirm that spillover areas experience adverse credit effects. This creates a fiscal asymmetry: a not-yet-legal state bears some of the social costs of betting through spillovers but captures none of the tax revenue. Our political economy simulation quantifies this asymmetry, showing that states closer to legal neighbors, such as Missouri, Nebraska, and South Carolina, already face substantial spillover exposure, so that legalization would generate relatively more tax revenue per additional delinquency because much of the adverse impact is already occurring. Conversely, geographically isolated states like California bear almost none of the spillover costs under the status quo, so legalization would represent a larger marginal increase in harm. Further, when a not-yet-legal state does legalize, the neighboring legal states that currently host cross-border betting lose tax revenue, a transfer that is not trivial: Illinois, for example, stands to lose an estimated \$83 million annually if Missouri and Wisconsin legalize, owing to its high tax rate and proximity to large populations in those states.

This interdependence means that each state's optimal policy depends not only on its own characteristics but on the decisions of its neighbors, a dynamic that extends well beyond sports betting to a general class of problems in which states independently regulate goods or activities that generate cross-border externalities. The literature on cross-border sales of cigarettes (Lovenheim, 2008; Merriman, 2010; Harding et al., 2012; DeCicca et al., 2013), recreational marijuana (Hansen et al., 2020), and alcohol (Lovenheim and Slemrod, 2010) has documented that differences in state-level regulation create incentives for cross-border consumption, and our paper contributes by showing that these spillovers are not merely a nuisance for estimation but may be a first-order feature of the policy environment. The structure of this problem creates a familiar fiscal externality: a state that legalizes sports betting captures tax revenue while exporting some social costs to neighbors whose residents bet but whose public services absorb the resulting financial distress, while a state that does not legalize forgoes revenue but still bears costs from spillover betting. This asymmetry pushes toward universal adoption even if the social costs of betting exceed the tax revenue in each state individually, because the marginal state compares legalizing (capturing revenue while bearing costs) against not legalizing and bearing costs without revenue. The resulting equilibrium may involve more widespread legalization than a social planner would choose. The mobile nature of sports betting amplifies this dynamic relative to activities that require physical presence at a fixed location, because a bettor need only cross the state line with a smartphone rather than travel to a specific establishment. As digital markets expand to other regulated activities, the spatial spillover channel we document is likely to become increasingly relevant for state-level policy design.

Several limitations suggest directions for future research. Our reduced-form estimates capture the intent-to-treat effect of legalization on the full population, and we cannot observe which individuals in the credit report data actually bet; linked administrative data combining betting and credit records would permit sharper estimates. Our sample period may also be too short to capture long-run effects, particularly for severe events like bankruptcy that emerge only after extended financial distress. The political economy simulation is partial equilibrium, holding all other states' legal status fixed without modeling strategic interactions or the potential for federal regulation. Finally, our analysis focuses on one dimension of social cost, consumer credit deterioration, and does not capture other channels through which betting may affect welfare, including mental health (Shaygan et al., 2024; Bersak et al., 2024), family stability (Wilken, 2025), labor market outcomes, and crime (for instance Grinols and Mustard (2006) documents increases in crime after casinos open in an area and Matsuzawa and Arnesen (2024) finds increases in domestic violence after sports' team losses in states where sports gambling is legal.). A complete welfare assessment would require integrating evidence across these domains alongside estimates of the consumer surplus that bettors derive from participation. Despite these limitations, our findings establish that spatial spillovers are quantitatively important for understanding the effects of sports betting legalization and that ignoring them leads to meaningfully attenuated estimates. As states continue to debate legalization, the cross-border externalities we document should be central to the policy discussion, not only for sports betting but for the broader class of state-level regulatory decisions in which one state's policy generates consequences that do not respect jurisdictional boundaries.

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Online Appendix A Appendix

Online Appendix A.1 Construction of Distance to Legal Sports Betting

Online Appendix A.1.1 Overview

We construct a time-varying measure of geographic proximity to legalized sports betting for use in our empirical analysis. This measure captures the distance from each observation unit (counties for the first stage and counties or ZIP-state pairs for credit outcomes) to the nearest state boundary where sports betting is legal in a given quarter. The construction involves three steps: computing population-representative geographic centroids, calculating distances to all state boundaries, and interacting these distances with the timing of state-level legalization.

Online Appendix A.1.2 Population-Weighted County Centroids

For the county-level analysis, we require a point location representing each county. Rather than using geometric centroids, which may fall in unpopulated areas (e.g., mountains, lakes, or industrial zones), we compute population-weighted centroids that more closely reflect where residents actually reside within each county.

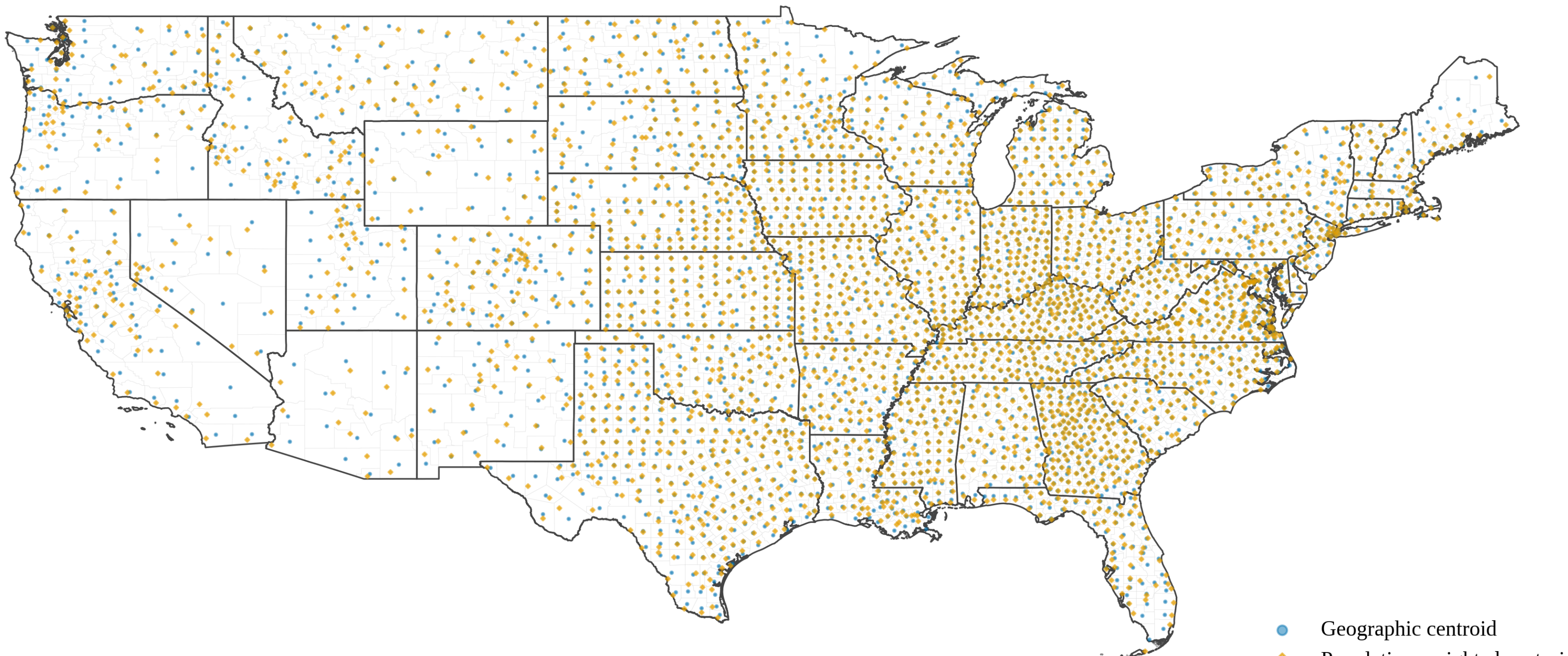
We obtain 2020 Decennial Census population counts at the census block level via the Census Bureau API. Because census block boundaries change between decennial censuses, we use the Census Bureau's Block Relationship Files to crosswalk 2020 population counts to 2010 block geography. When a 2020 block spans multiple 2010 blocks, we allocate population proportionally based on the land area of each intersection.

For each county c, we calculate the population-weighted centroid as:

$$\bar{x}_c = \frac{\sum_{b \in c} p_b \cdot x_b}{\sum_{b \in c} p_b}, \qquad \bar{y}_c = \frac{\sum_{b \in c} p_b \cdot y_b}{\sum_{b \in c} p_b}, \tag{A.1}$$

where (x_b, y_b) denotes the geographic centroid of census block b and p_b is the 2020 population of that block. This yields a single latitude–longitude coordinate pair for each of the approximately 3,100 counties in the United States. Figure A1 shows a comparison of the geographic centroid and the population-weighted centroid for each county.

Figure A1: Comparison of Geographic and Population-weighted centroids



Online Appendix A.2 Population-Weighted ZIP-State Centroids

For the ZIP code-level credit outcomes analysis, we compute population-weighted centroids for each ZIP-state pair using the same census block population data described above. Because census blocks are not nested within ZIP Code Tabulation Areas (ZCTAs), we spatially join each block's geographic centroid to the 2010 ZCTA polygon it falls within. We then apply Equation (A.1) at the ZCTA level, substituting ZCTA for county, to obtain population-weighted centroids.

Because some ZCTAs span state boundaries and our credit outcomes data are reported at the ZIP-state level, we process blocks state by state: each state's blocks contribute only to ZCTAs within that state, naturally splitting cross-border ZCTAs into distinct ZIP-state observation units. The small number of blocks whose centroids fall outside any ZCTA boundary are dropped, introducing a negligible rounding error relative to true ZCTA population totals.

Online Appendix A.2.1 Distance to State Boundaries

We calculate the distance from each county or ZIP-state centroid to the boundary of every U.S. state. Distances are measured to state boundary lines rather than state centroids, as boundary distance more accurately captures proximity to legal sports betting markets, since a resident near a state border can access betting in a neighboring state regardless of that state's geographic center.

All distance calculations are performed after projecting geographic coordinates to the NAD83 Conus Albers equal-area projection (EPSG:5070), which minimizes distance distortion across the contiguous United States. For each centroid-state pair, we compute the Euclidean distance in meters to the nearest point on the state's boundary and record the coordinates of that nearest boundary point.

We exclude each unit's own state from consideration, as we are interested in distance to other states where betting may be legal. This yields a matrix of distances from each unit centroid to each of the remaining 49 states (plus the District of Columbia).

Online Appendix A.2.2 Time-Varying Distance to Nearest Legal State

The final distance measure varies over time as states progressively legalize sports betting. Let $\mathcal{L}_t$ denote the set of states where sports betting is legal as of quarter t. For each geographic unit i (county or ZIP-state pair) in quarter t, we define the distance to legal sports betting as:

$$D_{it} = \min_{s \in \mathcal{L}_t} d_{is} \tag{A.1}$$

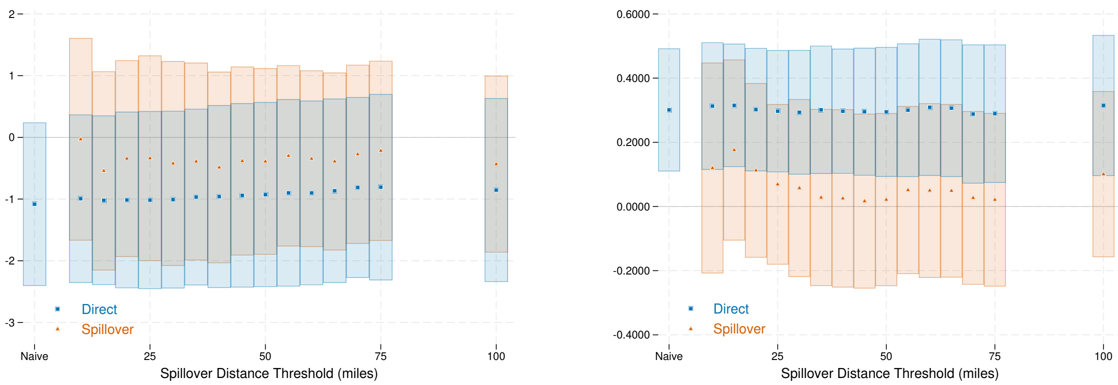
where d_{is} is the pre-computed distance from unit i to the boundary of state s. When the set of legal states is empty (prior to any legalization), this distance is undefined so we top-code to 9,999 miles. This measure decreases discontinuously when a nearby state legalizes sports betting and remains constant otherwise. The identifying variation in our empirical strategy comes from differential exposure to legalization based on geographic proximity: units closer to newly legalizing states experience larger decreases in distance to legal sports betting than units farther away.

Online Appendix A.3 Additional Tables and Figures

Figure A2: Difference-in-differences estimates for $\hat{\tau}^D$ and $\hat{\tau}^S$ for main reduced form outcomes, varying thresholds of spillover definition

(a) Median Credit Score

(b) Delinquency Rate



Notes: The figure above plots the estimates and 95% confidence intervals for $\hat{\tau}_D$ (in blue) and $\hat{\tau}_S$ (in orange) for various definitions of spillover counties as a function of distance from the nearest legal state. Panel A reports estimates for the median Equifax Risk Score 3.0 and Panel B reports estimates for the overall delinquency rate (share of individuals with any account 90 or more days past due on any debt product).
Source: New York Fed Consumer Credit Panel/Equifax.

Table A1: Difference-in-differences estimates of the impact of legalized mobile sports gambling on bankruptcy rate, by age group

	(1)	(2)	(3)	(4)
	All	Under 40	40 - 64	Over 65
Direct Effect	–0.25	–0.29**	–0.30	–0.07
	(0.18)	(0.14)	(0.28)	(0.09)
Spillover Effect	–0.24*	–0.24*	–0.37*	–0.08
	(0.13)	(0.12)	(0.22)	(0.07)
Pre-treat mean	2.99	2.16	4.71	1.69
Model	Spillover	Spillover	Spillover	Spillover
Observations	112,572	110,160	111,348	111,348
Geographies	3,127	3,060	3,093	3,093
# Clusters	129	129	129	129

Notes: The bankruptcy rate is the share of the credit population with any bankruptcy flag on their credit report. Observations are county-quarter cells. Spillover effects are estimated for counties within 15 miles of a legal state and Direct effects are estimated by considering spillovers to fade to zero after 60 miles from a legal state. All specifications include treatment-cohort and quarter fixed effects. Standard errors are clustered at the state-by-treatment-cohort level.
Source: New York Fed Consumer Credit Panel/Equifax.

Table A2: Difference-in-differences estimates of the impact of legalized mobile sports gambling on foreclosure rate, by age group

	(1)	(2)	(3)	(4)
	All	Under 40	40 - 64	Over 65
Direct Effect	–0.02	–0.03	–0.02	–0.00
	(0.02)	(0.02)	(0.03)	(0.01)
Spillover Effect	–0.00	0.01	–0.03	0.01
	(0.02)	(0.02)	(0.03)	(0.02)
Pre-treat mean	0.35	0.29	0.51	0.22
Model	Spillover	Spillover	Spillover	Spillover
Observations	112,572	110,160	111,348	111,348
Geographies	3,127	3,060	3,093	3,093
# Clusters	129	129	129	129

Notes: The foreclosure rate is the share of the credit population with any foreclosure flag on their credit report. Observations are county-quarter cells. Spillover effects are estimated for counties within 15 miles of a legal state and Direct effects are estimated by considering spillovers to fade to zero after 60 miles from a legal state. All specifications include treatment-cohort and quarter fixed effects. Standard errors are clustered at the state-by-treatment-cohort level.
Source: New York Fed Consumer Credit Panel/Equifax.

Online Appendix A.4 Alternate ETWFE Aggregation

To assess the sensitivity of our main results to specification choices, Figures A3 to A5 report difference-in-differences point estimates and 95% confidence intervals for each of our main outcomes where we iteratively drop a baseline specification change and we also report each model with population weights. Overall, the estimates are highly stable across specifications for both the first-stage spending outcomes and the reduced-form credit outcomes, supporting the robustness of the results reported above. However, there are a few general trends we note. First, the first stage results for Log(total transaction amount) are smaller with population weights. This suggests that larger counties have smaller percent changes in sports betting spend. Since we use our estimates to apply to more than 3,000 counties in our application, we prefer the unweighted results as these estimates are likely representative to a larger number of counties, but could overstate the magnitude of betting spend in large population centers.

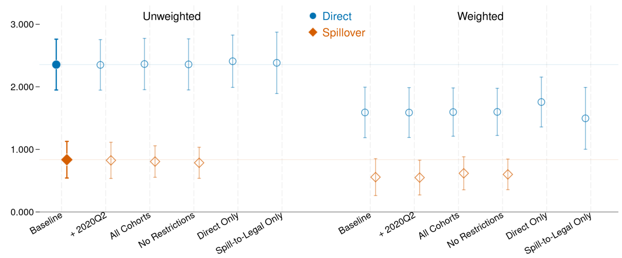
Next, as suggested in Section 4.5, we expect the results to be smaller when we include pre-pandemic adopting cohorts, since the industry was in its infancy at that time. These patterns are confirmed in Panels B, C, and D of Figure A3; however, the difference in estimates is quite small.

Lastly, we report estimates of $\hat{\tau}^D$ using only counties that transition directly into legalization and only counties that first experience spillovers and later legalize, respectively. Both specifications use our preferred sample restrictions. These comparisons provide a test of the immunity assumption, that spillover effects disappear once a state legalizes sports betting. If this assumption were violated, spillover-to-legal counties would be expected to exhibit larger estimated effects, reflecting the sum of the direct treatment effect and any remaining spillover effect. Such a violation could arise if spillover exposure "primes" a state for legalization by establishing persistent betting habits or relationships with sportsbooks. We do not see a systematic pattern of this effect. Panels A and C show similar effects across the two types while Panel B shows a larger estimate (but not statistically different) for spillover-to-legal while Panel D shows a larger estimate (but not statistically different) for direct-to-legal.

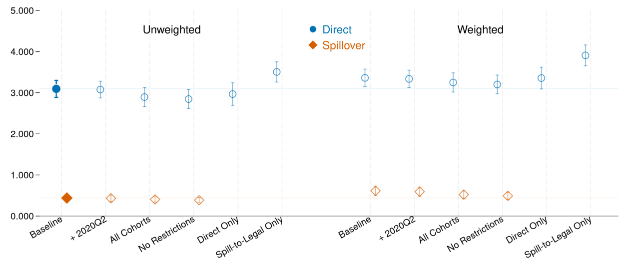
The reduced form results follow the same general pattern. We find that relaxing the pandemic-related

Figure A3: Specification robustness of first-stage difference-in-differences estimates

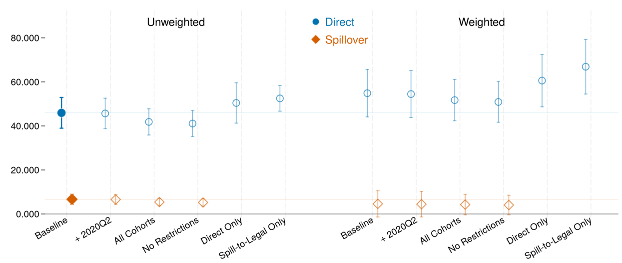
(a) Log(total transaction amount)



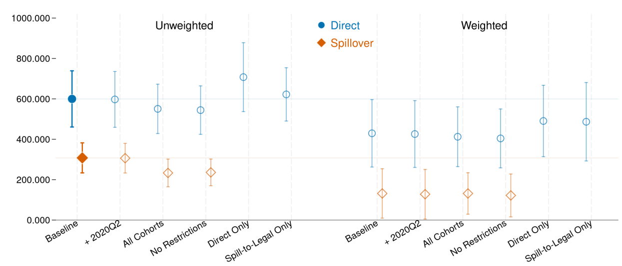
(b) Share with any sportsbook spend



(c) Quarterly spend per person



(d) Quarterly spend per bettor



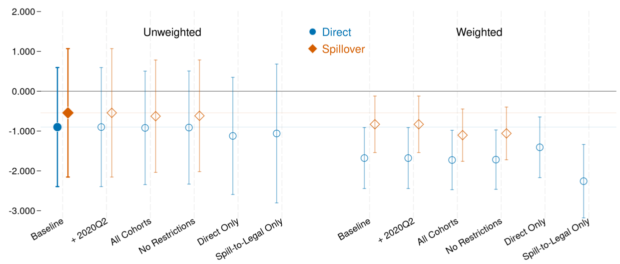
Notes: Each panel plots difference-in-differences estimates $\hat{\tau}^D$ (blue circles, spillover threshold of 60 miles) and $\hat{\tau}^S$ (orange diamonds, spillover threshold of 15 miles) with 95% confidence intervals across twelve specifications for the outcome denoted in the panel title. Estimates are averaged over all post-treatment event-time periods. Filled markers with horizontal reference lines indicate the baseline specification (Baseline model, unweighted); hollow markers denote alternatives. The left block reports unweighted specifications and the right block reports population-weighted specifications. Within each block, the six models are: *Baseline*, which uses post-pandemic cohorts (treatment after 2020:Q1), excludes 2020:Q2, and includes all cohort types; *+ 2020Q2*, which adds the 2020:Q2 cohort; *All Cohorts*, which includes all cohorts (including pre-pandemic) but excludes 2020:Q2; *No Restrictions*, which includes all cohorts and 2020:Q2; *Direct Only*, which applies baseline restrictions but aggregates only direct-to-legal cohorts; and *Spill-to-Legal Only*, which applies baseline restrictions but aggregates only spillover-to-legal cohorts. The solid gray line denotes zero.

Source: Earnest Analytics.

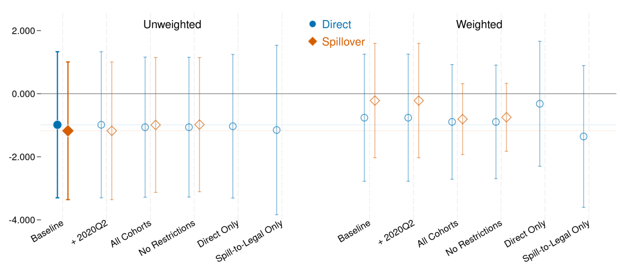
restrictions reduces the absolute value magnitude of treatment effects, but results are still similar across the panel.

Figure A4: Specification robustness of credit score difference-in-differences estimates

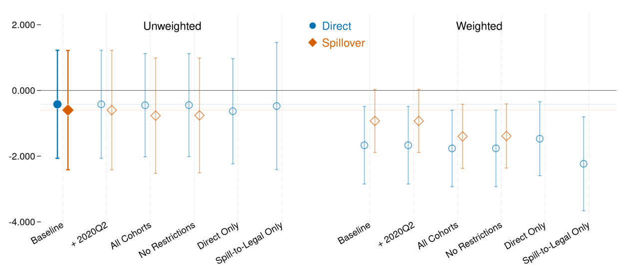
(a) All ages



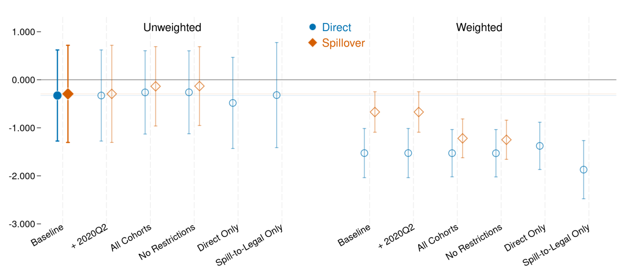
(b) Under 40



(c) 40 to 64



(d) Over 65

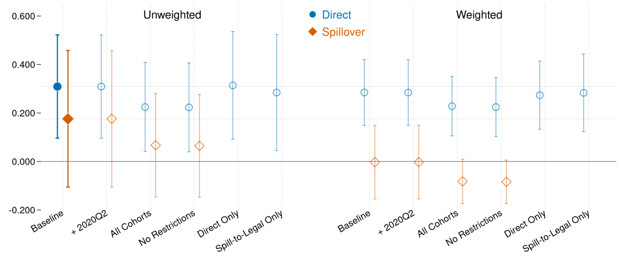


Notes: Each panel plots difference-in-differences estimates $\hat{\tau}^D$ (blue circles, spillover threshold of 60 miles) and $\hat{\tau}^S$ (orange diamonds, spillover threshold of 15 miles) with 95% confidence intervals across twelve specifications for the median Equifax Risk Score 3.0, separately by age group. Estimates are averaged over post-treatment event-time periods beginning four quarters after legalization to allow credit outcomes to respond to the buildup in betting activity. Filled markers with horizontal reference lines indicate the baseline specification (Baseline model, unweighted); hollow markers denote alternatives. The left block reports unweighted specifications and the right block reports population-weighted specifications. Within each block, the six models are: *Baseline*, which uses post-pandemic cohorts (treatment after 2020:Q1), excludes 2020:Q2, and includes all cohort types; *+ 2020Q2*, which adds the 2020:Q2 cohort; *All Cohorts*, which includes all cohorts (including pre-pandemic) but excludes 2020:Q2; *No Restrictions*, which includes all cohorts and 2020:Q2; *Direct Only*, which applies baseline restrictions but aggregates only direct-to-legal cohorts; and *Spill-to-Legal Only*, which applies baseline restrictions but aggregates only spillover-to-legal cohorts. The solid gray line denotes zero.

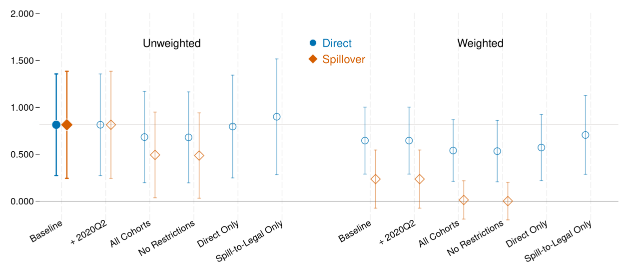
Source: New York Fed Consumer Credit Panel/Equifax.

Figure A5: Specification robustness of delinquency rate difference-in-differences estimates

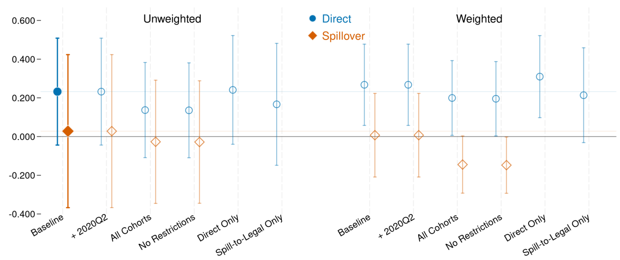
(a) All ages



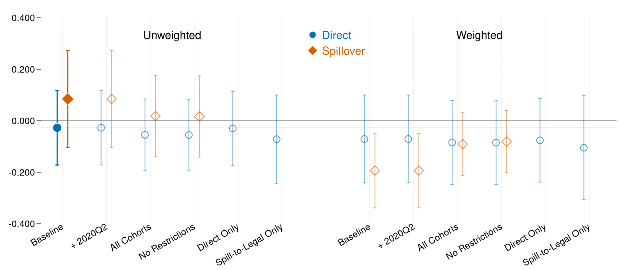
(b) Under 40



(c) 40 to 64



(d) Over 65



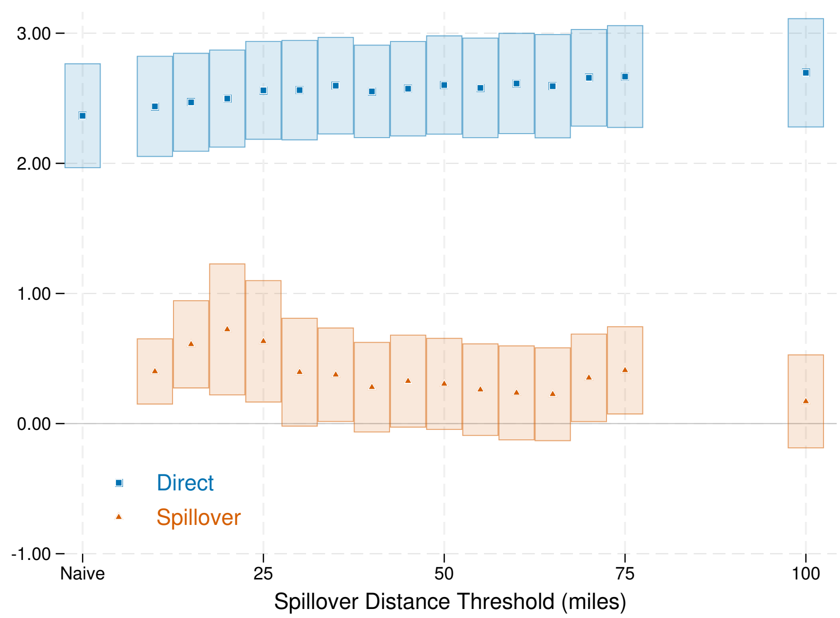
Notes: Each panel plots difference-in-differences estimates $\hat{\tau}^D$ (blue circles, spillover threshold of 60 miles) and $\hat{\tau}^S$ (orange diamonds, spillover threshold of 15 miles) with 95% confidence intervals across twelve specifications for the overall delinquency rate (share of individuals with any account 90 or more days past due on any debt product), separately by age group. Estimates are averaged over post-treatment event-time periods beginning four quarters after legalization to allow credit outcomes to respond to the buildup in betting activity. Filled markers with horizontal reference lines indicate the baseline specification (Baseline model, unweighted); hollow markers denote alternatives. The left block reports unweighted specifications and the right block reports population-weighted specifications. Within each block, the six models are: *Baseline*, which uses post-pandemic cohorts (treatment after 2020:Q1), excludes 2020:Q2, and includes all cohort types; *+ 2020Q2*, which adds the 2020:Q2 cohort; *All Cohorts*, which includes all cohorts (including pre-pandemic) but excludes 2020:Q2; *No Restrictions*, which includes all cohorts and 2020:Q2; *Direct Only*, which applies baseline restrictions but aggregates only direct-to-legal cohorts; and *Spill-to-Legal Only*, which applies baseline restrictions but aggregates only spillover-to-legal cohorts. The solid gray line denotes zero.

Source: New York Fed Consumer Credit Panel/Equifax.

Online Appendix A.5 Validation with Alternate Consumer Spending Data

To validate the first stage results from Section 5, we replicate our main results with an alternate consumer spending data provided by Commerce Signals, a Verisk Analytics business. This data is similar to Earnest Analytics in that it is sourced from anonymous bank statements, but there are several differences that make it less desirable for the main analysis. First, the data only includes spending totals and does not provide measures on the extensive margin: the share of people in a geography with a sportsbook transaction and the total count of active transactors in a geography. Second, the data ends in the first quarter of 2024 which reduces the number of treatment cohorts with post-treatment outcomes. Third, only one sportsbook appears separately from other spend in this dataset. Although it is a market-leading sportsbook, the sportsbook is not available in every state which will attenuate the estimates. Fourth, the geographic coverage is more sparse: we observe 1,071 counties that survive similar culling as the Earnest data compared to 1,690 in the Earnest data. This causes significantly fewer counties in the smaller spillover distance thresholds. Nevertheless, we report estimates using this data in Figure A6 to show that the first stage results presented with Earnest Analytics are broadly consistent with another consumer spending dataset.

Figure A6: Alternate Spending Data: Difference-in-differences estimates for τ_D and τ_S on Log(Transaction Amount), varying thresholds of spillover definition



Notes: The figure above plots the estimates and 95% confidence intervals for $\hat{\tau}_D$ (in blue) and $\hat{\tau}_S$ (in orange) for various definitions of spillover counties as a function of distance from the nearest legal state. The outcome variable is the log of (one plus) the total county-level transaction amount from online sportsbooks in a county-quarter.
Source: Commerce Signals, a Verisk Analytics business.

Online Appendix B Political Economy Simulation Details

This appendix provides additional detail on the construction of the political economy simulation described in Section 6. The simulation has two components: a spending and tax revenue exercise conducted at the county level. We describe each in turn.

Online Appendix B.1 Spending Simulation

The spending simulation applies our first-stage estimates to project how sportsbook spending would change in each not-yet-legal state upon legalization. The key input is the distance gradient of treatment effects estimated in the first stage, which provides the direct effect of legalization ($\hat{\tau}^D(\bar{d})$) and spillover effects ($\hat{\tau}^S(d_c)$) on the log of total quarterly county-level sportsbook spending at various distance thresholds, where $\bar{d} = 60$ miles is the maximum distance at which spillovers are assumed to operate.

For each county c in a not-yet-legal state, we assign a distance threshold based on the county's distance d_c to the nearest legal state. Counties within $\bar{d}$ miles of a legal state are matched to the spillover estimate at the corresponding distance threshold. The incremental spending effect of legalization for county c is then:

$$\Delta_c = \hat{\tau}^D(\bar{d}) - \hat{\tau}^S(d_c) \tag{B.1}$$

Counties beyond $\bar{d}$ receive the full direct effect ($\Delta_c = \hat{\tau}^D(\bar{d})$), since the spillover effect is assumed to be zero past that distance. We impose $\Delta_c \geq 0$ to ensure that legalization cannot reduce spending in any county.

Because the estimates are in log points, we convert the incremental effect to a proportional change. Let $\bar{y}_c$ denote the average betting spend per person in county c during the simulation quarter (observed in the Earnest Analytics data), N_c the adult population of county c from the ACS, and ϕ the annualization factor that scales one quarter's spending to an annual figure. The simulation is conducted using data from a single quarter (2025:Q1), and we define ϕ as the inverse of the simulation quarter's share of total national sportsbook spending (using 2021 to 2024 national spending data) to account for seasonal variation in betting activity across quarters.

The annualized pre-legalization spending level in county c is $\bar{y}_c \cdot N_c \cdot \phi$. We compute the simulated post-legalization spending by scaling by e^{Δ_c}, yielding the tax revenue expression in Equation (8):

$$\hat{T}_s = r \cdot \phi \cdot \sum_{c \in s} \bar{y}_c \cdot N_c \cdot e^{\Delta_c},$$

where r is the assumed tax rate (10% in our baseline).

Pre-legalization spending for each county is computed from the Earnest Analytics data. We calculate $\bar{y}_c$ as the average spending per person among Earnest-observed accounts in the county and multiply by N_c to project total county spending, under the assumption that the Earnest per-person average is representative of the county average before legalization. To limit the influence of outliers, we top-code $\bar{y}_c$ at the 95th percentile of the cross-county distribution. For counties with no Earnest spending data in the target quarter, we impute the median $\bar{y}_c$ from counties with observed spending. These county-level figures are annualized using ϕ and then aggregated to state totals.

Post-legalization spending is the sum of pre-legalization spending and the simulated new spending. Tax revenue for the legalizing state is computed by applying r to total post-legalization spending. However, recall our data only allows us to observe initial deposits but bettors often reinvest winnings. Due to this, our deposits measure is an underestimate of total taxable wagers and thus we underestimate tax revenue as a result.

Online Appendix B.2 Delinquency Simulation

The delinquency simulation applies reduced-form estimates to project the increase in auto loan and credit card delinquencies. The exercise is conducted at the county level using data from the New York Fed Consumer Credit Panel, separately by age group $a \in \{\text{under } 40, 40 \text{ to } 64, \text{over } 65\}$ and credit product type $y \in \{\text{auto loans}, \text{credit cards}\}$.

As with the spending simulation, we exploit the full distance gradient of reduced-form estimates to assign each county its distance-appropriate incremental effect. For each county c in a not-yet-legal state, we assign a distance bin based on the county's distance d_c to the nearest legal state. Counties within $\bar{d}$ miles of a legal state are matched to the spillover estimate at the corresponding distance threshold. The incremental delinquency effect for county c, product y, and age group a is then:

$$\delta_{y,a,c} = \max\left[\hat{\tau}^{D}_{y,a}(\bar{d}) - \hat{\tau}^{S}_{y,a}(d_c), 0\right] \tag{B.2}$$

where d_c is the distance from the county's population-weighted centroid to the nearest legal state boundary. Counties beyond $\bar{d}$ receive the full direct effect, $\delta_{y,a,c} = \hat{\tau}^{D}_{y,a}(\bar{d})$, since the spillover effect is assumed to be zero past that distance. We impose $\delta_{y,a,c} \geq 0$ to ensure that legalization cannot reduce delinquencies.

We convert estimated rate changes to counts of new delinquencies by multiplying the incremental rate by the number of individuals in that county, age group, and credit product cell who hold the relevant type of account:

$$\Delta\text{DQ}_{y,c,a} = \frac{\delta_{y,a}(d_c)}{100} \cdot N^{\text{acct}}_{y,c,a} \tag{B.3}$$

where $N^{\text{acct}}_{y,c,a}$ is the number of individuals in county c, age group a, with an active account of type y. The total simulated increase in delinquencies for state s and product y is obtained by summing across all counties and age groups as in Equation (9):

$$\Delta\text{DQ}_{y,s} = \sum_{c \in s} \sum_{a} \Delta\text{DQ}_{y,c,a},$$

along with the percentage change relative to the existing stock of delinquencies in that state.

Online Appendix B.3 Lost Tax Revenue for Neighboring Legal States

Legalization of a not-yet-legal state redirects cross-border spending that currently generates tax revenue for neighboring legal states. To quantify this, we restrict attention to not-yet-legal counties c with $d_c \leq \bar{d}$ and identify each county's nearest legal state $\ell(c)$. We attribute the county's observed pre-legalization spending $\bar{y}_c \cdot N_c$ to that state on the assumption that cross-border bettors travel to the nearest available legal jurisdiction. Spending is aggregated across all not-yet-legal counties for each legal state ℓ to obtain the total spillover spend that would be lost. The simulated lost tax revenue is then computed via Equation (10):

$$L_\ell = r_\ell \cdot \sum_{c:\ell(c)=\ell,\, d_c \leq \bar{d}} \bar{y}_c \cdot N_c,$$

where r_ℓ is the state-specific effective tax rate. The rates used are: Arkansas 15%, Arizona 20%, Colorado 10%, Florida 10%, Iowa 6.75%, Illinois 30%, Kansas 10%, Kentucky 14.25%, Louisiana 21.5%, Michigan

8.4%, North Carolina 18%, Oregon 51%, Tennessee 19.7%, and Wyoming 10%. For states with graduated rate schedules, such as Illinois, we use an approximate effective rate. In practice, some bettors may travel to a legal state other than the nearest one, and some spending may occur through channels not fully captured in the tax base. For this error, the estimates therefore represent an upper bound on the revenue loss for the nearest legal state. However, recall that our data only allows us to observe initial deposits, but bettors often reinvest winnings. Due to this, our deposits measure is an underestimate of total taxable wagers and thus we underestimate tax revenue as a result.