Online Gambling Policy Effects on Tax Revenue and Irresponsible Gambling

Wayne J. Taylor[*], Daniel M. McCarthy[†], Kenneth C. Wilbur[‡]

June 18, 2024

Abstract

26 U.S. states have legalized online gambling since 2018. We offer empirical research about gambling legalization consequences to help inform policymakers. We estimate how Online Sports Betting (OSB) policies–both with, and without, Online Casino Gaming (OCG)–changed operator revenue, tax collected, helpline calls, and suicides, as well as gambling behaviors measured in a balanced panel of 717,724 gamblers over five years. Gambling behaviors include gambling adoption, participation, regularity, acceleration, and Rates of Irresponsible Gambling, defined as proportions of potential gamblers spending more 1%, 5%, 10% or 15% of monthly income. We use a generalized synthetic control framework to predict counterfactual outcomes and estimate causal effects of state policy changes on outcomes. The findings indicate that gambling policies that legalize OCG increase tax revenue, irresponsible gambling, and gambling helpline calls significantly more than policies that do not legalize OCG. Low-income gamblers are most likely to increase irresponsible gambling after policy changes. We do not find that online gambling legalization increases suicide rates.

Keywords: Digitization, Gambling, Generalized Synthetic Control, Online Casino Gaming, Online Sports Betting, Quasi-Experiments

[*]Southern Methodist University, www.waynejtaylor.com/

[†]Emory University, www.danielminhmccarthy.com

[‡]University of California, San Diego, kennethcwilbur.github.io/website/

1 Introduction

In 2018, the United States Supreme Court overturned a federal law that prevented states from legalizing sports betting. Since then, online gambling markets have grown rapidly.

Online Sports Betting (OSB), defined as online wagers on the outcomes of sports contests, is now legal in 30 states (American Gaming Association, 2024a).[1] In addition, 36 states allow some form of Retail Sports Betting (RSB), which involves betting in physical establishments such as casinos or dedicated betting shops (American Gaming Association, 2024a). The impact of legalization on the sports betting market has been profound. OSB app downloads grew from 6 million in 2019 to 33 million in 2023 (Data.AI, 2024). OSB handle, or the total value of all OSB wagers, grew 14-fold over the same period, reaching $113.9 billion. Correspondingly, OSB operators' taxable revenues increased 18-fold to $9.2 billion.

Online casino gaming (OCG), which includes online wagers on casino games such as blackjack, poker, roulette, and slot machines, is currently legal in 7 states, including three new states since 2020. OCG taxable revenue grew 12-fold between 2019 and 2023, reaching $5.9 billion.

At the same time, calls to National Council on Problem Gambling (NCPG) gambler helplines grew by 150% over 5 years, from 32,666 in 2019 to 83,660 in 2023 (NCPG 2024a). NCPG (2024b) states that 1% of U.S. adults meet the criteria for a severe gambling problem in a given year, and another 2% to 3% experience mild or moderate gambling problems.

Most people accept gambling and many people gamble, but a substantial minority opposes gambling. In a survey, 49% of U.S. adults said they gambled within the past year (AGA 2023). 56% viewed casino gambling as "acceptable for me and others," and another 31% viewed gambling as "acceptable for others, but not for me personally." However, the remaining 12% viewed casino gambling as "not acceptable for anyone." The UK Department for Culture, Media and Sport (2023) summarizes the tradeoffs by saying that "Millions of people enjoy gambling every year... and much of this is now done through smartphones... people can bet 24 hours a day through 'mobile virtual casinos' in their pockets. Most people play without issue, but there are too many cases of addiction, catastrophic financial loss and, in some tragic cases, suicide."

In our view, gambling legalization resembles illegal substance policy debates, as both types of

[1] This market contrasts with the Daily Fantasy Sports (DFS) market, which involves online wagers on individual player performances within sports contests and has been legal in nearly all states since before 2018 (Wallach, 2017).

policy changes may cause a range of significant consequences. Legalization can increase personal freedom and enjoyment; enable regulation and taxation of provision and consumption; potentially reduce government spending on detection, enforcement and punishment of illegal provision and consumption; and make it easier for addicted consumers to access helpful resources. However, legalization may also expand access; license and normalize consumption, thereby increasing total consumption and potentially harmful consumption; it may therefore increase attendant harms and require more total treatment resources; and it may fail to curb illegal channels.[2]

State gambling policies differ and may have both positive and negative outcomes. Empirical research is needed to help inform policymakers as they re-evaluate recent policy changes, consider new policies, and continue to calibrate and refine policy choices. Policymakers require credible estimates in order to consider benefits and costs (e.g., Fong et al., 2022; Rao and Wang, 2017; Seiler et al., 2020; Tuchman, 2019).

In this paper, we estimate how online gambling policy changes have affected a range of related gambler behaviors, with the goal of informing policymakers about the measurable consequences – positive and negative – of those changes. We therefore intend a dispassionate analysis with the goal of "letting the data speak," as much as possible. We do not make normative recommendations or advocate policies, as there is no value-neutral way to trade off disparate outcomes against each other. For example, even if we could somehow translate all effects into economic costs and benefits, aggregating those quantities and advocating for a net-positive or net-negative approach embeds a political value system. Further, the full distributions of heterogeneous costs and benefits would be difficult to identify. So, even if gambling were evaluable as a social good or social bad in aggregate, policymakers may need to trade off distributions of harms and benefits, which again requires a political value system to reach a conclusion. Finally, there are some important decision-relevant information which we cannot measure directly, such as retail casino spending, which would inherently limit the credibility of any recommendations reached solely based on our empirical results. As researchers, we simply hope to provide causal evidence about recent policy changes to relevant representatives and regulators, in an effort to help inform their future policy evaluations and decisions.

[2]AGA (2022) estimates that illegal gambling revenue exceeds legal gambling revenue, as tax avoidance–among other undesirable social consequences–can enable unregulated operators to offer better odds than regulated operators.

Numerous additional policy changes appear possible or even likely, as lobbying continues and legislators consider new rules. At the time of writing, six states are considering OSB legislation or ballot initiatives (American Gaming Association, 2024a). Further public discussions and proposals are likely in states such as California and Texas which currently do not allow online gambling (Legal Sports Report, 2024). Meanwhile, several legal states are considering regulatory revisions (Fletcher, 2024). Therefore, we believe the results we present in this paper are important and timely.

We analyze how gambling policies affect the population of publicly-reported online gambling metrics, including OSB handle, operator taxable revenue and total tax revenues. We also analyze a financial panel dataset that tracks digital spending data for 717,724 U.S. gamblers across all major online gambling operators. Prior research shows that gambling harms increase disproportionately with the percentage of income spent on gambling (Canadian Centre on Substance Use and Addiction, 2021). Therefore, we introduce a measure we call "Rate of Irresponsible Gambling" (RIG*x*), tracking the proportion of gamblers whose net gambling spend exceeds income thresholds, such as 10%. We also track gamblers by gambling adoption, participation, regularity, and expenditure acceleration. We use a Generalized Synthetic Control model to estimate the gambling policy effects on a variety of policy-relevant outcome measures within a principled quasi-experimental framework, and compare the results to staggered difference-in-differences estimators. We discuss how these estimates might help to inform policymakers in relation to a range of relevant policy levers.

We group state policies by the types of gambling they allow. Our most important findings are the following:

1. Gambling policies that legalize OCG increased tax and irresponsible gambling significantly more than those that do not legalize OCG.
2. Gambling policy effects on irresponsible gambling are significantly larger among low-income gamblers than high-income gamblers. The change in bottom-tercile income earners spending at least 10% of income on gambling is about 5 times larger than the change in top-tercile income earners.
3. Online gambling policies increased gambler helpline calls but we do not find that they increased suicides per capita.

In the following sections, we review existing literature, describe the data and model-free evidence, explain the estimator, discuss the results, and then conclude with implications, limitations and directions for future research.

1.1 Existing Literature

A common concern about gambling legalization is the potential for misuse, as gambling has long been classified as a potentially addictive activity. According to the American Psychiatric Association (2024), Gambling Disorder may be diagnosed if an individual exhibits at least four out of nine symptoms in a year, including withdrawal, unsuccessful efforts to control gambling activity, chasing losses, preoccupation, deception, acclimation, and/or loss of a relationship or job. Psychologists have developed similar surveys to measure problem gambling severity in therapeutic settings and problem gambling incidence in longitudinal studies. Problem gambling surveys include the South Oaks Gambling Screen (Lesieur and Blue, 1987) and the Problem Gambling Severity Index (PGSI Ferris and Wynne, 2001), among others. As National Council on Problem Gambling (2024b) puts it, "The amount of money lost or won does not determine when gambling becomes problematic," and "The frequency of a person's gambling does not determine whether they have a gambling problem."[3]

The economics literature has historically inferred addiction based on the idea that past consumption directly affects current utility (Pollak, 1970; Becker and Murphy, 1988; Narayanan and Manchanda, 2012). Narayanan and Manchanda (2012) estimated a model of individual level gambling decisions, finding that 8% of gamblers studied showed signs of addictive behavior. They also found that addicted gamblers were more than twice as responsive to casino promotions ("comps") as non-addicted gamblers.[4] Castelo-Branco and Manchanda (2023) applied a related approach to measure video game addiction, finding that between 15% and 18% of players exhibited addictive tendencies. Allcott et al. (2022) extended this methodology to distinguish between habit

[3]Philander (2014) applied supervised machine learning algorithms to archival gameplay data to predict account deletion reasons such as "gambling-related problems," "no further interest" and "being unsatisfied with the service." He concluded that "Bet intensity, variability, frequency and trajectory... are noted to be insufficient variables to classify probable disordered gamblers with reasonable accuracy."

[4]In a similar vein, Park and Manchanda (2015) modeled three types of peer effects within groups of gamblers, finding that gamblers who were less affected by peer gambling showed stronger evidence of addictive behavior. Similarly, Taylor and Bodapati (2017) and Taylor and Zhang (2023) find that gambling outcomes strongly influence subsequent gambling trips and gambling frequency.

formation, which they estimated by manipulating incentives to change consumption behaviors, and self-control problems, which they estimated by manipulating the availability of consumption-constraining technology. They found that self-control problems caused 31% of social media use.

We do not directly study gambling addiction or problem gambling in this paper, as we do not directly observe individual gambles, individual gamble outcomes, or direct measures of gambling problems. Instead, we contribute to a newer "responsible gambling" literature. This recent literature correlates gambling harms with *Gamble%*, defined as a gambler's net gambling spend (losses minus winnings) divided by income.

The Canadian Centre on Substance Use and Addiction (CCSUA; 2021) conducted a meta-analysis of 11 longitudinal surveys that collected net gambling spend and problem gambling indicators. It found that all types of measured gambling harms increased with *Gamble%*. The increases were steeper when *Gamble%* exceeded 1% of monthly income. CCSUA developed a public health message: "Gamble no more than 1% of household income before tax per month." CCSUA surveyed 4,582 Canadian gamblers to evaluate the guideline (Young et al., 2024): 52% said the 1% guideline sounded "just right," 31% said "a little too high" or "very much too high," and 14% said "a little too low" or "very much too low." Therefore, the 1% responsible-gambling guideline appears to be aligned or conservative compared to most Canadian gamblers' opinions.

Further studies have substantiated some of the CCSUA (2021) findings using passively measured data. Jonsson et al. (2022) studied how passively measured *Gamble%* related to self-reported gambling harms, using spending data from the Norwegian monopoly gambling operator, and PGSI surveys administered to gamblers on-site. They found that risk curves were relatively flat for *Gamble%* below 1%, with an inflection point at about 1% of median household income and rapidly rising risks thereafter. Relatedly, Muggleton et al. (2021) partnered with a large U.K. bank in order to measure associations between passively-measured *Gamble%* and a passively-measured indicator of harm, All Cause Mortality. The gambling/harm relationship appeared again, with a large positive association between *Gamble%* and likelihood of premature death.

Of course, correlations between *Gamble%* and gambling harms do not prove causation or even a one-way relationship between the two variables. Causal evidence is difficult to obtain, as gambling behaviors and gambling harms may share common causes, and random assignment of *Gamble%* is not easily available in field data due to ethical and practical challenges. Yet policymakers may decide to act without waiting for high-quality causal evidence if the public health concerns are sufficiently compelling.

In this paper, we seek to build on the responsible-gambling literature by offering a contrasting 'Rate of Irresponsible Gambling' (RIG*x*) measure. RIG*x* is the proportion of gamblers whose *Gamble%* exceeds *x%* of income. We estimate how RIG*x* changed with various gambling policies, in an effort to inform policymakers of how legislative changes impact rates of irresponsible gambling. We report robustness to various income thresholds and show how policy effects on RIG*x* vary across income terciles. We believe this to be the first longitudinal analysis and policy evaluation that incorporates responsible gambling metrics, and that these measures will prove useful in future research and policy evaluations. We also estimate gambling policy effects on operator revenue and state tax revenues; gambler behaviors including adoption, participation, regularity and spending acceleration; gambler helpline calls and suicide rates, to further contextualize RIG*x* measures and present a broader array of related evidence.

2 Data: Treatments, Measures, Model-free Evidence

This section describes gambling policy changes, data sources and outcome measures. It concludes with data visualizations that illustrate the identifying variation and suggest policy effects.

2.1 A Brief Policy History, State Treatment Groups and Controls

The U.S. Congress passed the Professional and Amateur Sports Protection Act (PASPA) in 1992. PASPA prohibited new state regulations from allowing sports gambling. PASPA exempted existing sports gambling rules in four states (Delaware, Montana, Nevada and Oregon). Fantasy sports were exempted in both PASPA and the Unlawful Internet Gambling Enforcement Act of 2006, enabling DraftKings and FanDuel to operate Daily Fantasy Sports (DFS) services in nearly every state (Wallach, 2017).

In 2018, the U.S. Supreme Court ruled in *Murphy v. National Collegiate Athletic Association* that PASPA was an unconstitutional effort to control state lawmaking. The majority opinion held that "Congress can regulate sports gambling directly, but if it elects not to do so, each state is free to act on its own." Many states adopted new gambling regulations after the ruling.

Each state policy contains some unique elements, so we present state-specific policy effect estimates in Online Appendix Section OA.1. We also estimate more generalizable findings based on states that implemented similar policies, to understand how different policy types compared to each other. We took three steps to construct state treatment and control groups of states:

1. We studied American Gaming Association (2024b) to identify which states enacted new OSB and OCG rules during the sample period (January 2019 to September 2023).
2. We identified retail gambling policies these same states had enacted.
3. We did extensive further research, using news reports, government documents, trade press and consumer guides to understand policy attributes and implementation timing.

This process led to 5 "treatment groups" collectively representing 14 states that each enacted reasonably general policies between 2020 and 2023, including three single-state treatment "groups." It also identified a "control group" of 18 states that exhibited no relevant policy variation during the sample period. Table 1 lists policy types, states by policy type, and policy implementation timings, with the most recent date serving as the treatment date.[5]

"OSB after RSB" represents states implementing online sports betting after having previously legalized retail sport betting. "OSB with RSB" represents states that implemented online sports betting and retail sports betting within 60 days of each other. "OSB no RSB" implemented OSB but does not allow RSB. "OSB & OCG after RSB" and "OSB & OCG with RSB" are similar to the first two groups, except that they implemented online casino gaming at the same time as online sports betting. Each of the latter three groups consists of a single state, and therefore may be less generalizable than the two multiple-state groups.

18 states are excluded from the analysis. The excluded states are Florida, where OSB was legal for one month only; Indiana, Iowa, Pennsylvania and West Virginia, whose policies changed too early in 2019 to establish a baseline; Kentucky and Massachusetts, whose policies changed too late in 2023 to estimate an effect; and Maine, Montana, Nebraska, New Hampshire, North Dakota, Oregon, Rhode Island, South Dakota, Washington and Wisconsin, who each enacted online gambling policies with unusual attributes, e.g., limiting OSB to state lotteries and/or tribal operators, among other idiosyncrasies.

[5] All treatment states had legal retail casino gaming prior to 2019, except Arkansas which introduced RCG in 2020 and Virginia in 2022.

Table 1: Treatment Groups, States and Times

Treatment Group	State	RSB	OSB	OCG
OSB after RSB	Illinois	3/20	6/20	
	New York	7/19	1/22	
	Louisiana	10/21	1/22	
	Arkansas	7/19	3/22	
	Maryland	12/21	11/22	
OSB with RSB	Colorado	5/20	6/20	
	Virginia	1/21	1/21	
	Wyoming	9/21	9/21	
	Arizona	9/21	9/21	
	Kansas	9/22	9/22	
	Ohio	1/23	1/23	
OSB no RSB	Tennessee		11/20	
OSB & OCG after RSB	Michigan	3/20	1/21	1/21
OSB & OCG with RSB	Connecticut	9/21	10/21	10/21

Control states: AK, AL, CA, DE, GA, HI, ID, MN, MO, MS, NJ, NM, NV, OK, SC, TX, UT, VT

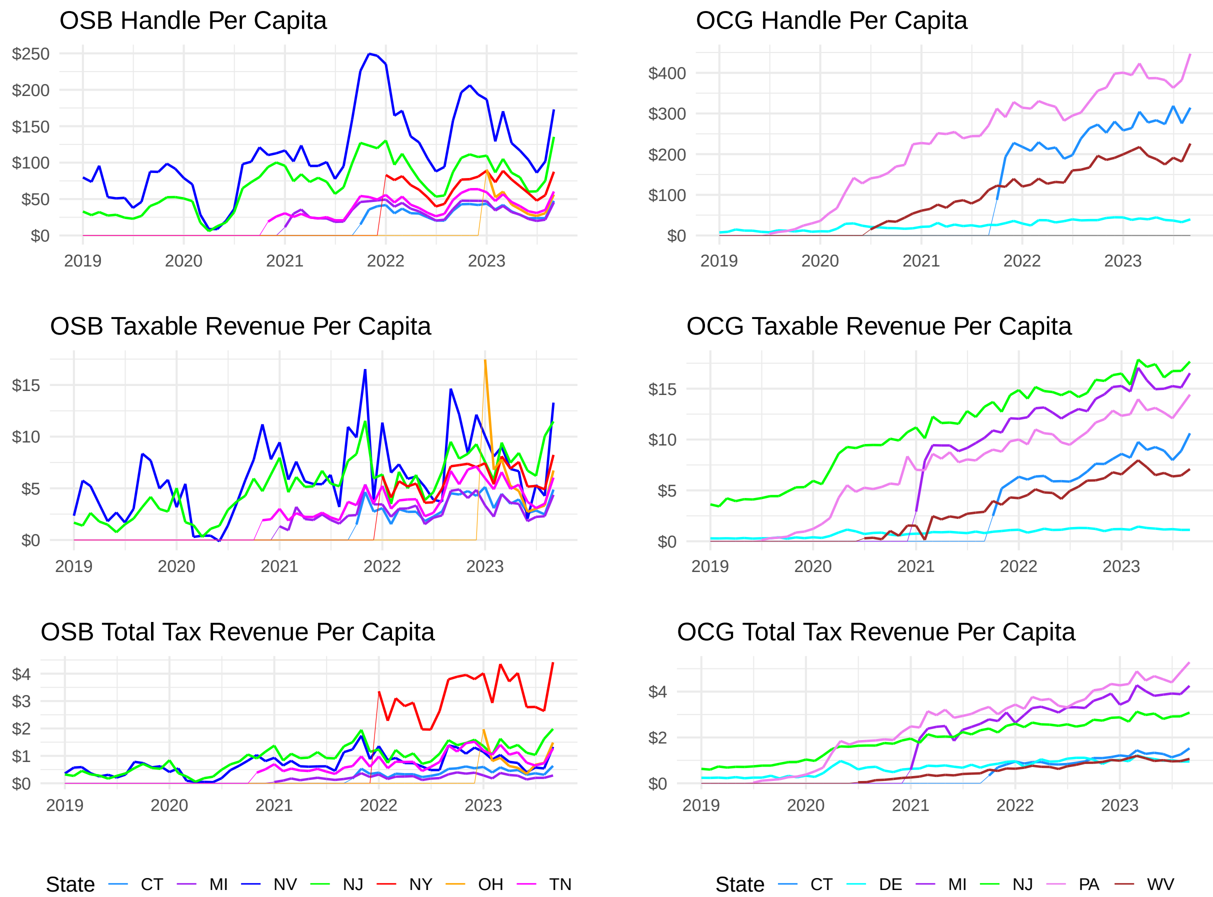
2.2 Public Metrics: Handle, Revenue and Tax

Gambling regulation directly increases tax revenue, so we analyze a dataset that reports the population of publicly-reported OSB and OCG handle, operator taxable revenue, and total tax revenue collected. The data were compiled by journalists working for *Legal Sports Report* from state gambling regulators and tax revenue reports. We have corroborated multiple data points by matching them to original sources.[6]

Figure 1a shows OSB handle, operator taxable revenue, and total tax revenue, for the most populous state in each treatment group and the two most populous control states with legal gambling. OSB became a sizable activity in some treatment states. For example, the average net gambling spend per New York state resident was $88.98 in March 2023. OSB trends correspond strongly to popular sporting events, especially professional football, the collegiate basketball tournament, and professional basketball playoff, with lessened activity in summer months. The Covid-19 pandemic and associated shutdown in March 2020 canceled most professional sports, explaining why OSB activity fell and remained low until regular sports activity resumed. In general, state OSB handles

[6]Throughout the paper, we use the Consumer Price Index (CPI) to convert nominal measures to real January 2023 dollars. We use 2020 state population data to express state-level outcomes on per-capita bases.

per capita do not show sustained upward trends in the final two years of the sample.



(a) Public OSB metrics

(b) Public OCG metrics

Figure 1: Public metrics for selected states

Taxable operator revenue per capita tracks OSB handle but with greater variance. On average, operators pay out 92.3% of all OSB handle to bettors and retain 7.7%. Operator taxable revenue per dollar of handle ranged across these 7 states from 5.5% in Nevada up to 9.8% in Tennessee.

OSB total tax revenue per capita includes federal, state and local tax payments. State governments received 85% of all reported tax revenue. On average, OSB operators in these seven states paid about 25% of operator revenue in taxes, but effective tax rates varied from 0.6% of handle dollars in Nevada up to 4.7% in New York. Total tax revenue per dollar of operator taxable revenue ranged from 7.7% in Michigan up to 53.9% in New York.[7]

[7]New York state collects 51% of gambling operator taxable revenues, then deploys 1% of money collected into services for problem gamblers, among other purposes (New York State, 2024).

Figure 1b shows Online Casino Gaming handle, taxable revenue and total tax revenue per capita. OSB wagers lead to money changing hands on every bet, but some OCG games–e.g., poker tournaments–involve long sequences of wagers with money only changing hands at the end of the sequence. Therefore, retail casinos typically do not track gaming handle, fewer states mandate its reporting or disclosure, and we do not include it in the set of causal effects we estimate.

Only four of the seven OCG states report total OCG handle. Figure 1b shows that, unlike OSB handle, OCG handle per capita grew steadily after legalization in all 3 states that legalized it recently, and reached considerable sizes. Connecticut and Pennsylvania both reached higher OCG handle per capita by September 2023 than the maximum OSB handle per capita in Figure 1a.

OCG operator taxable revenues and total tax revenues, like handle, grew steadily after legalization. These metrics are also available for Michigan and New Jersey, whose time series again show steady growth. Taxable revenue per dollar of handle varied little among the four states that report handle, from 3.0% in Connecticut to 3.3% in Pennsylvania and West Virginia. However, total tax revenue per dollar of taxable revenue ranged from 14.3% in Connecticut to 85% in Delaware, reflecting substantial variation in state gambling tax policies.

The public data report the population of publicly available OSB and OCG metrics, but they do not include all related lines of business. They contain little information about RSB, retail casino gaming (RCG) or lottery activity. This is partly due to fewer disclosure requirements in older regulations, and partly due to retail gambling businesses which operate on sovereign tribal lands and therefore are subject to different taxation and reporting regulations. They also exclude DFS metrics as few states regulated DFS during the sample period.

2.3 Gambler Panel and Measures

We analyze a financial panel dataset provided by an anonymous company. The panel reports card payment records from several million anonymous U.S. adults to about 5,000 large merchants. Each observation resembles a row on a monthly card statement, reporting an anonymous panelist ID, merchant name, transaction state, amount and date. The dataset also includes monthly pre-tax household income for some panelists who received direct deposits.

We constructed a balanced panel of 717,724 consumers who (a) used tracked cards regularly for primary expenses (e.g., groceries) in every year from 2019 until 2023, (b) transferred money to or from one of the 41 gambling merchants in the data (listed in Online Appendix Table OA1), and (c) whose first gambling transaction was a deposit.[8] We call these consumers "potential gamblers" (PG) because our longitudinal research designs track changes in gambling activity rates across months, and most potential gamblers did not gamble within any particular month.[9] Income records are available for 33.7% of potential gamblers.

The advantages of these data are that they passively measure financial transfers to and from all major online gambling services–including DraftKings, FanDuel, MGM Bet and others–for a large panel of potential gamblers over a long period of time. However, there are also data limitations which affect our research design and interpretations:

1. Gambling operator deposits and withdrawals enable us to measure net gambling spend at the person/month level, but we cannot measure gambling frequency or intensity within online gambling services, and we cannot observe balances carried within gambling services.
2. We cannot attribute payments to distinct services within merchants. Therefore, we cannot distinguish between DFS, OSB and OCG payments within large gambling merchants like DraftKings or FanDuel in places where multiple services are legal.[10]
3. We cannot observe cash gambling or attribute intermediated payments (e.g., Paypal, Venmo) to merchants. Most retail casinos and many online casinos do not accept credit cards for gambling transactions, to comply with regulations and to prevent gamblers from accumulating gambling debts at high interest rates (e.g., Karp and Bratton, 2024). Therefore, we have been unable to find any source that reliably measures retail gambling spend across competing brands.

Next, we further describe the panel gambling data, gambling behaviors that we track by state and time, and available indicators of external validity.

[8]Point (c) drops some left-censored gamblers who took withdrawals after unobserved deposits.

[9]We acknowledge the sample excludes consumers who never gambled during this five-year period, so this should be interpreted as closer to served available market rather than total addressable market.

[10]Online gambling operators decline to offer services in places where such services are illegal. Consumers can try to mask their digital locations but operators can detect, deter and punish many practices; see e.g. (DraftKings, 2024; Egerer and Marionneau, 2024; FanDuel, 2023; Gentry, 2023).

2.3.1 Panel descriptives

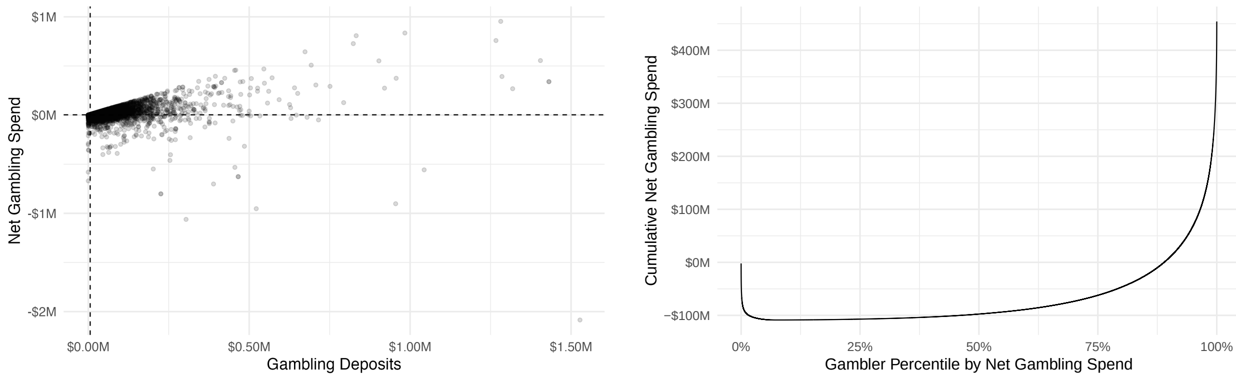
Table 2 summarizes gambling transactions in the balanced panel of potential gamblers. In total, the potential gamblers accumulated net gambling spend of $454.3 million. The average gambler made 18 deposits totaling $1,375 into gambling merchants, and 2 withdrawals totaling $742. These averages are inflated by long tails: the standard deviation of net gambling spend is 12.5 times larger than the mean, and the median gambler deposited only 10% as much as the avrage gambler.

Table 2: Panel statistics

			Percentile		
	Avg.	**SD**	**25th**	**50th**	**75th**
# of Transactions	21	100	1	3	7
# of Deposits	18	87	1	2	7
# of Withdrawals	2	18	0	0	0
Total Gambling Deposits	$1,375	$11,344	$39	$136	$503
Total Gambling Withdrawals	$742	$10,885	$0	$0	$0
Net Gambling Spend	$633	$7,910	$25	$99	$365
Annual Income	$132.0k	$110.3k	$62.7k	$104.6k	$171.7k

Panel 2a plots all 717,724 potential gamblers by aggregate deposits and net gambling spend into online gambling services. The dashed lines near the axes represent the 95th percentiles of each variable. Over 90% of potential gamblers locate in the tiny box at the origin, bounded by the axes and the dashed lines at the 95th percentiles. There are also long tails of gamblers ("whales"), including 43 who deposited more than $500,000 each into gambling services in this 4.75-year span, of whom 10 withdrew more than they deposited.

Figure 2b orders potential gamblers by their net gambling spend, and then plots cumulative operator revenue summed across gambler ranks. The curve shows that fewer than 5% of potential gamblers withdrew more than they spent, collectively earning more than $100 million. Most online gambling operators limit or ban bettors who are too successful in order to minimize future operator losses (Funt, 2022). Still, it takes the next 80% of gamblers to offset those initial operator losses, with the industry breaking even at about the 85th percentile of gamblers. Finally, about half of net operator revenue comes from the highest-spending 3% of gamblers, illustrating the importance of "whales" relative to "minnows."



(a) Net gambling spend by deposits

(b) Industry revenue by gambler rank

Figure 2: Gambler-level metrics

Panel (a) plots net gambling spend (the difference between customer deposits and withdrawals) against total deposits for every gambler in the dataset. The dashed lines represent the 95th precentile on each variable. Panel (b) orders all gamblers by their net gambling spend and plots cumulative net gambling spend by percentile.

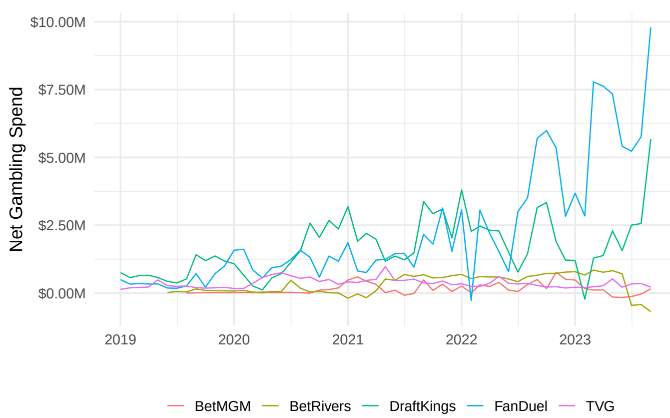


Figure 3: Net gambling spend for top 5 merchants

Figure 3 aggregates potential gamblers' net gambling spend by operator and month. FanDuel and DraftKings were the two largest consumer brands by far, and increasingly so as time proceeded.

2.3.2 Panel-based Gambling Activities

We measure policy effects on retail casino spending, both in-state and out-of-state. We do this by aggregating spending within a subset of 21 retail-only gambling merchants, such as Golden Nugget Hotel, MGM Grand (excluding Bet MGM, an online-only gambling merchant), Bellagio, Wynn, and The Venetian. Retail casino spending primarily pays for accommodation, meals and shows. As such, it offers a potential proxy for potential substitution between online and offline gambling modalities. Distinguishing in-state from out-of-state retail casino spending may indicate the extent to which state gambling rules can retain or repatriate state citizens' gambling taxes paid by state residents.

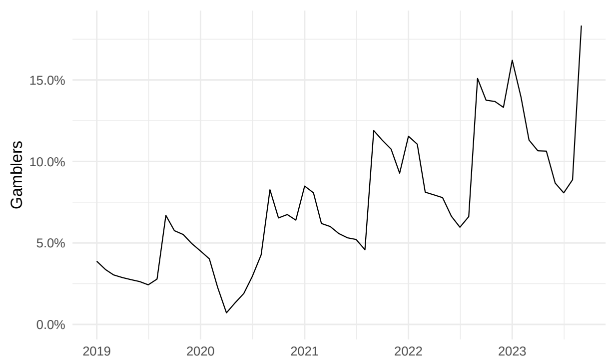
We also measure panel frequencies of four gambling activities in each state and month:

1. Gamblers, defined as any transaction with any gambling merchant, to assess policy effects on gambling participation;
2. New gamblers, defined as any potential gambler's first-observed gambling transaction, to assess policy effects on gambler activation;[11]
3. Regulars, defined as gambling transactions in at least 6 consecutive months, to assess policy effects on regular gambling;
4. Accelerators, defined as increasing net gambling spend by at least 50% in at least 2 consecutive months from a base of at least $100, because problem gambling surveys ask about "gambl[ing] with increasing amounts to achieve the desired excitement" and "after losing money gambling, often returning to get even." No transation-based metric can measure gambling problems directly but we think accelerating net gambling spend might indicate potentially troubling patterns.

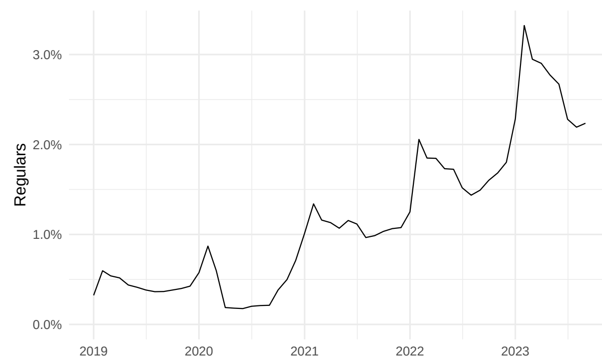
Figure 4 plots gambling behaviors per potential gambler ("PPG"). Panel 4a shows that monthly gambler participation steadily increased about five-fold over the sample period, exceeding 17%

[11] Panel membership is balanced and spending records go back as far as 2016 for some panelists, but it is possible that unobserved pre-2019 gambling could lead to small overestimates of new online gambling among potential gamblers.

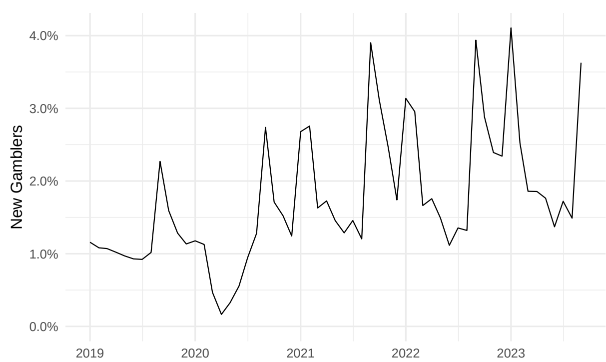
of potential gamblers, and reflected similar seasonal trends as OSB metrics. Panel 4b shows new gamblers PPG also reflected OSB seasonal trends but remained relatively stable from 2021-2023. Panel 4c shows that regular gamblers also increased about five-fold from 2019 until 2023, with faster increases during peak OSB periods, peaking at over 3% of potential gamblers. Panel 4d shows that accelerators PPG was relatively stable from 2021-23, peaking at about 0.05% of potential gamblers (about 359 panelists per month), and showing less correlation with OSB trends than other metrics.



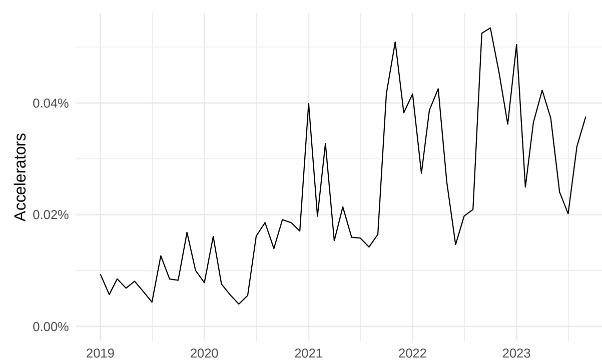
(a) Gamblers PPG



(c) Regulars PPG



(b) New gamblers PPG



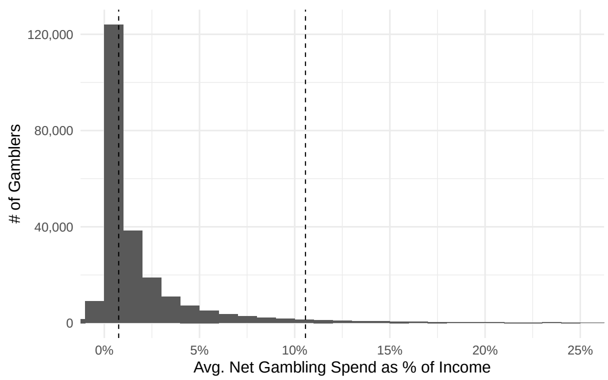
(d) Accelerators PPG

Figure 4: Gambling activities per potential gambler (PPG)

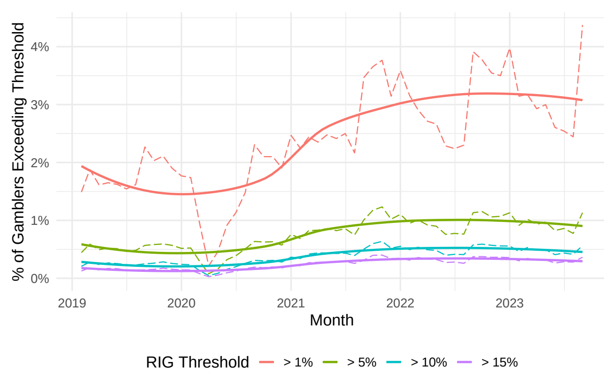
2.3.3 Rates of Irresponsible Gambling

Recent literature offers Responsible Gambling guidelines that advise gamblers to spend less than 1% of monthly income on gambling (CCSUA 2021). In contrast, we define Rates of Irresponsible Gambling ("RIG") as proportions of gamblers who exceeded 1% of income or higher thresholds.

We calculate mean *Gamble%* for each gambler across all gambling months, then plot the histogram of gambler-level *Gamble%* in Figure 5a. 4.6% of gamblers had net gambling spend below zero in gambling months, i.e., gambling withdrawals exceeding gambling deposits. Yet 43% of gamblers exceeded the CCSUA's 1% responsible gambling guideline during their average gambling month; 11.8% spent more than 5% of income on gambling in gambling months; 5.3% spent more than 10% of income on gambling in gambling months; and 3.2% spent more than 15% of income on gambling in gambling months. The substantial proportions of gamblers with high *Gamble%* lead us to report and analyze RIGx at multiple income thresholds. Still, one might argue that Figure 5a misreports Gamble% by excluding non-gambling months.



(a) # of Gamblers by mean Gamble% in gambling months



(b) 4 RIGx by time

Figure 5: Irresponsible gambling metrics

Panel (a) is a histogram of gambler-level mean *Gamble%* across all gambling months, with the median and 95th percentile denoted by vertical dashed lines. Panel (b) plots RIGx over time, where the dashed lines are the data points and the solid lines apply LOESS smoothing.

Figure 5b plots the proportions of gamblers whose net gambling spend exceeded 1%, 5%, 10% or 15% of income, this time including non-gambling months as zeros. RIGx measures increase throughout the sample period and trend upward during the NFL season. RIG1 ranged between 1.5-2.3% of gamblers in 2019, then increased to between 2.2-4.4% in 2023. The higher income thresholds show less variability and smaller proportional increases over time, and 2023 levels are not visually higher than 2022 levels. RIG5 hovered around 1.0% of panel gamblers in 2023, with RIG10 averaging around 0.4%, and RIG15 around 0.3%.

The time series of RIGx measures at various income thresholds show quite similar changes across time. For example, RIG10 correlates with RIG1 at .81, with RIG5 at .93, and with RIG15 at .94. We focus most of our analyses on RIG10, as the 10% threshold is a more conservative and concerning measure of irresponsible gambling than the more common 1% income threshold. We will further estimate policy effects on RIG10 within distinct terciles of the income distribution, labeled as low, medium and high.

2.3.4 Financial Panel External Validity

Panel privacy protections limit our knowledge about panel membership. We do not observe panelist demographics, other than income for some panelists. It is well known that some US consumers do not have access to digital payments, so no digital payments panel could be fully representative. At the same time, most unbanked consumers would not be in the market for online gambling services, so their relevance to the analysis is unclear. We report three indicators of potential external validity.

Figure 6 compares distributions of annual incomes observed among potential gamblers and the Current Population Survey (CPS). Overall, as one would expect, online gamblers earn more than the U.S. average: the CPS mean annual household income is $106,400, whereas the financial panel gambers' mean annual household income is $130,096, but there are potential gamblers observed in all income categories.

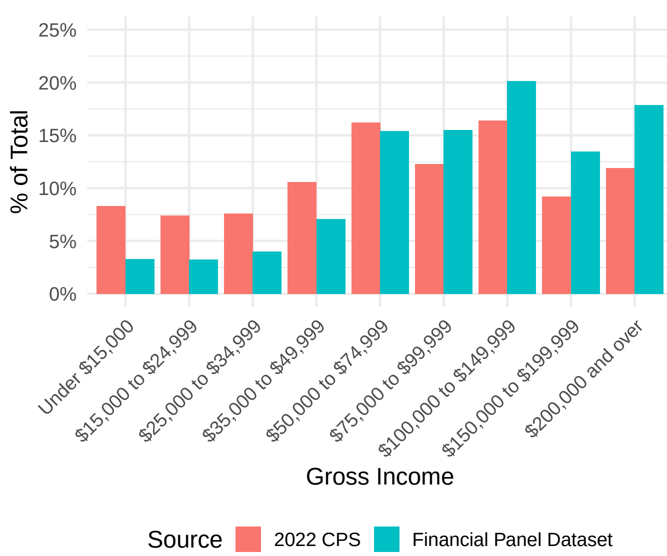
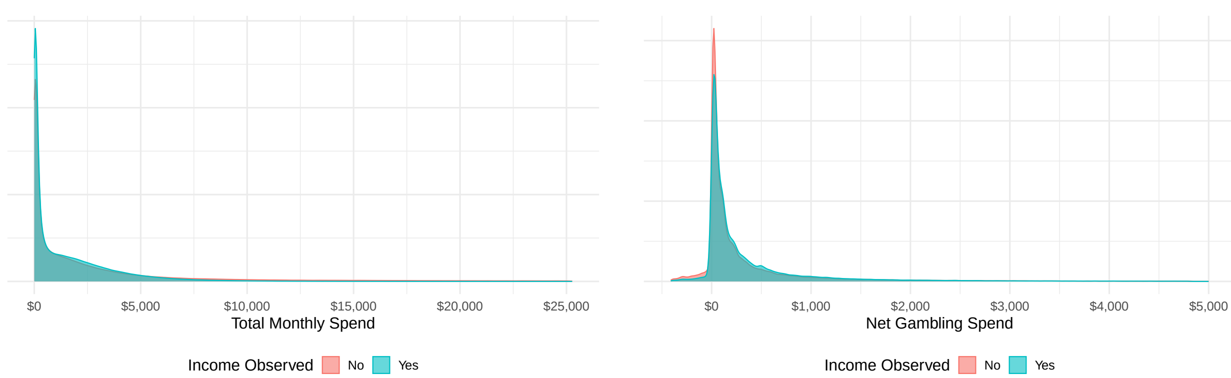


Figure 6: Annual Income Distribution Comparison

Kim and McCarthy (2023) checked panel representativeness by correlating panel-based revenue measures with public revenue disclosures for 36 restaurant brands over four years. They found a median correlation of 0.98, and an interquartile range of 0.92 to 0.99, suggesting that panel-based revenue metrics accurately tracked intertemporal changes in population-based revenue.

RIG*x* analyses condition on income observations, so we investigated how income observability relates to panel-based spending behaviors by comparing marginal distributions of spending with and without income observations. Figure 7a, compares total spending distributions (i.e., inclusive of non-gambling merchants) across these two subgroups, showing that there is little visual difference between the two densities. Figure 7b compares the net gambling spend distributions across the same two subgroups, showing that there is more mass near zero and below zero when income is unobserved, but there are limited apparent differences at higher levels of gambling spending. Overall, these results suggest that conditioning on income observability does not skew the sample much within the panel of potential gamblers.



(a) Total Spending by Income Observability

(b) Net Gambling Spend by Income Observability

Figure 7: PDFs of total spending and gambling spending by income observability

We acknowledge that the financial panel may not fully represent the full population of consumers, and the balanced panel of potential gamblers may not fully represent the full population of online gamblers. However, given that digital payment methods are required for online gambling and given the panel's size, internal validity and passive measurement, we believe the longitudinal findings based on the potential gambler behaviors are sufficiently important to merit reporting. Further research will be required before panel-based conclusions could be confidently projected to unrepresented gamblers.

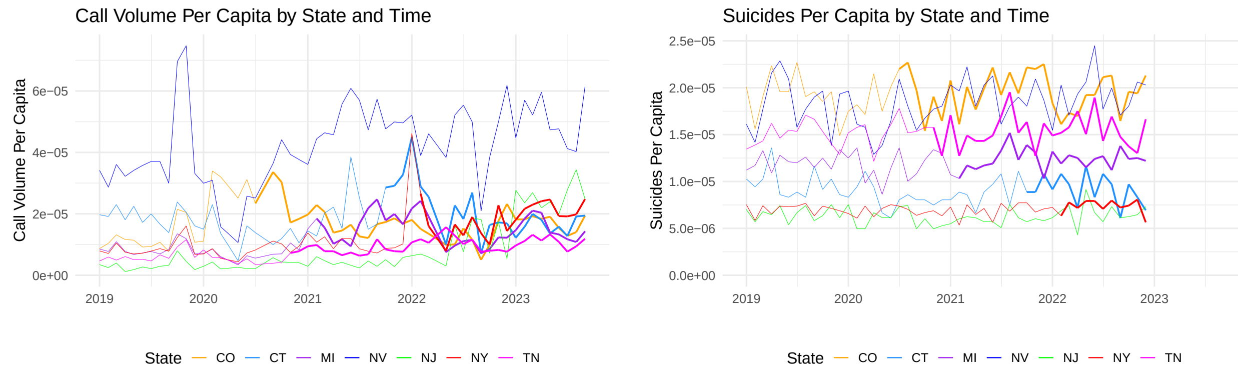
2.4 NCPG Calls and Suicide

We collected calls to NCPG gambler helplines by state and month (NCPG 2024a). Several caveats are in order. The NCPG website advises that "the data should not be used as a proxy to estimate problem gambling prevalence." Recall that problem gambling prevalence is measured in surveys or therapeutic settings based on symptoms experienced. The normative interpretation of gambler helpline calls is therefore unclear. Calls may represent a flow variable stemming from the unobserved stock variable of problem gambling prevalence; yet calls also indicate gambler attempts to access helpful resources when needed. We report policy effects on helpline calls, but their normative interpretation requires further research.

Additionally, some states operated their own gambler helplines during some parts of the sample period. For example, Michigan operated a state-specific gambler helpline until transitioning to NCPG helplines in February 2024 (Michigan Gaming Newsletter, 2024). Still, gamblers in all states are able to call NCPG helplines, and NCPG helplines were communicated in gambling advertisements throughout the sample period, including some national advertisements and promotions. Therefore, policy estimates on NCPG calls can only underestimate true effect sizes. We follow NCPG staff advice to focus on calls longer than one minute.

Figure 8a displays NCPG call volume per capita by month for the same states shown in Figure 1a. NCPG received an average 90 calls per state per month. The New York calls graph shows a four-fold spike at the time of OSB legalization, but the other six states do not show a strong correspondence of calls to OSB policy timing.

We analyze suicide data from the Centers for Disease Control (CDC), which aggregates individual records produced by state coroners offices. Gambling-related suicides are fundamentally difficult to measure accurately since coroners do not typically observe decedents' pre-death gambling behaviors or finances (Sorenson, 2024). Prior research has estimated associations between gambling behaviors and suicidal ideation and actions (e.g., Jolly et al., 2021; Wardle et al., 2023). Qualitative studies identify indebtedness and shame as potential causal mechanisms (Marionneau and Nikkinen, 2022). Rintoul et al. (2023) analyzed contributing factors in suicide case notes written by decedents, witnesses, police and coroners, finding 4% of suicides to be gambling-related. However, there have been limited longitudinal studies (Kristensen et al., 2024). Causal relation-



(a) (b)

Figure 8: Gambler Helpline Calls and Suicides Per Capita for selected states

Thin lines represent before legalization (or control states) and thick lines represent after legalization.

ships are difficult to disentangle from shared psychological risk factors like depression and impulsivity, or shared population-level risk factors such as gender and firearm access (Andreeva et al., 2022).

The potential links and topical importance motivated us to investigate whether state gambling policies changed suicides per capita, but the analysis faces several challenges. Most importantly, CDC data did not enable direct measurement of gambling-related suicides.[12] If the proportion of gambling-related suicides is around 4% (Rintoul et al., 2023), it may be difficult to detect a policy effect in population-level suicide data without more specific measurements. Additionally, some gambling-related suicides may occur without legalized gambling, as illegal gambling may occur without gambling legalization. Finally, gambling legalization may reduce suicidality for some gamblers (e.g., by making gambling assistance resources more easily accessible) which could offset an increased propensity among other gamblers, possibly leading to an undetectable net effect.

Figure 8b illustrates mean suicides per capita for the same selected states as before. The data show large differences in per-capita suicide rates between states, and also large variability over time within states. However, none of the time series show unusual increases that visually correspond to OSB policy change dates.[13]

[12]Specifically, none of the CDC suicide records indicate the "F63.0 Pathological Gambling" or "Z72.6 Gambling and betting excl compulsive or pathological gambling" codes in the International Classification of Diseases (10th edition).

[13]CDC had not yet released suicide data past December 2022, so we could not estimate a policy effect for Ohio, which enacted OSB in January 2023.

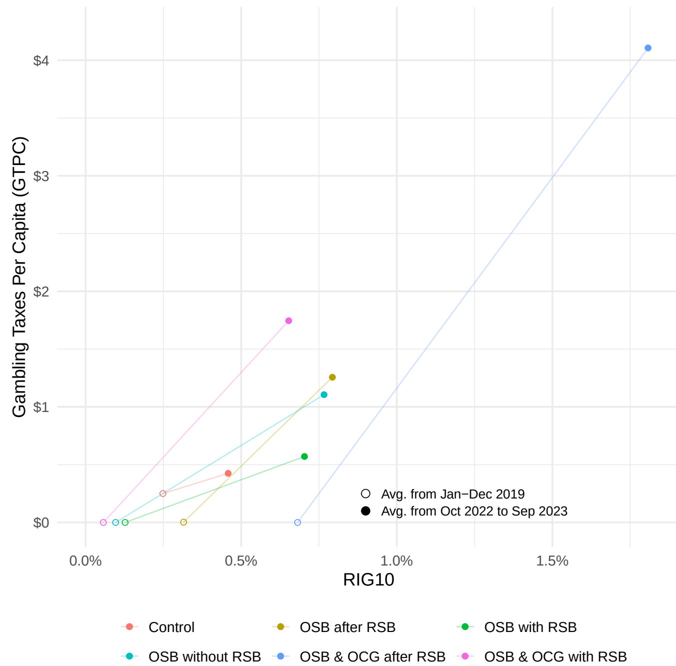
2.5 Suggestive Variation Across State Treatment Groups

Figure 9 transforms the data to provide suggestive evidence and to illustrate the identifying variation prior to model estimation. Panel 9a shows state groups by mean gambling tax revenue per capita and mean RIG10. The open circles show the state group means in the first 12 months of the sample period (Jan-Dec 2019) and the closed circles show the averages in the final 12 months of the sample (Oct 2022-Sep 2023). The figure shows that control states' mean tax per capita increased from $0.25 to $0.43. All five treatment groups increased tax revenue substantially more, with the largest changes in the two treatment states that introduced OCG. It also shows that control states' mean RIG10 increased from 0.25% to 0.46%, a rate of change that approximates the rate of tax increase. All five treatment groups increased tax per capita and RIG10 substantially more than the control group, with the largest changes in both variables in the two treatment states that introduced OCG.

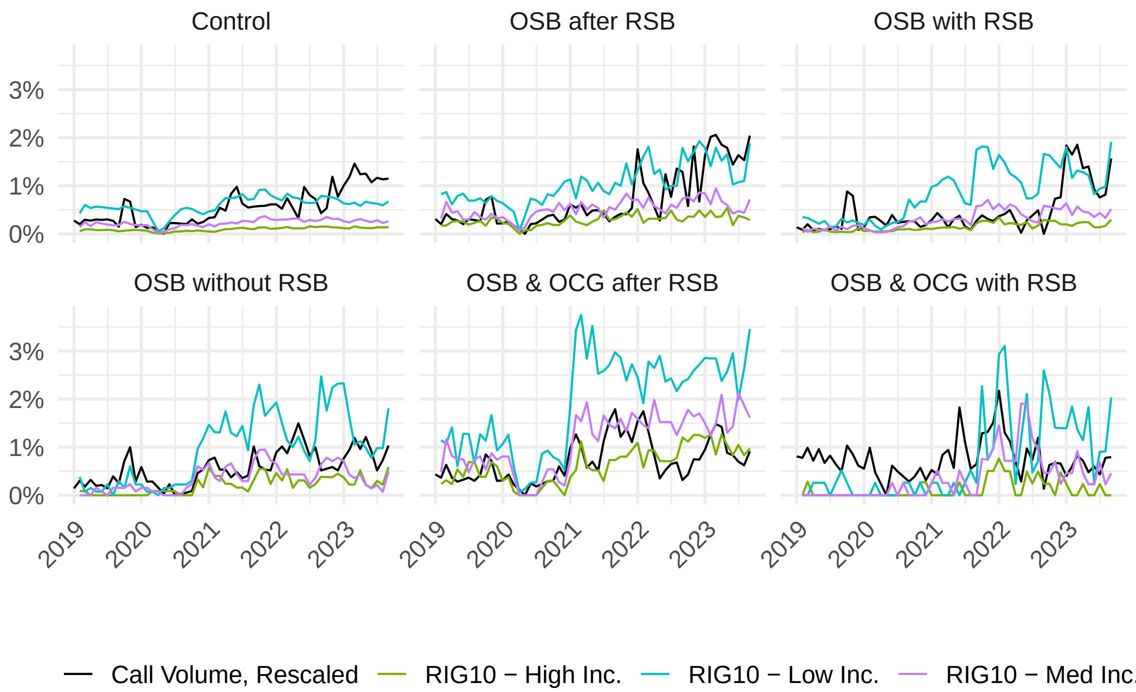
Figure 9b plots RIG10 by income tercile and NCPG calls (rescaled to fit) across months within each state group. Overall, RIG10 decreases across income terciles. A correspondence between NCPG calls and RIG10-Low is visually evident: NCPG calls correlate across all state/months with RIG1 at .158, with RIG5 at .112, with RIG10 at .090 and with RIG15 at .078.

NCPG calls and RIG10-Low both trended up in control states, and trended up much more in all state treatment groups. The changes in RIG10-Low are largest in the two state treatment groups that introduced OCG. The changes in RIG10-Low correspond strongly to times of treatment. RIG10-Medium and RIG10-High also respond to treatment timing, but with smaller proportional increases. Overall, NCPG calls are less responsive to policy changes than RIG10 measures.

Overall, these descriptives suggest substantial correspondence between gambling policy changes and response indicators, with larger correspondence in treatment state groups than control states, and especially large correspondence in OCG treatment groups. Descriptive interpretations do not condition on opaque modeling choices, but they are noisy, they do not isolate treatment effects from other changing factors, and they do not enable statistical inference. Therefore the next section specifies an empirical model to estimate principled counterfactual predictions of what gambling outcomes would have obtained in the absence of gambling policy changes, in order to estimate quasi-experimental treatment effects.



(a) Taxes and RIG10 by State Groups and Time



(b) NCPG Calls and RIG10-Income Terciles by State Groups and Time

Figure 9: Suggestive Variation by Treatment Group

3 Model and Estimation

The fundamental problem of causal inference is to predict counterfactual outcomes, i.e. what unobserved post-treatment outcomes would have occurred without treatment. In our setting, this means we want to predict what gambling-related outcomes would have occurred in legalizing states after policy enactment, if the legalizing state had not enacted online gambling. A few gambling-related variables have known counterfactual levels of zero, such as total tax revenue from online gambling. Therefore, we can estimate some policy effects directly as the observed means in treated units in treated periods. However, most variables have time-varying, non-zero baseline levels, such as gambling behaviors, NCPG calls, and suicides. In particular, gambling behaviors have nonzero counterfactuals primarily because the panel data do not distinguish DFS payments from OSB or OCG payments within the same merchants (e.g., DraftKings, FanDuel).

We use a Generalized Synthetic Control (GSC) approach to estimate unobserved counterfactual outcomes. The intuition for the GSC approach is that it proceeds in three steps. First, it uses the full panel of 18 control states to estimate time-varying latent factors specific to each outcome measure, as well as unit and time effects. Next, it uses the pre-treatment period for each treated state to estimate state-specific weights on each of the latent factors. Finally, it combines the post-treatment latent factors with the state-specific weights to predict each treated state's post-treatment counterfactual outcome.

GSC offers a number of attractive properties, such as nesting the popular synthetic control and two-way fixed effects estimators as special cases. Still, a plausible alternative to GSC is staggered difference-in-differences (e.g., Sun and Abraham, 2021; Callaway and Sant'Anna, 2021). Two theoretical properties recommend the GSC approach. First, GSC uses pre-treatment data to estimate relationships between treated units and control units, rather than assuming all untreated units are equally valid controls for all treated units. Second, GSC avoids using any pre-treatment observation as a control for any treated observation, which we view as more conservative. Section 4.2 shows that the estimates are mostly, though not entirely, reproduced by alternate estimators.

We adopt the interactive fixed effects model of Bai (2009), the estimation algorithm of Xu (2017), and the estimation refinements and inference theory of Li and Sonnier (2023). More specifically, for each state i, month t and treatment j, we model the outcome Y_{it} as follows:

$$Y_{it} = \delta_{it}^{j} D_{it}^{j} + \lambda_i' F_t + \mu_i + \mu_t + e_{it}$$

Here, D_{it}^{j} is a binary treatment indicator which equals 0 in all periods for untreated states, and in all pre-treatment periods for treated states; and equals 1 for all post-treatment periods for all treated states. δ_{it}^{j} is the effect of treatment j on state i in month t. F_t is a vector of time-varying latent factors that are common to all states, and λ_i is a state-specific vector of factor loadings. μ_i and μ_t are state- and time-specific fixed effects. This model formulation admits standard synthetic control, difference-in-differences and two-way fixed effects estimators as special cases.

The estimation algorithm proceeds in three steps (Bai, 2009; Xu, 2017; Li and Sonnier, 2023):

1. Use all control state data to estimate r common latent factors $\hat{F}_t$, $\hat{\lambda}_t$, and control states' $\hat{\lambda}_i$ and $\hat{\mu}_i$, including a cross-validation procedure to choose r
2. For each treated state i, use pre-treatment Y_{it}, $\hat{F}_t$ and $\hat{\mu}_t$ to estimate treated state's $\hat{\lambda}_i$ and $\hat{\mu}_i$
3. Infer treatment effects as the difference between post-treatment outcomes and the model's post-treatment predictions for treated units: $\delta_{it}^{j} = Y_{it} - \hat{\lambda}_i' \hat{F}_t - \hat{\mu}_i - \hat{\mu}_t$

We do this for each treatment j and each outcome measure Y.

4 Empirical Results

We present and interpret the treatment group-specific estimates in the text. The Appendix contains more detailed state-specific policy estimates and dynamic treatment effect estimates for key variables.

4.1 Findings

Figure 10 displays per-capita average treatment-on-the-treated (ATT) estimates of online gambling policies on public metrics and retail casino spend; whiskers indicate 95% confidence intervals. The top-left panel of Figure 10 shows that online gambling policies increased OSB handle per capita by \$36-45, with mostly overlapping confidence intervals. The only statistically significant differences between ATT estimates were for the two states that legalized OCG along with OSB, leading to significantly less OSB handle per capita than OSB-with-RSB states. Smaller OSB handle per capita in OSB & OCG states suggests that online casino gambling may be a net substitute for online sports betting.

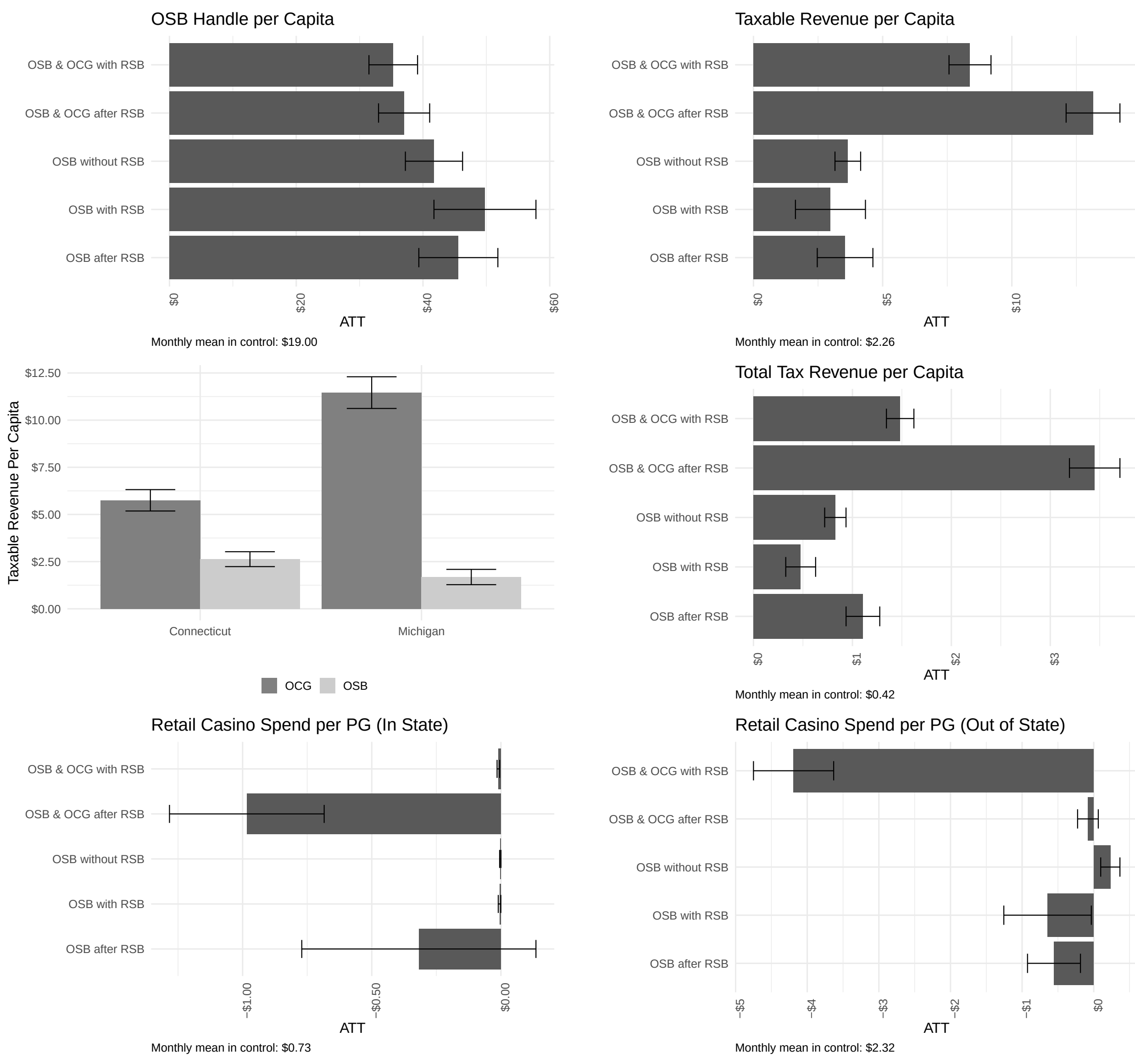


Figure 10: ATT estimates on public gambling metrics and retail casino spend

We do not find significant differences in treatment effects on OSB handle per capita among the OSB-without-OCG states, suggesting that RSB availability before OSB may not have a first-order effect on OSB betting. It may be that RSB "warms up" gamblers who then spend more on OSB; or it could be that early RSB adopters are less likely to adopt OSB. However, the results do not indicate that either potential mechanism dominates the other.

OSB-only policies did not differ significantly in their effects on operator taxable revenue per capita, bringing in about $3-4 per person per month (bottom three bars in top-right panel of Figure 10). However, policies that included OCG legalization yielded significantly more monthly revenue, at about $8 per person in Connecticut and $16 per person in Michigan, which may partially explain the gambling industry's continued push to legalize OCG in more states.

How much of operator taxable revenue increases in Connecticut and Michigan came from OSB vs. OCG? Connecticut generated about $2.50 per person of OSB taxable revenue, comparable to most non-OCG states, and slightly more than double in OCG revenue per person. Michigan, on the other hand, generated less OSB revenue per person than most comparable states (about $1.40), and significantly more OCG revenue at more than $11 per person. The disparity between Connecticut and Michigan suggest that a state's OSB activity per capita may not reliably predict its likely OCG activity per capita.

Monthly total tax revenue per capita was also greatest in OCG-legalizing states, at $1.50 in Connecticut and $3.50 in Michigan. OSB-only states generated about $0.50-$1.10 per person per month, differences which are partly driven by disparate taxation rates on operator revenue. Overall, total online gambling tax revenue is modest compared to annual total state and local tax collected per capita, which ranged from $4,192 in Alaska to $10,266 in New York in 2021 (Tax Foundation, 2023). The largest proportional contribution of gambling taxes to state revenue occurred in Michigan, where online gambling taxes generated 0.8% of annual state and local tax revenue.

Next, we examine how online gambling policies changed retail casino spend in-state and out-of-state. Two treatment groups legalized retail sports books prior to online gambling policy changes; online gambling policy reduced in-state retail casino spend significantly in Michigan, with a point estimate of about $1 per person, less than 10% of the state's ATT on online gambling operator taxable revenue. The effect in the OSB-after-RSB group did not differ significantly from zero. In terms of out-of-state retail casino spend, Connecticut's policy reduced out-of-state retail casino spend by about $4.20 per person, which was about half of the state's ATT estimate on operator taxable revenue. The OSB-with-RSB and OSB-after-RSB treatments have small but significant negative ATTs on out-of-state retail casino spend, with similar point estimates of about

$0.60 and $0.70 per person. Interestingly, Tennessee residents actually spent slightly more per person on out-of-state retail casino merchants, which is plausible given that Tennessee legalized online gambling but has no retail casinos in any form.

Figure 11 shows how online gambling policies changed monthly rates of gambler behaviors by state treatment group in the gambler panel: gamblers, new gamblers, regulars (defined as at least 6 consecutive months with gambling transactions), and accelerators (defined as at least 50% increases in at least two consecutive months after at least $100 net gambling spend in the previous month). All five gambling policy types significantly increased all four measures of gambler activity relative to the control states. These increases are over and above the control states' upward-trending gambling and DFS activity.

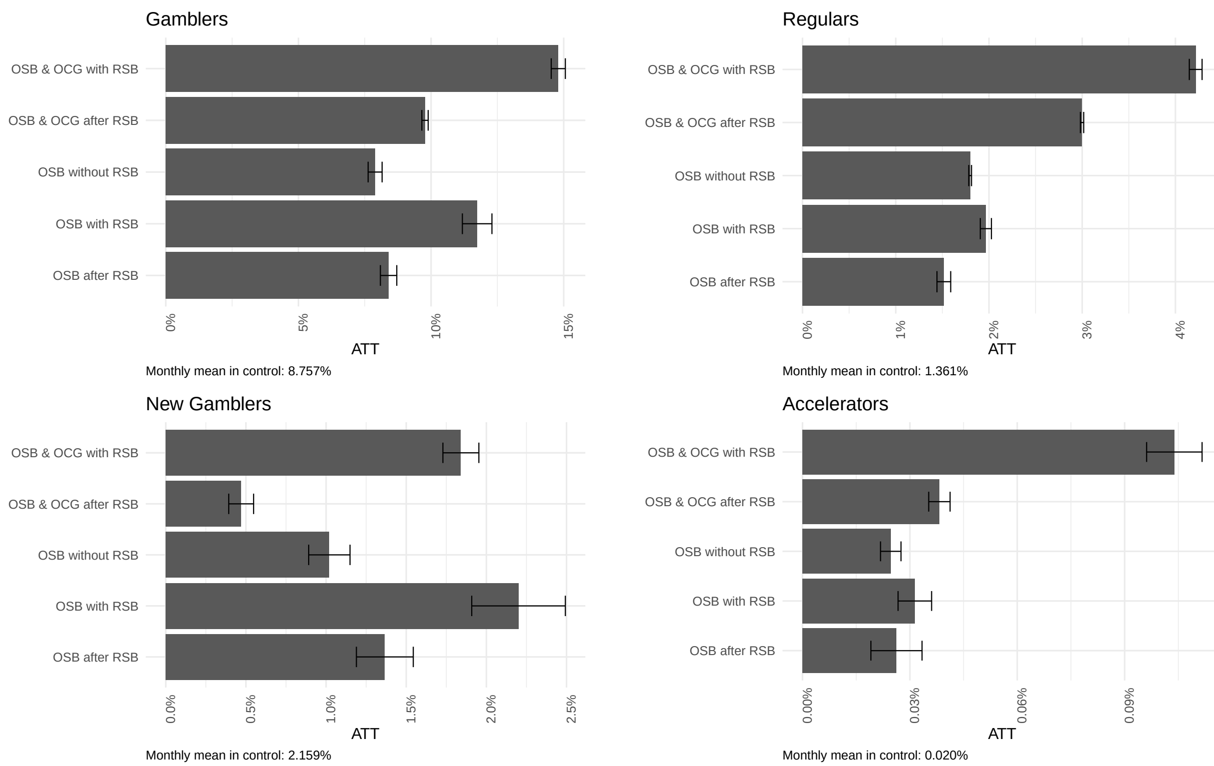


Figure 11: ATT estimates on gambler activities per potential gambler

Online gambling policies increased monthly gambling participation among potential gamblers by 7-15%, with the largest proportional increase in Connecticut. The six states that legalized online sports betting alongside retail sports betting saw a 4% larger treatment effect than those that legalized retail sports betting first, a difference consistent with the potential substitutability between online and offline sports betting, and suggestive that online sports betting may be cannibalizing some gambling activity from retail channels.

Treatment effects on new gamblers varied more across state treatment groups. Michigan increased new gamblers the least, by about 0.5% per month, whereas the group of six states that simultaneously legalized online and retail sports betting increased new gamblers by more than 2% on average. States that enacted retail sports betting prior to online sports betting had smaller OSB treatment effects on new gamblers, again consistent with the potential substitution between the two modalities.

Looking at regular gamblers, Connecticut's policy increased the proportion of gamblers who transacted for at least 6 consecutive months ("regulars"), with about 5.2% of potential gamblers becoming monthly regulars. This was followed by 3.3% in Michigan, 2.7% in the OSB-after-RSB group, 2.4% in the OSB-with-RSB group, and 2.0% in Tennessee (OSB-without-RSB). It is notable that the two states with online casino gaming treatments both increased regular gamblers the most.

The OCG states also saw the largest increases in accelerators after legalization, which we define as gamblers with net gambling spend of at least $100 in one month, followed by increases of 50% or more in at least two consecutive months. The treatment effects on monthly accelerators are much lower than on regulars, at about 0.3% in CT and 0.14% in MI, but both are large relative to the monthly average in control states (.02%).

Figure 12 shows how online gambling policies changed irresponsible gambling metrics, helpline calls and suicides. Treatment effects on RIG1 range from 2.2-4.3%, quite large relative to the control states average of 2.5%. Recall that RIG*x* metrics include non-gambling months, so these increases are not limited to observations of nonzero net gambling spend. Recall also that some gamblers in treatment states were spending over 1% of income on DFS prior to treatment. Therefore, these ATTs should be interpreted as increases in RIG*x* metrics above baseline rates in control states, not simply as post-treatment averages.

OCG policies increased RIG1 and RIG5 significantly more than non-OCG policies. OCG effects on RIG1 were about 3.8% and 4.2% for MI and CT, respectively, and treatment effects on RIG5 were about 1.3% and 1.6% in the same two states. Meanwhile, non-OCG policy effects on RIG1 ranged from 2.2-3.2%, and on RIG5 from 0.6-1.0%.

Michigan's policy effects on RIG10 and RIG15 remained high relative to non-OCG policies, at

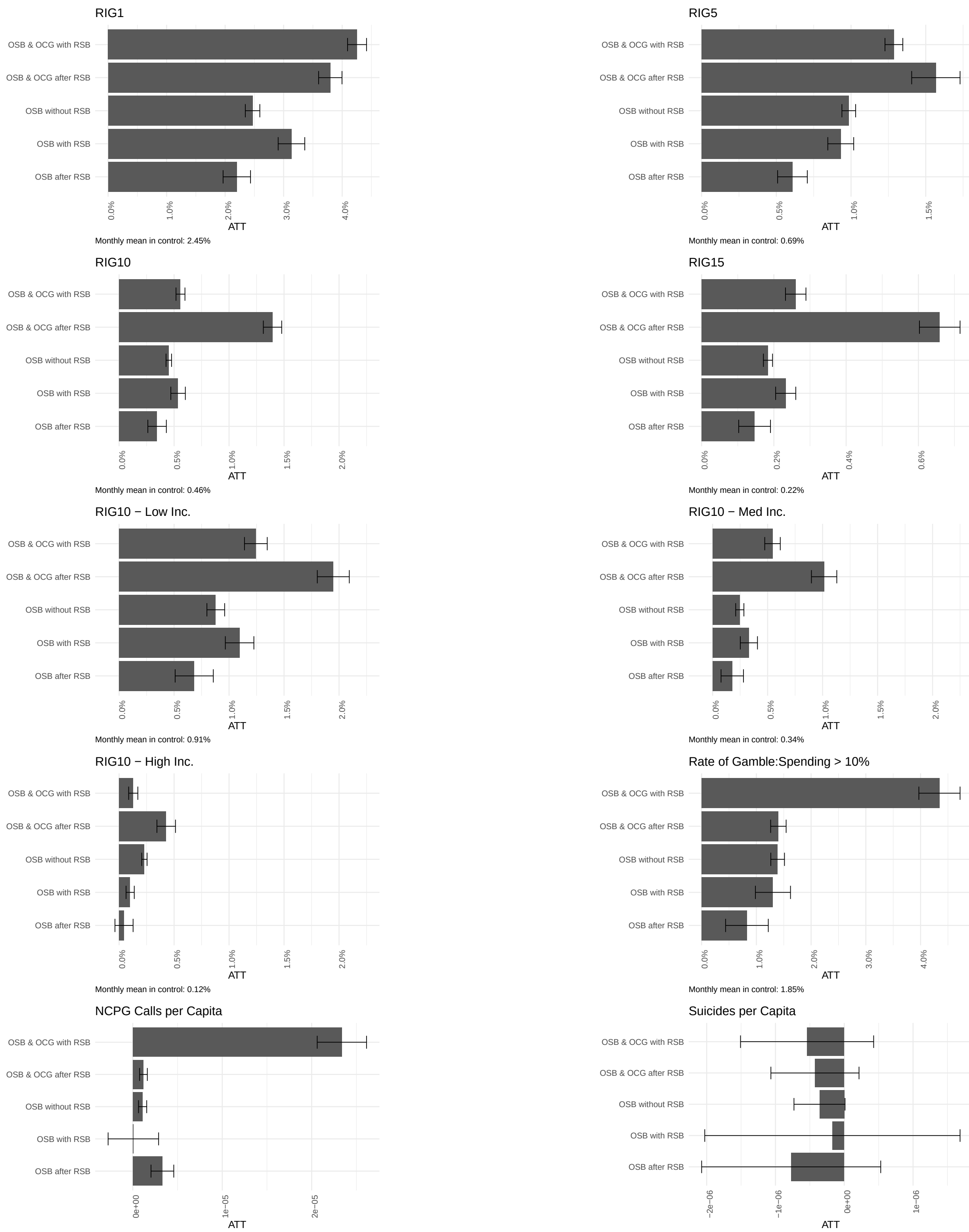


Figure 12: ATT estimates on RIG*x* metrics, helpline calls and suicides

1.4% and 0.7% of monthly potential gamblers, whereas Connecticut's treatment effects on RIG10 and RIG15 were closer to the non-OCG policy effects. Connecticut's effect on RIG10 was not statistically distinguishable from the OSB-with-RSB group estimates, increasing RIG10 by about 0.6% and RIG15 by about 0.3%.

Next we look at ATTs on RIG10 by terciles of the income distribution. Overall, policy effects on irresponsible gambling–across all five policy types–are much larger within the lowest income tercile, followed by the middle tercile, and smallest in the highest income bracket. Rates of irresponsible gambling above 10% of income increase around 5 times more in the lowest income tercile compared to the highest income tercile.

We also estimate policy effects rates of gamble:spending ratios above 10% as a robustness check.[14] Here again, OCG policies increase high net gambling proportions more than non-OCG policies. Michigan's policy effect on Gamble:Spending 10% is not significantly different from its policy effect on RIG10, with both at about 1.5%. However, CT's policy effect on Gamble:Spending 10% is much larger than its policy effect on RIG10. One possible explanation for the difference is the large difference in income distributions between the two states: Michigan's 2022 median household income was $66,986, whereas Connecticut's was $88,429 (Posey, 2023). Therefore, the tercile cutoffs were quite different between the two states.

Four out of five policy changes significantly increased NCPG gambler helpline calls, with a particularly large effect in Connecticut (OSB & OCG with RSB). Comparing Connecticut to other state-specific ATTs on NCPG helpline calls per capita, it is statistically indistinguishable from Ohio, but larger than all others. The one non-significant ATT is for Michigan, which ran its own state-specific helpline during the sample period, then adopted NCPG helplines in February 2024. Media reports indicate that calls to Michigan's helpline nearly tripled in the year after legalization compared to the year prior (Wells, 2022), though of course that increase is a descriptive change that may also reflect an increasing national trend. Still, our measure of NCPG calls could not capture the state helpline calls increase since we are only able to measure NCPG helpline calls.

Finally, policy effect point estimates on suicides are negative with confidence intervals that include zero. The treatment effects on suicide vary in their statistical power. The most diffuse

[14] We used a random subsample of 53,132 gamblers due to high computational costs of summing total spending within each gambler/month.

estimate (OSB with RSB) has a confidence interval that rules out a positive treatment effect of 16% of the control state mean suicide rate, while the narrowest confidence interval rules out a 2% increase in suicides compared to the mean rate in control states. To the best of our knowledge, this is the first causal evidence regarding how gambling policies change aggregate suicide rates, though it may be viewed as inconclusive. From one perspective, the analysis is as highly powered as possible, given that we consider the population of suicides in these 32 treatment and control states across nearly 5 years. On the other hand, the substantial variation in suicide rates within places across time makes it difficult to pin down a precise point estimate, and the confidence intervals are diffuse so we do not interpret them as precise nulls.[15]

4.2 Robustness checks

We report several different types of robustness checks. We relied on visual inspection to check for parallel pretrends, as is customary in GSC estimation (Li and Sonnier, 2023). Figures OA3, OA4, and OA5 show state-specific dynamic treatment effect estimates for two outcome variables, RIG10 and gamblers per potential gambler, which we thought to be particularly important in the analysis. In general the dynamic treatment effect graphs show that pre-treatment treatment/control confidence intervals include zero, significant spikes occur shortly after legalization, and state-specific estimates show heterogeneous patterns over time after legalization.

We sought to evaluate robustness by changing the observed times of treatment, a practice known as "placebo treatments." For each treated state, we created an artificial time of treatment by reducing that state's pre-treatment period by 20% of the time between sample start and actual time of treatment. Intuitively, a 95% confidence interval for the estimated treatment effect should be indistinguishable from zero 95% of the time since no actual treatment occurred. Empirically, we find this to be true in approximately 98% of the 224 individual state-outcome treatment effect estimates.

We also sought to evaluate robustness by changing the estimator. Table 3 compares unified-treatment GSC estimates to two popular staggered difference-in-differences estimators by Sun and Abraham ("SA;" 2021) and Callaway and Sant'Anna ("CS;" 2021). The "μ Same" column reports

[15]More conclusive results would likely become available if suicides could be classified as gambling-related or not, a topic we return to when discussing research implications.

statistical tests of unequal treatment effect sizes between the GSC estimator and each alternate estimator. 11 of 14 outcome variables reject the null hypotheses of unequal treatment estimates between both pairs of estimators (GSC/SA and GSC/CS) at the 95% confidence level. Three outcome variables reject the null hypotheses of unequal treatment estimates in exactly one of the two pairs. In all cases where the estimates are unequal, all three estimates are significantly different from zero in the same direction. Therefore, even the rare differences can be interpreted as differences in effect magnitudes rather than qualitative differences in findings.

Overall, we conclude that the empirical findings are reasonably robust to minor changes in the estimation methodology.

Table 3: Estimator comparison

Outcome Variable	**GSC**	**SA**	**CS**	**μ Same**
RIG1	0.0288 (0.0011)	0.0276 (0.0027)	0.0282 (0.0019)	Both
RIG5	0.0089 (0.0005)	0.0086 (0.0013)	0.0085 (0.0009)	Both
RIG10	0.0053 (0.0004)	0.0053 (0.0009)	0.0053 (0.0006)	Both
RIG15	0.0023 (0.0002)	0.0028 (0.0005)	0.0028 (0.0004)	Both
RIG10 - Low Inc.	0.0100 (0.0007)	0.0097 (0.0014)	0.0099 (0.0010)	Both
RIG10 - Med Inc.	0.0033 (0.0004)	0.0041 (0.0010)	0.0040 (0.0007)	Both
RIG10 - High Inc.	0.0012 (0.0003)	0.0021 (0.0006)	0.0018 (0.0005)	Both
Rate of Gamble:Spending >10%	0.0136 (0.0017)	0.0140 (0.0027)	0.0127 (0.0020)	Both
New Gamblers	0.0167 (0.0011)	0.0091 (0.0020)	0.0123 (0.0022)	CS
Gamblers	0.1035 (0.0022)	0.0860 (0.0085)	0.0930 (0.0084)	CS
NCPG Calls per Capita x1000	0.0030 (0.0011)	0.0017 (0.0027)	0.0024 (0.0019)	Both
Suicides per Capita x1000	-0.0005 (0.0007)	-0.0013 (0.0008)	-0.0012 (0.0007)	Both
Regulars	0.0203 (0.0003)	0.0196 (0.0026)	0.0188 (0.0020)	Both
Accelerators x1000	0.3474 (0.0294)	0.3888 (0.0446)	0.3983 (0.0461)	SA

"Outcome Variable" column represents the dependent variables of interest from Section 4.1. "GSC" represents the point estimates (standard errors) of the proposed Generalized Synthetic Control method. "SA" and "CS" represent the corresponding results of the staggered diff-in-diff estimators by Sun and Abraham (2021) and Callaway and Sant'Anna (2021), respectively. "μ Same" denotes whether alternate estimators (either "SA", "CS", or "Both") rejects the null hypothesis of a treatment effect size unequal to the GSC estimate at the 95% confidence level.

5 Discussion

We present an early evaluation of online gambling policy changes. We estimate causal policy effects on outcomes including OSB handle, operator taxable revenue, and total tax revenue; gambler adoption, participation, regularity, and spending acceleration; a range of new irresponsible gambling metrics; gambler helpline calls and suicides. Overall, policies that include online casino gaming tend to have larger effects on both tax revenues and markers of concerning gambling behaviors, though not all markers. They have especially large effects on irresponsible gambling by low-income and middle-income gamblers. Gambling policies increased gambler helpline calls, but we do not find significant effects on suicides.

5.1 Policy Implications

The clearest and most important implication for policymakers applies to states that have not adopted online casino gaming. OCG caused more irresponsible gambling than OSB-only policies, especially among low-income gamblers. Therefore, we think that policymakers who are considering OCG policies may also wish to consider strengthening consumer safeguards and allocating more resources for problem gambler helplines. This implication applies both to states that currently allow OSB, and to states that may yet consider implementing online gambling policies.

This paper's findings could also have many more nuanced implications for policymakers. Each state policy is composed of multiple granular choices, such as the number and types of gambling licensees and operators; licensing fees; allowable wagers (e.g., parlays, college sports); tax rates and deductions (e.g., gambling promotions); resources available for problem gambling; advertising and promotion policies (e.g., gambler helpline notifications, promotional bet offers); availability of self-limiting user controls; permissibility of various data-driven marketing tactics such as targeting or demarketing (e.g., Blumenthal, 2024); and regulatory oversight and public reporting requirements, among other elements. Policymakers within individual states may wish to compare state outcomes to pre-policy expectations or to other states' outcomes. States that experienced more irresponsible gambling than desired could use these results to identify which states experienced less irresponsible gambling, and thereby help to identify modifiable policy elements that might help to improve outcomes. A similar logic might hold for states whose policies generated less tax revenue than expected.

Finally, we encourage policymakers to directly measure potential health-related gambling harms. Those would include better measurement of gambling-related suicides, but also less extreme public health-related outcomes that may relate to gambling, such as divorce, bankruptcy, mental health problems, etc. Better measures are needed for a more complete accounting of gambling policy effects on society and to inform possible future policy refinements.

5.2 Limitations and Extensions

Like all research, our work has prominent limitations that suggest directions for future research. First, we do not directly observe retail gambling or illegal gambling. Therefore, we have limited insights into how much of the policy effects on online gambling represent new activity as opposed to shifts from unobserved existing channels. We do not know of wide-scale passive measures that we can access, but state regulators might collect retail gambling data from operators, and surveys might be able to measure trends in illegal gambling.

Second, we have relied on previous literature's longitudinal findings to link high *Gamble%* to potential gambling harms, and while the financial panel dataset we analyze has good internal validity, it may not be fully representative of the online gambling market. Gambling regulators might productively collect customer data from gambling operators, including either self-reported income, or combining operator data with third-party sources like income tax records. Such data could show how irresponsible gambling changes across time, customer types, sports seasons, casino games, or with operator promotions. It also may help to enable proactive interventions if individual gamblers show early signs of potential trouble.

Finally, the analysis herein is aggregated at the state/month level. Richer analyses may be possible at the individual level. For example, can non-gambling spending data predict future problem gambling? Could gambling operators productively engage in demarketing to 'cool down' potentially disordered gamblers before the gamblers quit gambling entirely? How do individual gambling problems affect operators' long-term revenues? These are empirical questions, as an addicted gambler may be especially profitable for the operator in the short run, but then may quit permanently and hence end the long-run stream of profits. It would be interesting to know how much operator and regulator incentives align or diverge, as it could suggest the degree to which regulators may trust gambling operators to self-regulate.

To conclude, we hope this study contributes to our collective understanding of desirable and undesirable online gambling policy effects. We have intended a dispassionate analysis of recent policy changes to help inform policymakers as they refine and improve policy. We hope that future policy efforts may benefit from similar examinations, and that future research will build further on what we have offered here.

Acknowledgements: For helpful comments, we thank *Legal Sports Report*, the anonymous panel data provider, NCPG staff, Tat Chan, Kathy Li, Eric Ramsey, and seminar participants at Bocconi, HKUST, Indiana, Northwestern, Santa Clara, SMU, UT Austin, the Theory+Practice in Marketing Conference, and UCSD. Any mistakes are ours alone.

References

Hunt Allcott, Matthew Gentzkow, and Lena Song. Digital addiction. *American Economic Review*, 112(7):2424–2463, 2022.

American Gaming Association. Sizing the illegal and unregulated gaming markets in the united states, Nov. 2022. URL https://www.americangaming.org/wp-content/uploads/2022/11/Sizing-the-Illegal-and-Unregulated-Gaming-Markets-in-the-US.pdf. [Online; accessed 30. Apr. 2024].

American Gaming Association. American Attitudes Towards Gaming 2023, October 2023. URL https://www.americangaming.org/wp-content/uploads/2023/10/AmericanAttitudes2023_FINAL.pdf. [Online; accessed 17. Apr. 2024].

American Gaming Association. Interactive U.S. Map: Sports Betting - American Gaming Association, March 2024a. URL https://www.americangaming.org/research/state-gaming-map-mobile. [Online; accessed 16. Apr. 2024].

American Gaming Association. Interactive U.S. Map: Sports Betting - American Gaming Association, March 2024b. URL https://www.americangaming.org/research/state-gaming-map-mobile. [Online; accessed 17. Apr. 2024].

American Psychiatric Association. What is Gambling Disorder?, April 2024. URL https://www.psychiatry.org/patients-families/gambling-disorder/what-is-gambling-disorder. [Online; accessed 16. Apr. 2024].

Mariya Andreeva, Sophie Audette-Chapdelaine, and Magaly Brodeur. Gambling-Related completed suicides: a scoping review. *Addiction Research & Theory*, November 2022. ISSN 1606-6359. doi: 10.1080/16066359.2022.2055001.

Jushan Bai. Panel Data Models With Interactive Fixed Effects. *Econometrica*, 77(4):1229–1279, 2009. doi: 10.3982/ECTA6135.

Gary S Becker and Kevin M Murphy. A theory of rational addiction. *Journal of political Economy*, 96(4):675–700, 1988.

Richard Blumenthal. Ahead of March Madness, Blumenthal Demands Sports Betting Companies Stop Targeting Problem Gamblers | U.S. Senator Richard Blumenthal of Connecticut, 2024. URL https://www.blumenthal.senate.gov/newsroom/press/release/ahead-of-march-madness-blumenthal-demands-sports-betting-companies-stop-targeting-problem-gamblers. [Online; accessed 27. May 2024].

Brantly Callaway and Pedro H. C. Sant'Anna. Difference-in-Differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230, December 2021. doi: 10.1016/j.jeconom.2020.12.001.

Canadian Centre on Substance Use and Addiction. Developing lower-risk gambling guidelines, 2021. URL https://gamblingguidelines.ca/app/uploads/2023/06/LRGG-Developing-Lower-Risk-Gambling-Guidelines-Report-2021-en.pdf. [Online; accessed 21. Apr. 2024].

Bruno Castelo-Branco and Puneet Manchanda. Is Video Gaming Addictive?: An Empirical Analysis, June 2023. [Online; accessed 21. Apr. 2024].

Data.AI. State of Mobile 2024 - data.ai, April 2024. URL https://www.data.ai/en/go/state-of-mobile-2024. [Online; accessed 15. Apr. 2024].

DraftKings. Why do I need to download/install the GeoComply Player Location Check plugin? (US), 2024. URL https://help.draftkings.com/hc/en-us/articles/4405232242323-Why-do-I-need-to-download-install-the-GeoComply-Player-Location-Check-plugin-US. [Online; accessed 29. May 2024].

Michael Egerer and Virve Marionneau. Blocking measures against offshore online gambling: a scoping review. *International Gambling Studies*, 2024. ISSN 1445-9795. doi: 10.1080/14459795.2023.2190372.

FanDuel. Where can I download the FanDuel GeoComply plugin?, 2023. URL https://support.fanduel.com/s/article/Where-Can-I-Download-the-GeoComply-Plugin. [Online; accessed 29. May 2024].

Jackie Ferris and Harold Wynne. The canadian problem gambling index: Final report, 2001. URL https://www.greo.ca/Modules/EvidenceCentre/files/Ferris%20et%20al(2001)The_Canadian_Problem_Gambling_Index.pdf. [Online; accessed 21. Apr. 2024].

Robert Fletcher. DraftKings and Flutter stock falls on proposed Illinois tax hike. *iGamingBusiness*, 2024. URL https://igamingbusiness.com/finance/draftkings-flutter-stock-falls-on-proposed-illinois-tax-hike.

Jessica Fong, Tong Guo, and Anita Rao. Debunking Misinformation About Consumer Products: Effects on Beliefs and Purchase Behavior. *Journal of Marketing Research*, 2022. doi: 10.1177/00222437221147088.

Danny Funt. Sportsbooks say you can win big. Then they try to limit winners. *Washington Post*, 2022. URL https://www.washingtonpost.com/sports/2022/11/17/betting-limits-draft-kings-betmgm-caesars-circa.

Dana Gentry. State gambling regulators ask feds for help combating illegal offshore betting • Nevada Current, 2023. URL https://nevadacurrent.com/briefs/state-gambling-regulators-ask-feds-for-help-combating-illegal-offshore-betting. [Online; accessed 29. May 2024].

Taranjeet Jolly, Chintan Trivedi, Mahwish Adnan, Zeeshan Mansuri, and Vishesh Agarwal. Gambling in patients with major depressive disorder is associated with an elevated risk of Suicide: Insights from 12-years of Nationwide inpatient sample data. *Addictive Behavior*, 118, July 2021. doi: 10.1016/j.addbeh.2021.106872.

Jakob Jonsson, David C. Hodgins, Axel Lyckberg, Shawn Currie, Matthew M. Young, Ståle Pallesen, and Per Carlbring. In search of lower risk gambling levels using behavioral data from a gambling monopolist. *Journal of Behavioral Addictions*, 11(3):890–899, September 2022. ISSN 2063-5303. doi: 10.1556/2006.2022.00062.

Gregory Karp and Jae Bratton. Can I Use My Credit Card for Online Gambling? *NerdWallet*, 2024. URL https://www.nerdwallet.com/article/credit-cards/use-credit-card-for-online-gambling.

Kyeongbin Kim and Daniel Minh McCarthy. Wheels to Meals: Measuring the Impact of Micromobility on Restaurant Demand. *Journal of Marketing Research*, 61(1):128–142, May 2023. ISSN 0022-2437. doi: 10.1177/00222437231179021.

Joakim Hellumbråten Kristensen, Ståle Pallesen, Jonas Bauer, Tony Leino, Mark D. Griffiths, and Eilin K. Erevik. Suicidality Among Individuals With Gambling Problems: A Meta-Analytic Literature Review, April 2024. URL https://psycnet.apa.org/fulltext/2024-35228-001.html.

Legal Sports Report. Where is Sports Betting Legal - All 50 States Covered 2024, April 2024. URL https://www.legalsportsreport.com/sportsbetting-bill-tracker. [Online; accessed 16. Apr. 2024].

Henry R. Lesieur and Sheila B. Blue. The south oaks gambling screen (sogs): A new instrument for the identification of pathological gamblers. *American Journal of Psychiatry*, 144:1184–1188, September 1987.

Kathleen T. Li and Garrett P. Sonnier. Statistical Inference for the Factor Model Approach to Estimate Causal Effects in Quasi-Experimental Settings. *Journal of Marketing Research*, 60(3): 449–472, 2023. doi: 10.1177/00222437221137533.

Virve Marionneau and Janne Nikkinen. Gambling-related suicides and suicidality: A systematic review of qualitative evidence. *Frontiers in Psychiatry*, 13:1–11, October 2022. doi: 10.3389/fpsyt.2022.980303.

Michigan Gaming Newsletter. Michigan Gaming Control Board Adopts 1-800-GAMBLER as Statewide Problem Gambling Helpline . 2024. URL https://michigangaming.com/michigan-gaming-control-board-adopts-1-800-gambler-as-statewide-problem-gambling-helpline.

Naomi Muggleton, Paula Parpart, Philip Newall, David Leake, John Gathergood, and Neil Stewart. The association between gambling and financial, social and health outcomes in big financial data. *Nat. Hum. Behav.*, 5:319–326, March 2021. ISSN 2397-3374. doi: 10.1038/s41562-020-01045-w.

Sridhar Narayanan and Puneet Manchanda. An empirical analysis of individual level casino gambling behavior. *Quantitative Marketing and Economics*, 10:27–62, 2012.

National Council on Problem Gambling. National problem gambling helpline dashboard - incoming traffic, 2024a. URL https://public.tableau.com/app/profile/national.council.on.problem.gambling.ncpg/viz/NationalProblemGamblingHelplineDashboard-IncomingTraffic/IncomingTraffic. [Online; accessed 15. Apr. 2024].

National Council on Problem Gambling. FAQs: What is Problem Gambling? - National Council on Problem Gambling, March 2024b. URL https://www.ncpgambling.org/help-treatment/faqs-what-is-problem-gambling. [Online; accessed 16. Apr. 2024].

New York State. Governor Hochul Announces Nearly $2 Billion in Wagers Over the First 30 Days of Mobile Sports Wagering, April 2024. URL https://www.governor.ny.gov/news/governor-hochul-announces-nearly-2-billion-wagers-over-first-30-days-mobile-sports-wagering. [Online; accessed 24. Apr. 2024].

Hee Mok Park and Puneet Manchanda. When harry bet with sally: An empirical analysis of multiple peer effects in casino gambling behavior. *Marketing Science*, 34(2):179–194, 2015.

Kahlil S. Philander. Identifying high-risk online gamblers: a comparison of data mining procedures. *International Gambling Studies*, January 2014. ISSN 1445-9795. doi: 10.1080/14459795.2013.841721.

Robert A Pollak. Habit formation and dynamic demand functions. *Journal of political Economy*, 78(4, Part 1):745–763, 1970.

Kirby G. Posey. Household income in states and metropolitan areas: 2022, 2023. URL https://www.census.gov/content/dam/Census/library/publications/2023/acs/acsbr-017.pdf. [Online; accessed 28. May 2024].

Anita Rao and Emily Wang. Demand for "Healthy" Products: False Claims and FTC Regulation. *Journal of Marketing Research*, 54(6):968–989, 2017. doi: 10.1509/jmr.15.0398.

Angela Rintoul, Jeremy Dwyer, Ciara Millar, Lyndal Bugeja, and Huy Nguyen. Gambling-related suicide in Victoria, Australia: a population-based cross-sectional study. *Lancet Regional Health - Western Pacific*, 41:1–10, 2023. doi: 10.1016/j.lanwpc.2023.100903.

Stephan Seiler, Anna Tuchman, and Song Yao. The Impact of Soda Taxes: Pass-Through, Tax Avoidance, and Nutritional Effects. *Journal of Marketing Research*, 58(1):22–49, 2020. doi: 10.1177/0022243720969401.

Emily Sorenson. Debunking the Myth of Gambling-Related Suicides in the UK, 2024. URL https://www.psychreg.org/debunking-myth-gambling-related-suicides-uk. [Online; accessed 29. May 2024].

Liyang Sun and Sarah Abraham. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199, December 2021. doi: 10.1016/j.jeconom.2020.09.006.

Tax Foundation. How High Are State and Local Tax Collections in Your State?, 2023. URL https://taxfoundation.org/data/all/state/state-local-tax-collections-per-capita-fy-2021. [Online; accessed 28. May 2024].

Wayne Taylor and Anand V Bodapati. Incorporating experience quality data into crm models: The impact of gambler outcomes on casino return times. *SSRN Working Paper*, 2017. URL https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3455460.

Wayne Taylor and Jonathan Z Zhang. Reference dependent targeting: Evidence from the casino industry. *SSRN Working Paper*, 2023. URL https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4501436.

Anna E. Tuchman. Advertising and Demand for Addictive Goods: The Effects of E-Cigarette Advertising. *Marketing Science*, 38:913–1084, 2019. URL https://pubsonline.informs.org/doi/abs/10.1287/mksc.2019.1195?casa_token=6sb1nt3ejo8AAAAA:gYzwEYSXaPxh5xvAHmMYm_0MKLMtRgzS-Wydo2HKtn3vD73Ck2ZE6YeDmBFLakUl4womHqUOsfw.

UK Department for Culture, Media and Sport. Major reform of gambling laws to protect vulnerable users in smartphone era. 2023. URL https://www.gov.uk/government/news/major-reform-of-gambling-laws-to-protect-vulnerable-users-in-smartphone-era.

Daniel L. Wallach. Daily Fantasy Sports and PASPA: How to Assess Whether the State Regulation of Daily Fantasy Sports Contests Violates Federal Law. *OUP Academic*, August 2017. doi: 10.1093/oxfordhb/9780190465957.013.35.

Heather Wardle, Viktorija Kesaite, Sarah Tipping, and Sally McManus. Changes in severity of problem gambling and subsequent suicide attempts: a longitudinal survey of young adults in Great Britain, 2018–20. *Lancet Public Health*, 8(3):e217–e225, March 2023. doi: 10.1016/S2468-2667(23)00008-7.

Kate Wells. Calls to the Michigan gambling problem helpline nearly tripled last year. 2022. URL https://www.michiganpublic.org/2022-02-25/calls-to-the-michigan-gambling-problem-helpline-nearly-tripled-last-year.

Yiqing Xu. Generalized Synthetic Control Method: Causal Inference with Interactive Fixed Effects Models. *Political Analysis*, 25(1):57–76, 2017. doi: 10.1017/pan.2016.2.

Matthew M Young, David C Hodgins, Shawn R Currie, Natacha Brunelle, Magali Dufour, Marie-Claire Flores-Pajot, and Louise Nadeau. Not too much, not too often, and not too many: The results of the first large-scale, international project to develop lower-risk gambling guidelines. *International Journal of Mental Health and Addiction*, 22(1):666–684, 2024.

OA Appendix

OA.1 State-specific treatment effect estimates

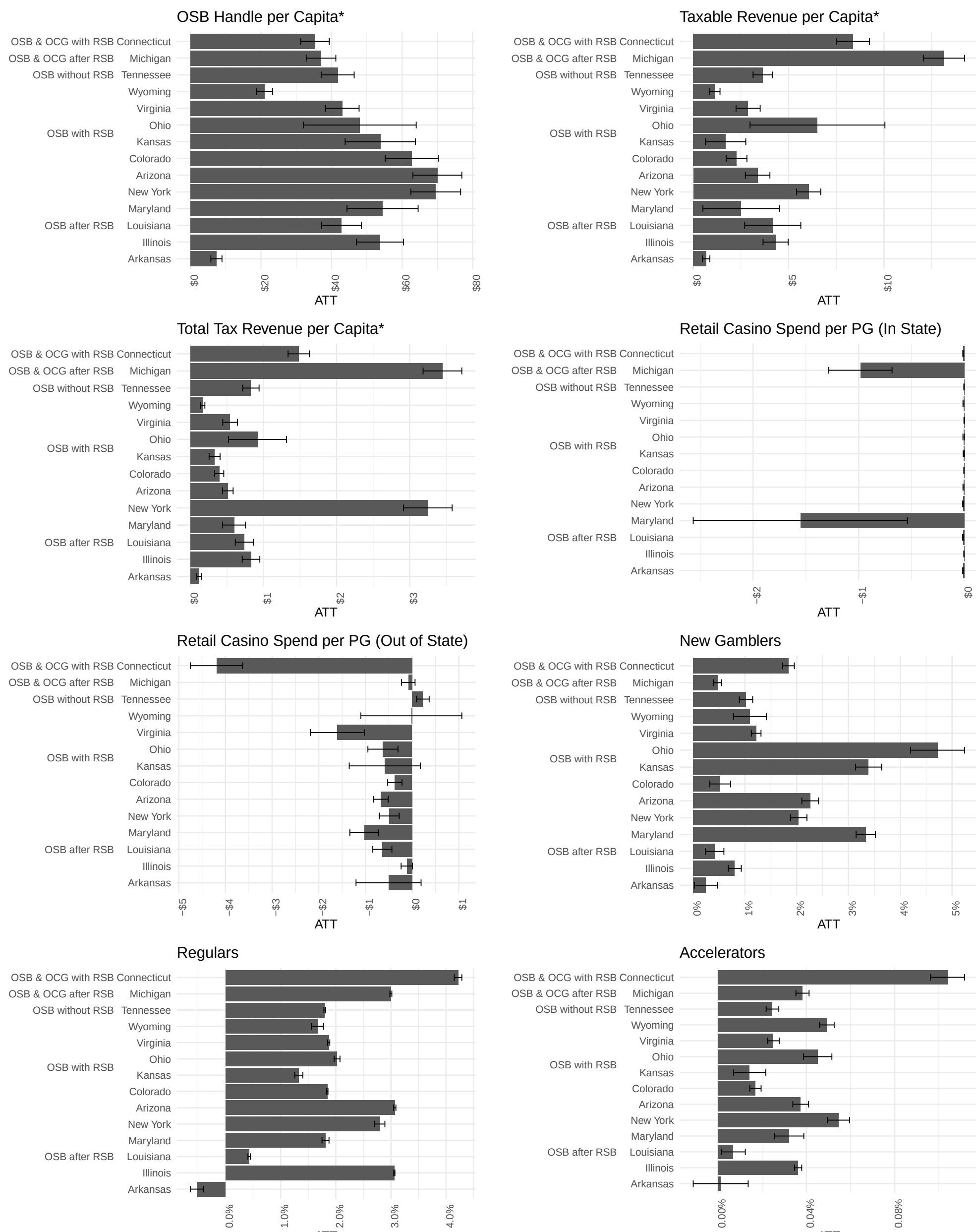


Figure OA1: State-specific ATT estimates (1)

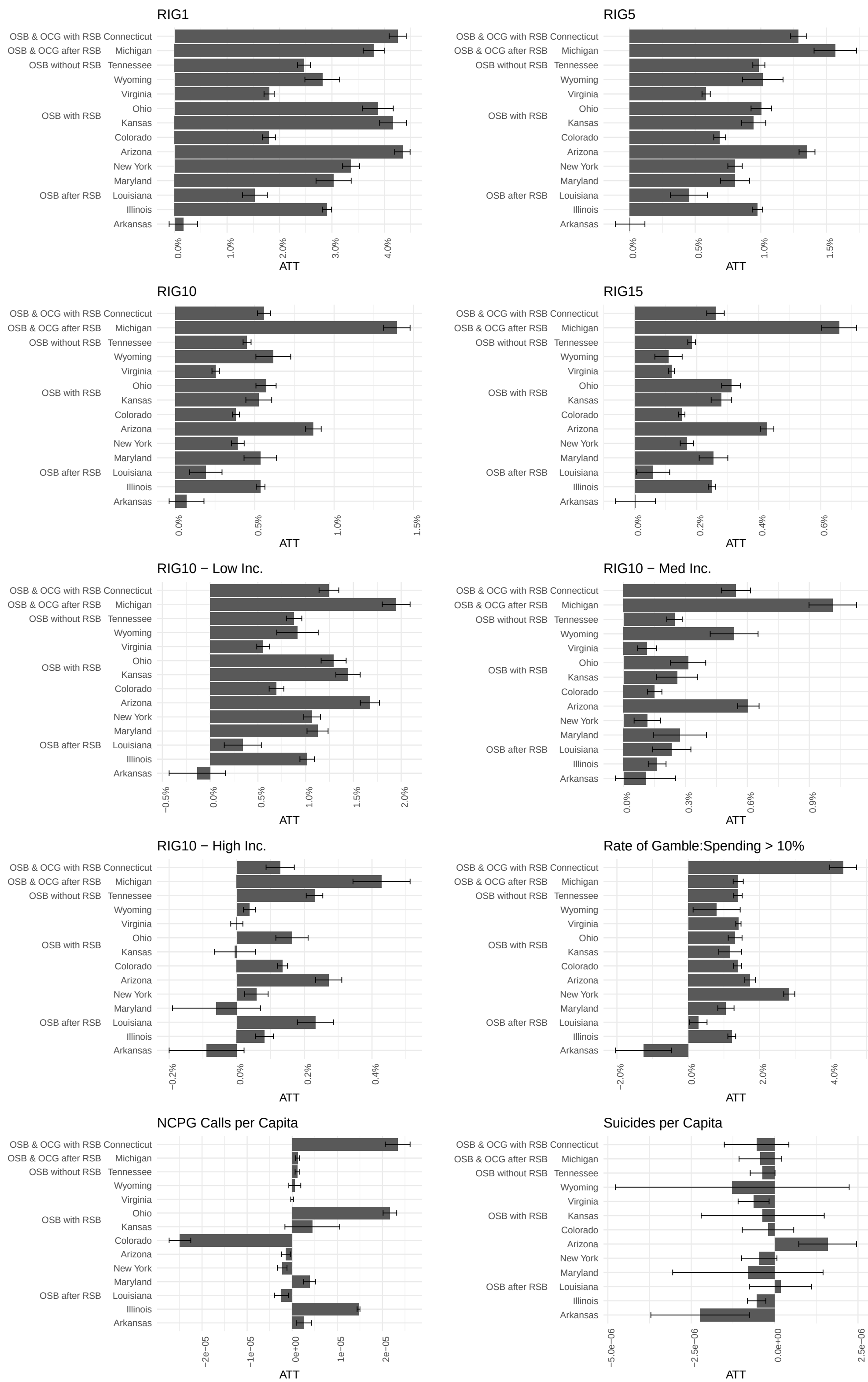


Figure OA2: State-specific ATT estimates (2)

OA.2 Dynamic Treatment Effects

Figures OA3, OA4, and OA5 shows dynamic treatment effects for RIG10 and GPPG. Standard errors are calculated using the approach described in Li and Sonnier (2023).

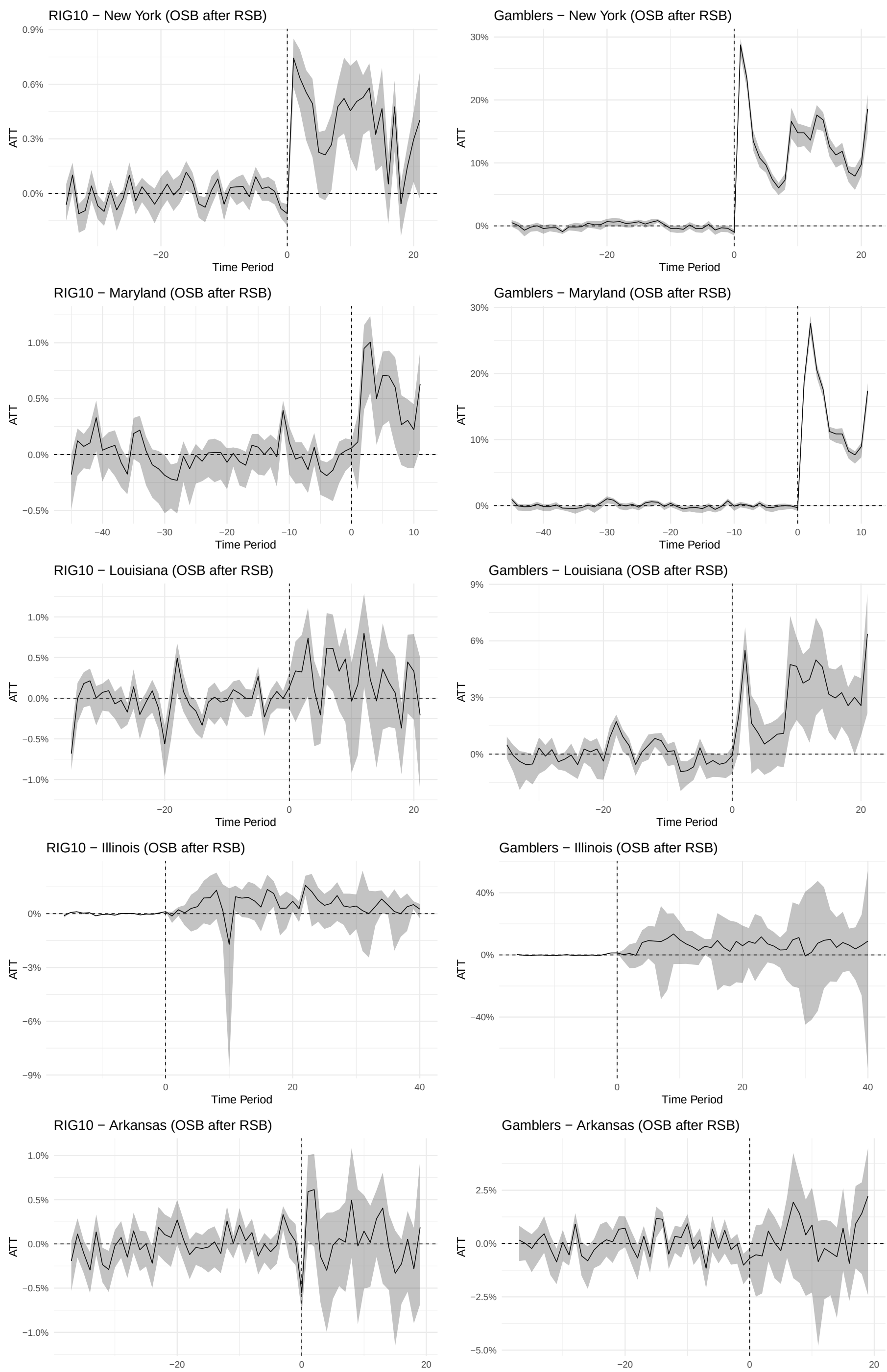


Figure OA3: State-specific Dynamic ATT estimates (1)

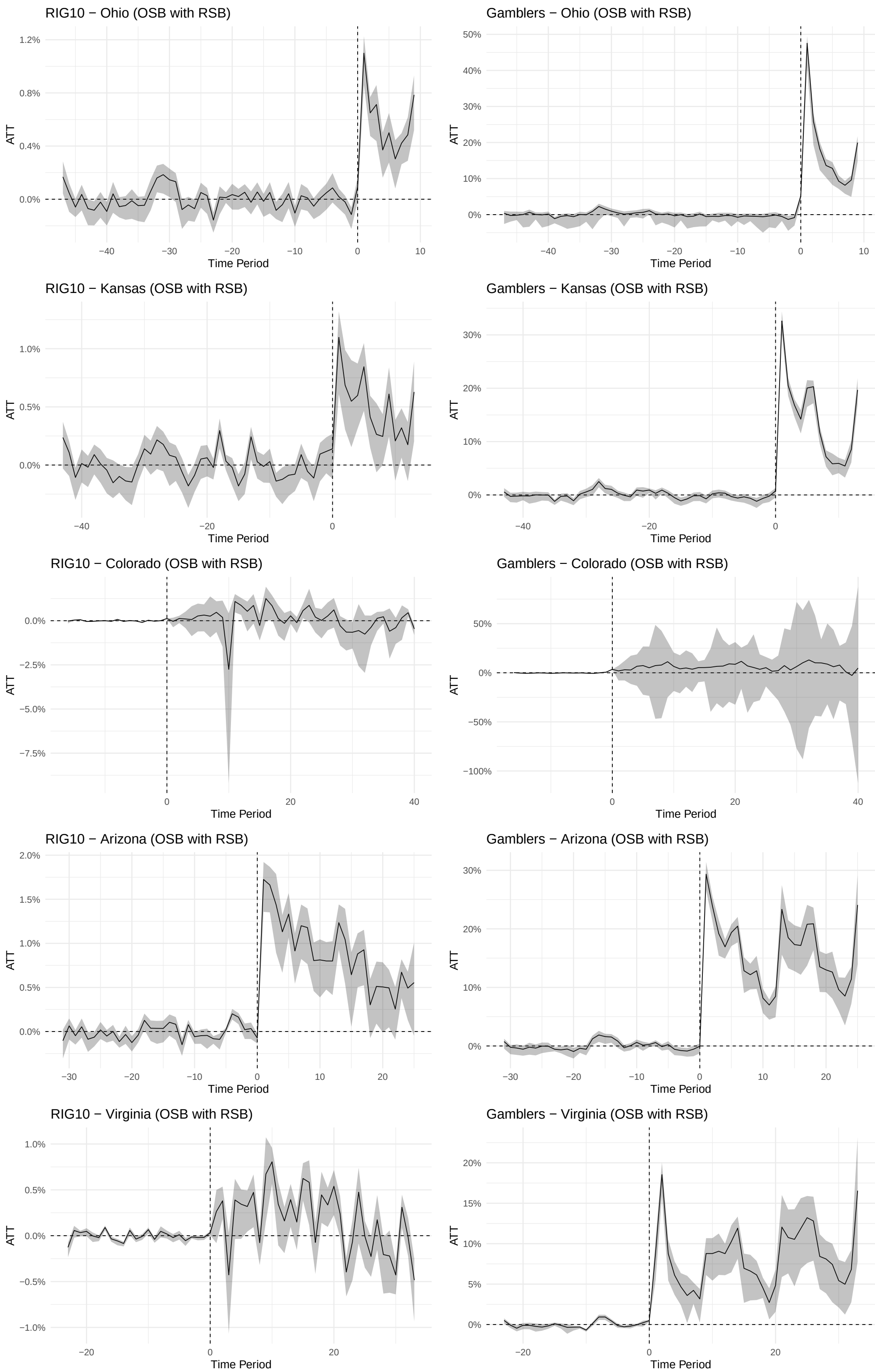


Figure OA4: State-specific Dynamic ATT estimates (2)

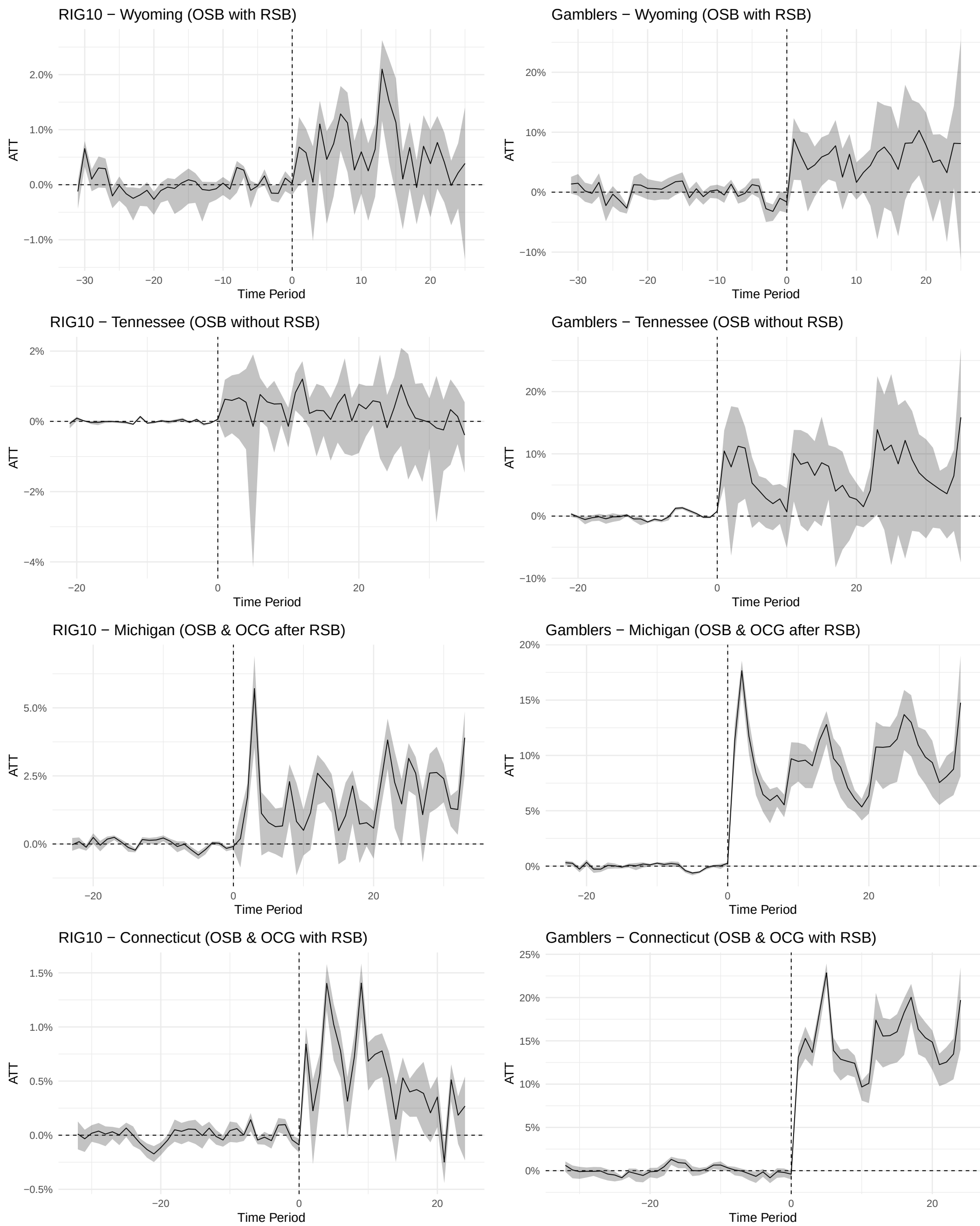


Figure OA5: State-specific Dynamic ATT estimates (3)

OA.3 Gambling Merchant List

Table OA1: Gambling Merchants

Merchant	Retail Casino
Barstool Sportsbook	No
Bet365	No
Betfair	No
BetMGM	No
BetRivers	No
Betway	No
Caesars Online Deposits	No
Caesars Sportsbook	No
DraftKings	No
FanDuel	No
FOX Bet	No
Golden Nugget Online Gaming	No
Hard Rock Digital	No
PartyPoker	No
Pointsbet	No
theScore Bet	No
TVG Network	No
Underdog Sports	No
Unibet	No
WynnBET	No
ARIA Resort & Casino	Yes
Beau Rivage	Yes
Bellagio	Yes
Borgata	Yes
Circus Circus Las Vegas	Yes
The Cosmopolitan of Las Vegas	Yes
Excalibur	Yes
Gold Strike	Yes
Golden Nugget Hotel and Casino	Yes
Luxor	Yes
Mandalay Bay	Yes
MGM Grand Detroit	Yes
MGM Grand	Yes
MGM National Harbor	Yes
MGM Springfield	Yes
New York-New York	Yes
Park MGM	Yes
The Mirage	Yes
The Venetian Resort	Yes
Vdara Hotel & Spa	Yes
Wynn Resorts	Yes