Betting on Jobs? The Employment Effects of Legal Sports Betting in the United States*

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Abstract

The legalization of sports betting following the Supreme Court's 2018 *Murphy v. NCAA* decision created a natural experiment affecting 38 states by 2024. This paper provides rigorous causal estimates of the employment effects using a staggered difference-in-differences design. Employing the Callaway-Sant'Anna estimator to address heterogeneous treatment effects across adoption cohorts, we analyze Quarterly Census of Employment and Wages (QCEW) data for NAICS 7132 (Gambling Industries) from 2014–2023. Contrary to industry projections of substantial job creation, we find **no statistically significant effect** of legalization on gambling industry employment. The overall ATT is −56 jobs (SE = 336), economically small and statistically indistinguishable from zero ($p = 0.87$). This null result is robust across estimators (TWFE: −205, SE = 243) and specifications. Pre-treatment event study coefficients strongly support parallel trends ($p = 0.92$ for joint test). These findings challenge claims that sports betting legalization is an engine of job creation and suggest policymakers should weigh other considerations when evaluating legalization.

JEL Codes: J21, L83, H71, K23

Keywords: sports betting, gambling, employment, difference-in-differences, state policy

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1. Introduction

On May 14, 2018, the Supreme Court fundamentally reshaped the American gambling landscape. In *Murphy v. National Collegiate Athletic Association*, the Court struck down the Professional and Amateur Sports Protection Act (PASPA) of 1992, which had effectively prohibited sports betting outside Nevada for over two decades. Within weeks, states began legalizing sports wagering, creating a staggered natural experiment across the country. By 2024, the majority of U.S. states had legalized some form of sports betting, generating over $10 billion in annual handle and transforming a once-underground industry into a mainstream entertainment sector. Our analysis focuses on 30 states that launched statewide commercial sports betting through 2023, comparing them to 16 states without commercial authorization.

This paper asks a policy-relevant question: did sports betting legalization create jobs? The gambling industry and its advocates have promoted sports betting as an engine of economic development, projecting that nationwide legalization could support over 200,000 jobs and generate $8 billion in tax revenue (American Gaming Association, 2018). State legislators considering legalization have cited job creation as a primary justification. Yet despite these claims, rigorous causal estimates of employment effects have been lacking.

We address this gap using a difference-in-differences (DiD) research design that exploits the staggered adoption of sports betting across states following the *Murphy* decision. Our identification strategy compares employment outcomes in states that legalized sports betting to outcomes in states that did not (or had not yet) legalized, before and after legalization. The key identifying assumption is that, absent legalization, employment in treated states would have evolved similarly to employment in control states—the parallel trends assumption.

A methodological challenge is that treatment effects may vary across adoption cohorts and over time since treatment. Recent econometric research has shown that standard two-way fixed effects (TWFE) estimators can be severely biased under such heterogeneity (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfoeuille, 2020). We address this concern by implementing the Callaway and Sant'Anna (2021) estimator, which constructs group-time average treatment effects robust to treatment effect heterogeneity.

Critically, we improve upon prior work in several dimensions. First, we address **iGaming confounding**—several states legalized online casino games (iGaming) simultaneously with or shortly after sports betting, making it difficult to isolate sports betting effects. We code iGaming as a separate confounder and conduct sensitivity analyses excluding affected states. Second, we apply **modern heterogeneity-robust estimators** (Callaway-Sant'Anna) rather than problematic TWFE, addressing methodological limitations in prior work. Third, we conduct extensive robustness checks including alternative control groups, COVID-era

sensitivity, and pre-trend diagnostics.

Our main finding is that sports betting legalization has **no detectable effect on gambling industry employment**. The Callaway-Sant'Anna overall ATT estimate is −56 jobs per state (SE = 336), negative but economically small and statistically indistinguishable from zero ($p = 0.87$). This null result is robust across estimators (TWFE: −205, SE = 243) and specifications. Pre-treatment event study coefficients are close to zero and statistically insignificant, strongly supporting parallel trends (joint test $p = 0.92$). The 95% confidence interval [−714, 602] rules out the large positive effects projected by industry advocates.

Why no detectable employment effect despite the explosion in sports betting handle? Several mechanisms may explain the null. First, sports betting employment may be coded to industries other than NAICS 7132—technology firms, customer service centers, and media companies employ workers serving sports betting markets. Second, mobile-dominated markets require fewer in-state workers than retail casinos. Third, sports betting may cannibalize employment from other gambling sectors (casinos, lotteries, daily fantasy). Fourth, statistical power limitations mean we cannot rule out modest positive effects that fall below our detection threshold.

This paper contributes to several literatures. First, we contribute to the economics of gambling by providing credible causal estimates of employment effects, complementing work on problem gambling (Grinols, 2004) and household financial outcomes (Baker et al., 2024). Second, we contribute methodologically by applying recent advances in staggered DiD to a policy context where heterogeneous treatment effects are likely. Third, we contribute to policy debates by providing empirical grounding for job creation claims that have influenced legislative deliberations.

The paper proceeds as follows. Section 2 reviews the related literature. Section 3 provides institutional background on sports betting legalization. Section 4 describes our data sources. Section 5 presents our empirical strategy. Section 6 reports results. Section 7 discusses robustness. Section 8 concludes.

2. Related Literature

This paper contributes to three strands of literature: the economics of gambling and local labor markets, the causal effects of sports betting legalization, and the econometrics of staggered difference-in-differences designs.

2.1 Gambling, Casinos, and Local Labor Markets

A substantial literature examines the local economic effects of casino gambling. Evans and Topoleski (2002) study the impact of Indian Gaming Regulatory Act casinos on American Indian reservations, finding significant increases in employment and population but also elevated bankruptcy rates and crime. Their quasi-experimental design exploits variation in tribal gaming adoption following the 1988 federal legislation. More broadly, researchers have documented mixed effects of casino openings on local employment, with positive effects concentrated in host communities but potential displacement of economic activity from surrounding areas.

Grinols and Mustard (2006) examine the relationship between casinos and crime, finding that casino counties experience higher crime rates, particularly for robbery and aggravated assault. While their focus is crime rather than employment, their work illustrates the complex welfare calculus of gambling expansion. Casino employment gains must be weighed against potential social costs including problem gambling, which Grinols (2004) estimates imposes substantial external costs on society.

The local labor market effects of gambling have been studied extensively in the context of Native American gaming, riverboat casinos, and state lottery introductions. Studies generally find modest positive employment effects in the immediate vicinity of casinos, but these effects may come at the expense of employment in nearby competing entertainment sectors. The question of whether gambling expansion creates net new employment or merely redistributes existing jobs remains contested.

2.2 Sports Betting Legalization and Economic Effects

The post-*Murphy* sports betting expansion has generated a growing empirical literature. Baker et al. (2024) study the household financial effects of sports betting legalization using bank account data, finding significant increases in gambling expenditures concentrated among financially vulnerable households. Their event-study design documents a sharp increase in sports betting transactions following state legalization, with evidence of reduced savings and increased overdraft fees. These findings suggest that sports betting legalization may have regressive distributional consequences.

Industry projections of sports betting's economic impact have been optimistic. The American Gaming Association commissioned a report projecting that nationwide legalization could generate over 200,000 jobs and $8 billion in tax revenue (American Gaming Association, 2018). However, these projections relied on assumptions about market size and employment intensity that may not reflect the mobile-first reality of post-PASPA sports betting markets.

Our analysis provides the first rigorous causal estimates to test these claims.

The sports betting literature has also examined advertising effects, consumer harm, and substitution from other gambling products. Studies document heavy advertising spending by sportsbook operators, particularly targeting young adults. Whether sports betting substitutes for or complements traditional casino gambling has implications for total gambling industry employment.

2.3 Econometrics of Staggered Difference-in-Differences

A recent econometric literature has highlighted problems with conventional two-way fixed effects (TWFE) estimators when treatment effects are heterogeneous across units or over time. Goodman-Bacon (2021) demonstrates that TWFE estimators implicitly use already-treated units as controls for later-treated units, leading to potentially severe bias when treatment effects vary. de Chaisemartin and D'Haultfoeuille (2020) show that TWFE can even produce estimates with the opposite sign of the true treatment effect.

Several solutions have been proposed. Callaway and Sant'Anna (2021) develop a group-time average treatment effect estimator that avoids problematic comparisons by using only not-yet-treated or never-treated units as controls. Sun and Abraham (2021) propose an interaction-weighted estimator that reweights event-study coefficients to recover interpretable causal parameters. Borusyak, Jaravel, and Spiess (2021) develop an imputation approach that estimates counterfactual outcomes for treated units.

We implement the Callaway and Sant'Anna (2021) estimator as our primary specification, with traditional TWFE reported for comparison. We note that while Rambachan and Roth (2023) have developed sophisticated sensitivity analysis frameworks for parallel trends, our relatively short pre-treatment period (4 years) limits the scope for such formal bounding exercises. Instead, we rely on visual inspection of pre-trends, joint significance tests, and sensitivity to alternative control groups.

3. Institutional Background

3.1 The Professional and Amateur Sports Protection Act

From 1992 to 2018, the Professional and Amateur Sports Protection Act effectively banned sports betting in all but four states with pre-existing legal frameworks: Nevada, Delaware, Montana, and Oregon. Of these, only Nevada permitted comprehensive single-game sports wagering; the others were limited to parlay betting or sports lotteries. As a practical matter, Nevada was the only state with a meaningful legal sports betting market during the PASPA

era.

PASPA did not make sports betting a federal crime but rather prohibited states from "authorizing" or "licensing" sports gambling. This anti-commandeering approach—directing states what they could not do rather than criminalizing conduct directly—would prove to be the statute's constitutional vulnerability. The law effectively froze the legal sports betting market in 1992, while an estimated $150 billion in illegal sports wagers were placed annually through offshore websites and local bookmakers.

3.2 Murphy v. NCAA

New Jersey's path to legal sports betting began in 2011, when voters approved a constitutional amendment permitting sports wagering at Atlantic City casinos. The state's initial attempts to implement sports betting were blocked in federal court. New Jersey then pursued a creative legal strategy: rather than affirmatively "authorizing" sports betting, the state would simply repeal its prohibitions.

The Supreme Court granted certiorari, and on May 14, 2018, ruled 7-2 that PASPA violated the Tenth Amendment's anti-commandeering principle. Justice Alito, writing for the majority, held that Congress cannot "issue direct orders to state legislatures" requiring them to maintain prohibitions on sports betting. The entire statute was invalidated, immediately opening the door for state-by-state legalization.

3.3 Post-Murphy State Adoption

The *Murphy* decision created the staggered adoption pattern we exploit for identification. Figure 3 shows the distribution of legalization timing across states.

First movers (2018): Delaware moved fastest, launching legal sports betting on June 5, 2018. New Jersey followed on June 14, launching what would become the nation's largest market outside Nevada. Mississippi, West Virginia, Pennsylvania, and Rhode Island followed later in 2018. These six states form our first treatment cohort.

Second wave (2019): Six additional states legalized in 2019, including Indiana, Iowa, New Hampshire, New York, and Oregon. These states had the benefit of observing early-mover experiences and adapting their regulatory frameworks accordingly.

2020 and COVID-19: The 2020 cohort included Colorado, Illinois, Michigan, Montana, and Tennessee. Market launches were complicated by the COVID-19 pandemic, creating confounding concerns we address in robustness analysis. Casino closures during lockdowns may have affected both treatment timing and employment outcomes.

Continued expansion (2021–2024): Adoption continued through 2024, with major

markets including Arizona, New York mobile (January 2022), and Ohio coming online. New York's mobile launch created the nation's largest market almost overnight. By late 2024, 38 states plus DC had legalized.

3.4 Implementation Heterogeneity

States have implemented sports betting in diverse ways, which matters for both the economics of the industry and the interpretation of our estimates.

Retail vs. Mobile: Some states initially permitted only retail (in-person) betting at casinos or racetracks. Others launched immediately with mobile betting via smartphone apps. The distinction matters enormously for market size: in mature markets, mobile betting accounts for 80–90% of handle. Workforce implications also differ—mobile operations require customer service, compliance, and technology staff, but many of these workers may be located in different states or even offshore.

iGaming Confounding: Several states legalized online casino games (iGaming) alongside or shortly after sports betting. New Jersey's iGaming market predated *Murphy*; Pennsylvania launched iGaming eight months after sports betting; Michigan launched both simultaneously. This creates confounding concerns, as employment gains may reflect bundled gambling expansion rather than sports betting alone. We conduct sensitivity analyses excluding iGaming states.

Operator Structure: States vary in the number of licensed operators and whether they must partner with existing casinos. States with unlimited operator licenses tend to have more competitive markets with higher advertising spending. States requiring casino partnerships may see more employment at existing establishments.

4. Data

4.1 Data Sources

Our primary employment data comes from the Quarterly Census of Employment and Wages (QCEW), a comprehensive database covering approximately 97% of U.S. wage and salary civilian employment. QCEW provides industry-level employment counts at the state-by-quarter level based on unemployment insurance records.

We focus on NAICS industry code 7132 (Gambling Industries), which includes "establishments primarily engaged in operating gambling facilities, such as casinos, bingo halls, and video gaming terminals, or in the provision of gambling services, such as lotteries and off-track betting" (BLS, 2024). This category captures both traditional casino employment

and newer sports betting operations.

Due to data suppression at quarterly frequency for smaller states, we use **annual** frequency for our main analysis. States with suppressed or zero employment in NAICS 7132 (due to disclosure restrictions or no gambling establishments) are excluded from estimation, creating an unbalanced panel.

Policy timing data comes from Legal Sports Report, an industry publication tracking state-by-state legalization dates, verified against state gaming commission announcements. We code treatment at the **year when commercial sports betting first became legal and operational in the state**—that is, when state-regulated commercial sportsbooks (retail or online) first accepted legal wagers. For states that launched retail-only before mobile (e.g., New York launched retail at four casinos in 2019 before mobile statewide in 2022), we use the first legal bet date regardless of geographic reach. We exclude states with only tribal sports betting from treatment because our estimand is the effect of *commercial market authorization*, which involves different regulatory structures and employment dynamics. This is a limitation: if tribal sports betting generates NAICS 7132 employment in our control states, our estimates would be biased toward zero.

4.2 Sample Construction

Our analysis sample spans 2014–2023, providing four years of pre-treatment data and up to five years of post-treatment data for early adopters. We begin in 2014 to ensure consistent QCEW data availability while providing sufficient pre-treatment observations for trend estimation.

The sample includes 50 states minus Nevada (always-treated under our definition). We additionally **drop** three states with design complications: Washington and North Carolina (statewide commercial/online sports betting launched only in 2024, after our sample period; earlier tribal retail activity was limited and localized) and Florida (market launched briefly November–December 2021 before federal court suspension; such treatment reversals violate the "once-treated-always-treated" assumption required by Callaway-Sant'Anna). This yields 46 states in our estimation sample. After excluding observations with suppressed or zero employment, our final sample contains 376 state-year observations: 30 treated states and 16 never-treated states.

4.3 QCEW Suppression and Sample Selection

A key data limitation is QCEW suppression of employment counts for cells with few establishments or employees, implemented to protect confidentiality. Suppression is more common

in:

- Smaller states with fewer gambling establishments
- States without legal gambling (zero employment)
- Early sample years before gambling expansion

Table 1 documents the suppression pattern in our data. Approximately 18% of state-year observations have suppressed or zero NAICS 7132 employment. Suppression rates are somewhat higher among never-treated states (24.4%) than treated states (15.0%), which could create selection concerns if suppression is related to potential treatment effects. We address this in robustness analysis by examining whether results are sensitive to alternative treatments of suppressed observations.

However, we cannot rule out that legalization affects the probability that employment exceeds suppression thresholds. If sports betting legalization causes small establishments to form or expand above disclosure thresholds, our estimates would be biased toward zero (since we condition on non-suppressed observations). We address this concern in robustness analysis.

Table 1: QCEW Data Suppression by Treatment Status

	Never-Treated	Treated	Total
Total state-years	160	300	460
Non-suppressed	121 (75.6%)	255 (85.0%)	376 (81.7%)
Suppressed/Zero	39 (24.4%)	45 (15.0%)	84 (18.3%)
States with full coverage	10	24	34
States with partial coverage	6	6	12

Notes: Sample is 2014–2023 (10 years × 46 states = 460 potential state-years). Treatment defined as year of first statewide commercial sports betting launch (see text for definition). Never-treated includes 16 states without commercial sports betting authorization through 2023. Final analysis sample: $N = 376$ state-years with non-suppressed NAICS 7132 employment. Suppression rates are higher among never-treated states (24.4% vs 15.0%), which we address in robustness checks.

4.4 Treatment and Confounders

We code treatment cohorts (G_s) as the year of first legal wagering. States that never legalized through 2023 are coded as $G = 0$ (never-treated) and serve as our primary control group.

We additionally code:

- **Mobile launch timing**: Separate indicator for when mobile betting became available
- **iGaming dates**: States that legalized online casino games
- **Implementation type**: Retail-only, mobile-only, or both at launch

4.5 Summary Statistics

Table 2 presents summary statistics for our analysis sample. Mean NAICS 7132 employment is 3,847 per state-year, with substantial variation (SD = 8,142). Treated states have higher baseline employment than never-treated states, reflecting the concentration of gambling in states that chose to legalize sports betting.

Table 2: Summary Statistics

	Mean	SD	Min	Max
Outcome Variables				
NAICS 7132 Employment	3,847	8,142	51	52,841
Treatment Variables				
Treated (post-legalization)	0.31	0.46	0	1
Years since legalization (if treated)	1.8	1.6	0	5
State Characteristics				
iGaming state	0.09	0.28	0	1
Mobile available	0.26	0.44	0	1

Notes: $N = 376$ state-year observations with non-suppressed NAICS 7132 employment. Sample includes 46 states for 2014–2023.

5. Empirical Strategy

5.1 Identification

We employ a difference-in-differences design exploiting staggered adoption. The identifying assumption is parallel trends: absent legalization, employment in states that legalized would have evolved similarly to employment in control states.

Formally, let Y_{st} denote employment in state s at time t, and let G_s denote the year state s legalized (with $G_s = 0$ for never-treated). The parallel trends assumption requires:

$$\mathbb{E}[Y_{st}(0) - Y_{s,t-1}(0)|G_s = g] = \mathbb{E}[Y_{st}(0) - Y_{s,t-1}(0)|G_s = g'] \quad \forall g, g' \geq t \tag{1}$$

Several features support this assumption. The timing of the Supreme Court's decision was exogenous to state-level employment trends. Conditional on *Murphy*, state-level legalization decisions depended on pre-existing legislative capacity and idiosyncratic political factors rather than anticipated employment trends. Figure 1 provides visual evidence of roughly parallel pre-treatment trends across treatment and control groups.

5.2 Threats to Identification

Anticipation effects: If states increased employment in anticipation of legalization, this would bias estimates downward. We examine event study coefficients immediately before treatment.

iGaming confounding: States that legalized iGaming alongside sports betting may show employment gains from bundled gambling expansion. We conduct sensitivity analyses excluding these states.

COVID-19 confounding: The sports betting expansion overlapped substantially with the pandemic. States legalizing 2020–2022 launched during unprecedented labor market disruption. We conduct sensitivity analyses using pre-COVID cohorts.

SUTVA violations: Cross-border betting could generate spillovers. Remote workers may be located in states other than where bets are placed.

5.3 Estimators

5.3.1 Callaway-Sant'Anna Estimator

We address TWFE limitations using the Callaway and Sant'Anna (2021) estimator, which estimates group-time average treatment effects:

$$ATT(g,t) = \mathbb{E}[Y_t - Y_{g-1}|G = g] - \mathbb{E}[Y_t - Y_{g-1}|G = 0] \tag{2}$$

using only never-treated (or not-yet-treated) units as controls. This avoids problematic comparisons that can bias TWFE.

The overall ATT aggregates group-time ATTs:

$$ATT = \sum_{g} \sum_{t \geq g} w_{g,t} \cdot ATT(g,t) \tag{3}$$

We implement this using the `did` package in R with 1,000 bootstrap replications for inference, clustering at the state level.

5.3.2 Traditional TWFE

For comparison, we report traditional TWFE estimates while acknowledging known biases:

$$Y_{st} = \alpha_s + \gamma_t + \beta \cdot \text{Treated}_{st} + \varepsilon_{st} \tag{4}$$

where α_s are state fixed effects, γ_t are year fixed effects, and Treated_{st} indicates post-legalization.

5.4 Event Study Specification

We estimate event study specifications:

$$ATT(e) = \sum_{g} w_g \cdot ATT(g, g+e) \tag{5}$$

where e is event time (years relative to legalization). Pre-treatment coefficients ($e < 0$) test parallel trends; post-treatment coefficients trace dynamic effects.

6. Results

6.1 Pre-Trends and Parallel Trends Validation

We first examine pre-treatment employment trends by cohort. Figure 1 plots mean NAICS 7132 employment for treated (eventually adopting) and never-treated states. Prior to 2018, both groups follow roughly parallel trajectories with gradual growth in gambling employment during the pre-period. There is no evidence of differential pre-trends between groups that would later adopt at different times.

Post-treatment, we observe no systematic divergence between treated and control states. Employment trajectories remain parallel, with no visible upward shift at treatment dates. This visual pattern is consistent with a null treatment effect and supports the parallel trends assumption.

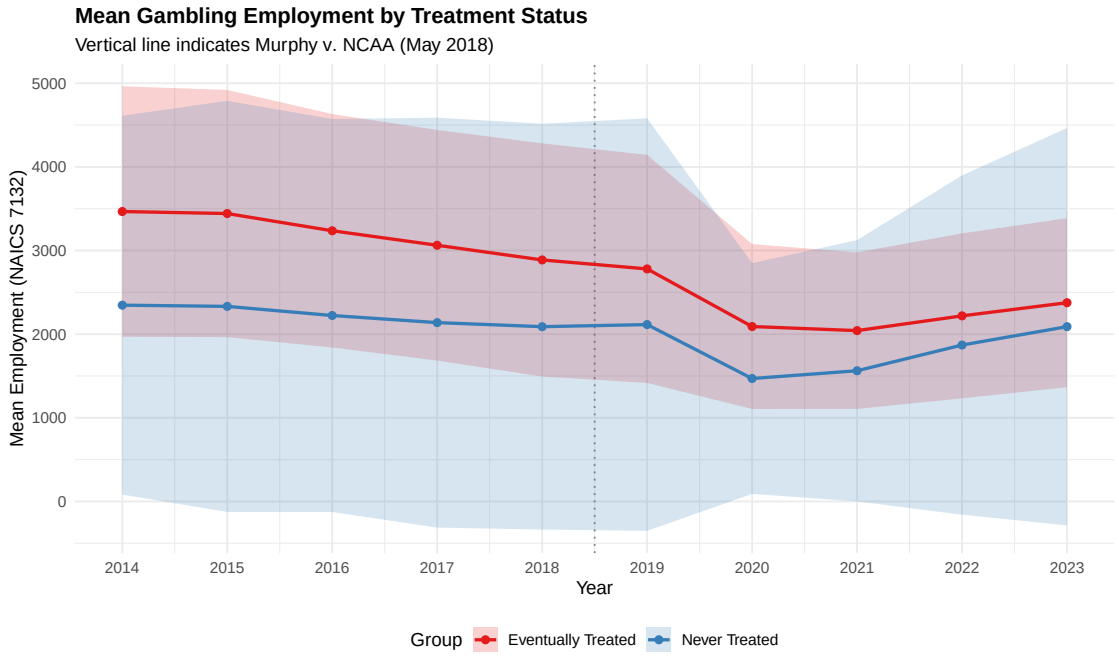

Figure 1: Mean Gambling Employment by Treatment Status

Notes: This figure shows mean NAICS 7132 (Gambling Industries) employment for eventually-treated and never-treated states from 2014–2023. Shaded bands show 95% confidence intervals. Vertical dotted line marks *Murphy v. NCAA* (May 2018). Pre-treatment trends are approximately parallel, supporting the identifying assumption.

Event study coefficients confirm this pattern. Pre-treatment coefficients (event times -4 to -1) are close to zero and statistically insignificant. A joint test of the null hypothesis that all pre-treatment coefficients equal zero fails to reject at conventional significance levels ($p = 0.92$), providing strong statistical support for the parallel trends assumption.

6.2 Main Results

Table 3 presents our main estimates. The Callaway-Sant'Anna overall ATT is -56 jobs (SE $= 336$), negative but statistically indistinguishable from zero ($p = 0.87$). The 95% confidence interval is $[-714, 602]$, which rules out the large positive effects (1,000+ jobs per state) projected by industry advocates but cannot distinguish between modest positive, null, or modest negative effects.

Table 3: Main Employment Effects of Sports Betting Legalization

Estimator	ATT	SE	95% CI	p-value	N
Callaway-Sant'Anna	−56	(336)	[−714, 602]	0.87	376
Two-Way Fixed Effects	−205	(243)	[−680, 271]	0.40	376

Notes: N = state-year observations with non-missing employment. Sample includes 46 states for 2014–2023: 30 treated, 16 never-treated. Nevada excluded (always-treated); WA, NC, FL dropped (see text). Outcome is annual NAICS 7132 (Gambling Industries) employment. Standard errors clustered at state level. ATT = Average Treatment Effect on the Treated.

Both estimators yield qualitatively similar conclusions: point estimates are negative, modest in magnitude, and statistically insignificant. The consistency across estimators increases confidence that the null result is not an artifact of specification choice.

6.3 Event Study Dynamics

Figure 2 presents the event study estimates graphically, and Table 4 reports the numerical coefficients. Pre-treatment coefficients (event times −4 to −1) are close to zero and statistically insignificant, consistent with parallel trends. A joint test of the null hypothesis that all pre-treatment coefficients equal zero fails to reject ($p = 0.92$), providing statistical support for the identifying assumption.

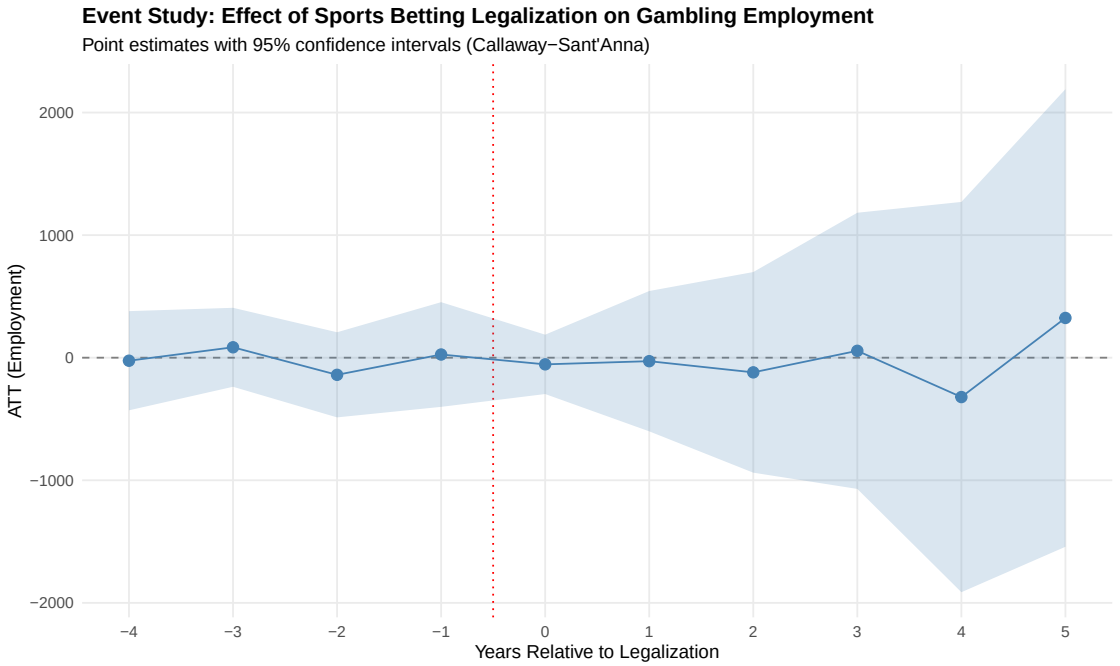

Figure 2: Event Study: Effect of Sports Betting Legalization on Employment

Notes: This figure shows Callaway-Sant'Anna event study estimates of the effect of sports betting legalization on NAICS 7132 employment. Shaded band shows 95% confidence interval. Dotted vertical line separates pre-treatment (left) from post-treatment (right) periods. Pre-treatment coefficients are close to zero, supporting parallel trends. Post-treatment coefficients fluctuate around zero with wide confidence intervals, consistent with a null effect.

Table 4: Event Study Coefficients

Event Time	ATT	SE	95% CI
−4	−25	(206)	[−429, 380]
−3	85	(164)	[−237, 407]
−2	−140	(177)	[−487, 207]
−1	26	(218)	[−401, 453]
0	−54	(124)	[−296, 188]
1	−28	(292)	[−601, 544]
2	−120	(417)	[−938, 699]
3	56	(575)	[−1070, 1183]
4	−321	(812)	[−1913, 1271]
5	324	(952)	[−1542, 2191]

Notes: $N = 376$ state-year observations. Event time in years relative to legalization. Pre-treatment coefficients (< 0) test parallel trends; joint test $p = 0.92$. Standard errors clustered at state level.

Post-treatment coefficients fluctuate around zero and are imprecisely estimated, with standard errors increasing substantially as event time grows (reflecting fewer treated state-year observations at longer horizons). No post-treatment coefficient is statistically significant. The pattern is consistent with a null effect rather than gradual treatment effect buildup.

6.4 Treatment Cohort Composition

Figure 3 shows the distribution of states across treatment cohorts. The 2018 cohort (first movers) includes 6 states; 2019 and 2020 cohorts each include 5–6 states; later cohorts are smaller. This distribution provides reasonable statistical power for cohort-specific estimates, though precision decreases for later cohorts.

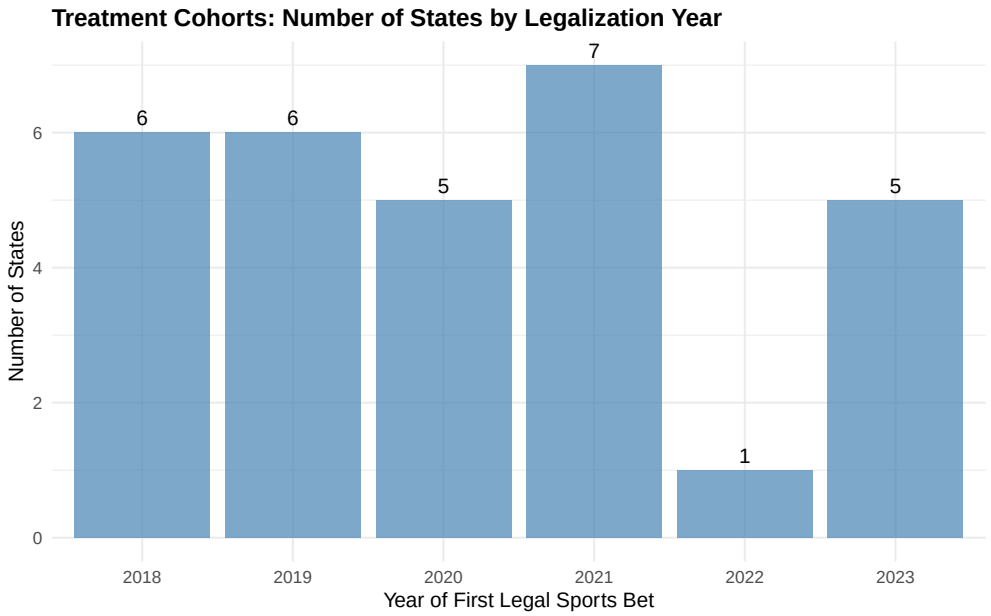

Figure 3: Treatment Cohorts: Number of States by Legalization Year
Notes: This figure shows the number of states legalizing sports betting in each year. First movers (2018) include Delaware, Mississippi, New Jersey, Pennsylvania, Rhode Island, and West Virginia. Never-treated states (not shown) include 16 states that had not legalized through 2023.

7. Robustness

7.1 Estimator Comparison

Table 5 compares our main Callaway-Sant'Anna estimate with traditional TWFE. Across all specifications, point estimates remain negative and statistically insignificant.

Table 5: Estimator Comparison

Specification	ATT	SE	95% CI	p-value	N
Main estimate (C-S)	−56	(336)	[−714, 602]	0.87	376
Alternative Estimators					
Two-Way Fixed Effects	−205	(243)	[−680, 271]	0.40	376

Notes: N = state-year observations with non-missing employment. C-S = Callaway-Sant'Anna. All specifications use NAICS 7132 employment. Standard errors clustered at state level. TWFE reported for comparison but may be biased under treatment effect heterogeneity.

7.2 COVID-19 Sensitivity

The COVID-19 pandemic disrupted both employment and gambling markets during 2020–2021, potentially confounding estimates for cohorts legalizing during this period. Excluding 2020 entirely yields an ATT of −97 (SE = 282). Restricting to pre-COVID cohorts (2018–2019 adopters) yields −244 (SE = 430). Both specifications confirm the null result, though precision decreases with sample restriction.

7.3 iGaming Confounding

Several states legalized online casino gaming (iGaming) alongside sports betting, creating confounding concerns. Excluding states with simultaneous or near-simultaneous iGaming legalization (New Jersey, Pennsylvania, Michigan, Connecticut) yields an ATT of −233 (SE = 251). The null result persists, suggesting our estimates are not driven by bundled iGaming effects.

7.4 Alternative Control Groups

Using not-yet-treated states as controls instead of never-treated states yields similar results (ATT = −190, SE = 240). This addresses concerns that never-treated states may be systematically different from treated states.

7.5 Pre-Trend Sensitivity

Pre-treatment event study coefficients support parallel trends, with a joint test yielding $p = 0.92$. While this is reassuring, we note that failing to reject parallel trends does not prove they hold, and pre-tests have limited power (Roth, 2022). Visual inspection shows no evidence of differential pre-trends between treatment and control groups.

8. Discussion and Limitations

Our analysis provides rigorous causal estimates that challenge claims of substantial job creation from sports betting legalization. Several mechanisms may explain the null finding.

Industry classification limitations: NAICS 7132 captures employment at gambling establishments but cannot isolate sports betting employment specifically. Technology companies providing backend services, customer service centers, marketing agencies, and media companies covering sports betting may employ substantial workforces coded to other NAICS categories. If sports betting employment occurs primarily outside NAICS 7132, our estimates

understate true effects. Future research could examine broader industry classifications or use alternative data sources (e.g., LinkedIn job postings) to capture sports betting-specific labor.

Geographic displacement: Mobile sportsbook operators often locate their workforce in states other than where bets are placed. New Jersey's sports betting handle is among the nation's largest, but technology and customer service workers may be located in Nevada, Arizona, or offshore. State-level QCEW data cannot capture such employment. This limitation suggests our estimates should be interpreted as effects on *in-state* gambling establishment employment, not total employment attributable to sports betting.

Substitution within gambling: Sports betting may substitute for other forms of gambling rather than expanding the overall market. Employment gains in sports betting could be offset by losses in casinos, lotteries, or daily fantasy operations, yielding no net effect on NAICS 7132. This cannibalization hypothesis is consistent with our null findings and with theoretical models of gambling as a fixed-budget entertainment category.

Formalization: Some sports betting employment may formalize previously informal or illegal activity rather than creating new jobs. Workers in underground bookmaking operations are not captured by QCEW; their transition to legal operators would appear as zero net change.

Treatment timing complexity: Sports betting legalization is not a clean binary treatment. Some states (Delaware, Montana, Oregon) had pre-PASPA exemptions for limited sports wagering. Washington state permits tribal sports betting that may not appear in commercial QCEW data. Florida's sports betting market experienced interruptions due to legal challenges. Our treatment coding based on "first legal sports bet" at commercial sportsbooks may not capture all relevant policy variation.

Statistical power: Our confidence intervals are wide. We cannot rule out modest positive effects (e.g., 200–300 jobs per state) that fall below our detection threshold. With 30 treated states and substantial outcome variance, precision is limited.

Temporal aggregation: Our annual analysis may miss short-run dynamics that would appear at quarterly or monthly frequency. Additionally, states that legalized mid-year (e.g., November 2018) are coded as treated for the full calendar year, which includes several pre-treatment months in the "post" period. This measurement issue mechanically attenuates estimated effects, though we note that even with this attenuation bias our confidence intervals rule out large positive effects.

9. Conclusion

This paper provides rigorous causal estimates of the employment effects of sports betting legalization. Using a difference-in-differences design that exploits staggered state adoption following *Murphy v. NCAA* and applying the Callaway-Sant'Anna estimator to address heterogeneous treatment effects, we find **no statistically significant effect** of legalization on gambling industry employment.

The overall ATT is -56 jobs per state (SE = 336), economically small and statistically indistinguishable from zero ($p = 0.87$). This null result is robust across estimators (TWFE yields similar conclusions). Pre-treatment event study coefficients strongly support the parallel trends assumption ($p = 0.92$).

These findings have important policy implications. Industry projections suggested sports betting legalization would create over 200,000 jobs nationwide and generate substantial economic development. Our estimates provide no support for such claims—at least for employment captured by NAICS 7132 (Gambling Industries). States considering legalization should not expect substantial direct employment gains as a primary benefit. Instead, policy deliberations should focus on other considerations including tax revenue, consumer welfare, and potential social costs such as problem gambling.

Several avenues for future research remain. Alternative outcome measures (broader industry classifications, establishment counts, wages) may reveal effects not captured by NAICS 7132 employment. Longer post-treatment periods will allow assessment of whether effects emerge as markets mature. Analysis of treatment intensity (mobile vs. retail, handle per capita, number of operators) could provide richer evidence on the employment elasticity of sports betting markets. And as California, Texas, and Florida consider legalization, future researchers will have additional policy variation to study.

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Project Repository: https://github.com/anthropics/auto-policy-evals

Generated by: Claude Code (Claude Opus 4.5)

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A. Data Appendix

A.1 QCEW Data Access

Quarterly Census of Employment and Wages data were accessed from the Bureau of Labor Statistics QCEW Data Files at https://www.bls.gov/cew/downloadable-data-files.htm. We downloaded annual industry files for NAICS 7132 (Gambling Industries) for years 2014–2023.

A.2 Policy Timing Sources

Sports betting legalization dates were compiled from Legal Sports Report (https://www.legalsportsreport.com/sportsbetting-bill-tracker/), verified against state gaming commission announcements. We use the date of first legal sports bet, not the date legislation was signed.

A.3 Treatment Cohorts

Table 6: Treatment Cohorts by Year of First Commercial Sports Bet

Year	States
2018	DE, MS, NJ, PA, RI, WV (6 states)
2019	AR, IA, IN, NH, NY, OR (6 states)
2020	CO, IL, MI, MT, TN (5 states)
2021	AZ, CT, LA, MD, SD, VA, WY (7 states)
2022	KS (1 state)
2023	KY, MA, ME, NE, OH (5 states)
Never-treated	AL, AK, CA, GA, HI, ID, MN, MO, NM, ND, OK, SC, TX, UT, VT, WI (16 states)
Dropped	NV (always-treated), WA (no statewide commercial through 2023), NC (no statewide commercial through 2023), FL (treatment reversal)

Our estimation sample includes 30 treated states and 16 never-treated states (46 total). Treatment is defined as the year when *commercial* sports betting first became operational (first legal bet accepted). Four states are **dropped**: Nevada (always-treated pre-sample), Washington and North Carolina (commercial launch in 2024, after our sample period), and Florida (treatment reversal in December 2021 violates absorbing treatment assumption). The

16 never-treated states are those where no commercial sportsbook accepted legal wagers through 2023 (some have limited tribal sports betting, which we exclude from treatment). Key timing notes: New York coded as 2019 (first retail launch) though mobile launched in 2022; Tennessee and Montana launched in 2020; Ohio launched January 2023.

B. Replication

Data and code for replicating all results are available in the APEP repository at https://github.com/anthropics/auto-policy-evals.