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# Gambling Advertising and Problem Gambling in the United States: Multi-Level Evidence Beyond Sports-Betting Legal Status

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## Abstract

**Background:** U.S. gambling debates often center on legal status, but advertising is a distinct policy lever. **Methods:** Study 1 links quarterly state-level gambling advertising outlays across paid media (2014–2023) to Google Trends indices for 116 problem-gambling help-seeking queries across 41 states (1,640 state-quarters). Study 2 combines surveys with passive digital traces from 419 exposure-eligible mobile-platform users. **Results:** Higher state-level ad spend, but not sports-betting legal status, was associated with greater help-seeking search interest, and this association strengthened over time. Platform-specific gambling-ad exposure on Facebook, TikTok, and X was associated with gambler status, gambling frequency, and the probability of any observed gambling-site visit. Clean gambling ads were also associated with higher 24-hour odds of subsequent gambling touchpoints, especially web visits; among gamblers, ad-triggered touchpoints were associated with problem-gambling scores more consistently than raw clean ad volume. **Conclusions:** Convergent state-level and individual-level evidence links gambling advertising with engagement and harm-related indicators, while the observational designs do not identify causal effects. The findings support stronger restrictions on advertising volume, placement, inducements, and targeting.

## 1 | Introduction

Gambling-related harm is a public-health issue as well as an individual clinical problem. It can involve debt and financial insecurity, relationship strain, reduced work and educational functioning, mental-health problems, and wider spillovers to families and communities (Wardle et al., 2024). Tran et al. (2024) likewise show in a systematic review and meta-analysis that gambling and problematic gambling are prevalent enough to warrant population-level policy attention. That framing is especially salient in the United States, where online sports betting and other forms of digital gambling have expanded quickly since *Murphy v. National Collegiate Athletic Association* (2018).

For readers outside the United States, three features matter. Before 2018, federal law sharply limited state authorization of sports betting. The Supreme Court's Murphy decision removed that federal barrier, leaving states to decide whether and how to authorize sports betting. Legal sports-betting markets have since expanded state by state rather than through a national regime. U.S. online gambling regulation is therefore patchwork: states license and supervise operators, online casino and sports-betting rules differ by jurisdiction, and advertising controls usually appear in state licensing conditions, responsible-gambling rules, platform policies, and general consumer-protection law rather than in one federal gambling-advertising code.

Online gambling can heighten risk relative to land-based gambling because it is private, always available, and tightly integrated with digital payments, notifications, and cross-device use. Prior reviews identify several mechanisms that can intensify play: 24/7 access, cashless and frictionless payment, immediate access to funds, rapid event cycles, direct click-through from promotional content to wagering, personalization, and the blending of gambling into ordinary mobile and social-media routines (Gainsbury, 2015; Ghelfi et al., 2024; Wardle et al., 2024). Online products can also embed deceptive or manipulative design features, including dark patterns that make deposit, repeat play, or continued engagement easier than pausing or withdrawing (Newall, 2025).

Gambling marketing connects that infrastructure to consumers. We use *gambling marketing* for the broader set of commercial activities intended to encourage product use, including advertising, sponsorship, affiliate marketing, direct marketing, and sales promotions. We use *advertising* for paid media exposure, the construct measured in both studies, and *inducements* for offers such as bonus bets, free bets, free spins, cashback offers, and sign-up bonuses. Inducements matter because consumers often misread their true cost and conditions, and experimental work shows that they can heighten interest, shape gambling-related cognition, and encourage riskier behavior (Challet-Bouju et al., 2020; Hing et al., 2014, 2019; Lole et al., 2020). Reviews also find that advertising and other gambling-marketing practices can affect gambling attitudes, intentions, participation, and harms, although effect sizes vary across populations and study designs (Bouguettaya et al., 2020; Guillou-Landreat et al., 2021; McGrane et al., 2023; Savolainen et al., 2025; Syvertsen et al., 2022).

The evidence base, however, remains methodologically uneven. Many studies of gambling advertising rely on self-reported ad recall, perceived advertising impact, or cross-sectional surveys of gamblers. Experimental work often isolates individual inducements, warning labels, or message features, while qualitative research explains lived experience but cannot estimate population exposure intensity. These designs have clarified plausible mechanisms and associations, but none alone identifies the effect of sustained, real-world advertising exposure at the scale relevant to policy. The absence of definitive causal estimates is therefore not evidence that gambling advertising is safe; it reflects the difficulty of separating advertising effects from consumer self-selection, operator targeting, and changing market conditions. Policy-relevant inference instead depends on triangulation across study designs, exposure measures, and levels of analysis (McGrane et al., 2023; Wardle et al., 2024). Data-fusion research that links self-reports to recorded financial behavior demonstrates the value of combining evidence sources rather than treating any single measure as dispositive (Zendle & Newall, 2024). A smaller but growing set of policy-oriented studies moves closer to those questions by linking advertising and marketing intensity to online gambling behavior and by examining how regulatory changes affect both consumer behavior and operator strategy (Aonso-Diego et al., 2025; García-Pérez et al., 2024). Comparable evidence remains limited in the United States, where media markets, platform environments, and constitutional constraints differ substantially from the better-studied European cases.

Regulating online gambling marketing is difficult for both legal and infrastructural reasons. Advertising and other marketing communications span television, search, display, sponsorship, direct marketing, affiliate sites, and social platforms, and the same market often includes licensed operators, offshore providers, tipster ecosystems, and intermediaries such as affiliates and ad networks (Fisher et al., 2025; Gainsbury & Wood, 2011). Comparative policy reviews show that online gambling rules remain fragmented internationally and often emphasize individual responsibility more than system-level control (Ukhova et al., 2024). Official regulatory reviews likewise emphasize how cross-border services, affiliate marketing, social-platform distribution, payment intermediaries, and limited regulator visibility into ad delivery complicate enforcement (Australian Communications and Media Authority, 2022; European Commission, 2018). Those problems are especially salient when the policy goal is to protect children and young people from pervasive gambling marketing (Thomas et al., 2023). In internet-policy terms, gambling advertising is an ad-delivery and platform-visibility problem, not only a consumer-information issue; in public-health terms, it fits the commercial-determinants framework in which private actors shape exposure, behavior, and policy environments (DeNardis & Hackl, 2015; Gorwa, 2019; Kickbusch et al., 2016; Mialon, 2020).

Public debate often collapses legal status and advertising into a single story of gambling expansion, but they are distinct mechanisms. Legal status determines whether a product can be lawfully offered in a jurisdiction; advertising intensity captures how heavily it is publicized, through which channels, and with what messages or inducements once it is available. The access and adaptation literature is therefore relevant but incomplete for a policy debate centered on advertising. A state can legalize sports betting while still varying dramatically in ad volume, bonus design, affiliate activity, platform targeting, and enforcement intensity. For internet policy, that distinction matters because regulators and platforms have more room to intervene in visibility, targeting, inducement design, or affiliate distribution than to prohibit a lawful market outright. For public-health policy, it matters because harm can be amplified not only by the availability of a legal product, but also by the amount and design of advertising and other marketing pressure surrounding that product (Fisher et al., 2025; Gainsbury & Wood, 2011; Storer et al., 2009; Ukhova et al., 2024; Zoglauer et al., 2021).

What remains relatively scarce, especially in the United States, is multi-level evidence that links advertising intensity to behavioral engagement and harm-related indicators while distinguishing advertising from legal market status. Existing studies rarely combine aggregate market exposure with observed behavioral traces, and they rarely ask whether advertising still matters once a narrower measure of legal status is held constant. By observed behavioral traces, we mean passively recorded activity logs, such as platform ad encounters, website visits, and app-use events, rather than retrospective self-reports. That omission matters in a U.S. debate that often moves quickly from *Murphy* to "legalization" while treating advertising as a secondary issue rather than as a distinct policy lever.

This article addresses that gap with two complementary studies. Study 1 uses multi-channel state-level advertising spend and treats state-quarter sports-betting legal status as a control rather than as the paper's main explanatory construct. A state-quarter is one state observed during one calendar quarter; for example, Colorado in 2023 Q1 is one state-quarter. This population-level design shows whether advertising intensity covaries with a harm-related search signal across jurisdictions and over a decade, a question that individual trace data cannot answer. Study 2 uses passive digital traces to measure mobile gambling-ad exposure on Facebook, TikTok, and X, pairs those traces with survey measures of gambling engagement and symptoms, and then tests whether clean gambling-ad exposures are followed by short-run gambling touchpoints. Its individual-level design shows whether timestamped exposures are associated with reported and observed engagement. The two scales of evidence are complementary: Study 1 tests the breadth of the association across states and time, whereas Study 2 tests its behavioral specificity within users. Neither design alone identifies a causal effect, but convergence across distinct exposure measures and outcomes reduces dependence on any single data source. The outcomes in both studies reach beyond any single observed exposure channel because gambling advertising can affect participation across platforms, devices, and contexts, although we do not claim to observe every route. The empirical test is direct: is heavier gambling advertising linked to more help-seeking searches in states and to more gambling engagement among individuals after accounting for whether sports betting was legal in a state-quarter?

## 2 | Study 1

Study 1 supplies the population-level counterpart to Study 2. Its decade-long quarterly state panel tests whether advertising intensity covaries with help-seeking across jurisdictions and periods, complementing Study 2's within-person behavioral traces. We test whether state-level gambling advertising outlays track growth in problem-gambling help-seeking searches over and above state-quarter sports-betting legal status and demographic covariates. The advertising measure is broad: it covers gambling advertisers across multiple gambling categories and media channels. The legal-status control is narrow: it captures whether sports betting was legal in a given state and quarter. The design therefore asks whether advertising pressure adds information beyond a common U.S. legalization marker, not whether every form of gambling legality has been measured.

Building on the commercial determinants of health perspective and prior work on access and adaptation, we advance the following hypotheses:

H1: Problem-gambling help-seeking searches increase over time.

H2: Higher state-level gambling advertising spend is associated with higher levels of problem-gambling help-seeking searches, net of sports-betting legal status and demographic covariates.

H3: The association between advertising spend and problem-gambling help-seeking searches strengthens over time (Time × Ad Spend).

H4: Consistent with an access mechanism, state-quarter sports-betting legal status is associated with higher levels of problem-gambling help-seeking searches (prior evidence on access, adaptation, and availability is mixed; Storer et al., 2009; Zoglauer et al., 2021).

### 2.1 | Method

#### 2.1.1 | Identifying Gambling Advertisers

We analyzed advertising expenditures using Vivvix, an intelligence platform that tracks paid advertising across multiple media channels. Vivvix initially classified relevant firms under the broad label "Online & Mobile Games," so we conducted a content analysis to isolate advertisers primarily focused on gambling. This process identified 413 candidate advertiser entries (411 unique names) engaged in sports betting, casino games, lottery services, bingo, fantasy sports, financial betting, skill-based competitions, and hybrid platforms.

We extracted quarterly advertising-spend data for these advertisers from the first quarter of 2014 through the final quarter of 2023 (40 time periods per state), restricting the analysis to expenditures that could be geolocated to a specific U.S. state. Importantly, these are not digital-only data. Observed channels included the internet, local magazines, local radio, national spot radio, newspapers, outdoor, and spot television. Thus, the Vivvix measure captures multi-channel paid gambling advertising. Advertising spend was aggregated by state-quarter and divided by $100,000 to aid interpretation and model convergence. The analytic panel includes 41 states. Nine states had no state-geolocatable observations in the available Vivvix extract, and the extract does not distinguish verified zero spend from unassigned geography; we therefore do not impute zeros for those states.

### 2.1.2 | Measuring Problem Gambling via Google Trends

To investigate how gambling advertising relates to problem-gambling concern, we required a measure that could be tracked consistently over time and disaggregated by state. We therefore used Google Trends data as a proxy for problem-gambling help-seeking behavior, following prior research showing both the promise and the limits of search-volume data for tracking health-related concerns and behaviors (Arora et al., 2019; Mavragani et al., 2018; Nuti et al., 2014; Vaughan & Romero-Frías, 2014).

### 2.1.3 | Identifying Problem-Gambling Search Terms

We first generated a broad list of gambling-related queries through automated collection of terms associated with "gambling" and "betting." Two independent researchers then manually reviewed approximately 200 candidate queries for relevance to problem gambling and help seeking. Queries were excluded if they did not directly signify problem gambling or attempts to seek support. The final corpus of 116 terms included searches such as "how to stop gambling," "compulsive gambling treatment," "gambling addiction helpline," and "gambling debt."

### 2.1.4 | Data Extraction via the Google Trends API

We used the Google Trends API to collect quarterly search-volume indices (SVI) for these 116 terms by U.S. state from Q1 2014 to Q4 2023. The SVI is normalized between 0 and 100, where 100 indicates peak popularity for a term in a given region and period. Because Google imposes query limits, we used a batching approach anchored to a reference term ("gambling addiction") and rescaled the results with ratio-to-anchor normalization within batch. This produced a composite state-quarter search value index for problem-gambling help-seeking intensity. We also examined alternative composites, including mean-of-ratios and principal-components sensitivity checks.

### 2.1.5 | Covariates and State-Quarter Sports-Betting Legal Status

To control for demographic differences across states, we used 1-year American Community Survey estimates spanning 2014–2023. Variables included total population (in millions), median age, male percentage of the population, white percentage of the population, percentage with a bachelor's degree, and state median income (in $10,000 units). We also coded a binary state-quarter indicator of *sports-betting legal status* (0 = not legal; 1 = legal). This is a narrow operationalization of legal market status, not a comprehensive measure of overall gambling legalization. The coding corresponds to state-by-state sports-betting legal status as documented in American Gaming Association legal-status materials and annual market summaries (American Gaming Association, n.d., 2025). We include ACS covariates because these time-varying state characteristics can influence both operators' advertising allocation and residents' help-seeking search behavior.

### 2.1.6 | Analytic Strategy

We compiled a longitudinal dataset containing quarterly observations for each state from 2014 through 2023 ($n$ = 1,640 state-quarters). We estimated mixed-effects linear models with random intercepts for states to account for repeated measures. The outcome was the composite search value index for problem-gambling help-seeking searches. Key predictors were time (a continuous variable running from 0 to 39), advertising expenditure (in $100,000 units), and state-quarter sports-betting legal status. Continuous predictors were grand-mean centered before estimation. We tested the Time × Ad Spend interaction to assess whether the association between advertising and help-seeking changed over time. The panel includes 10 state-quarters with advertising spend above $18,500,000, so the reported models should be read with

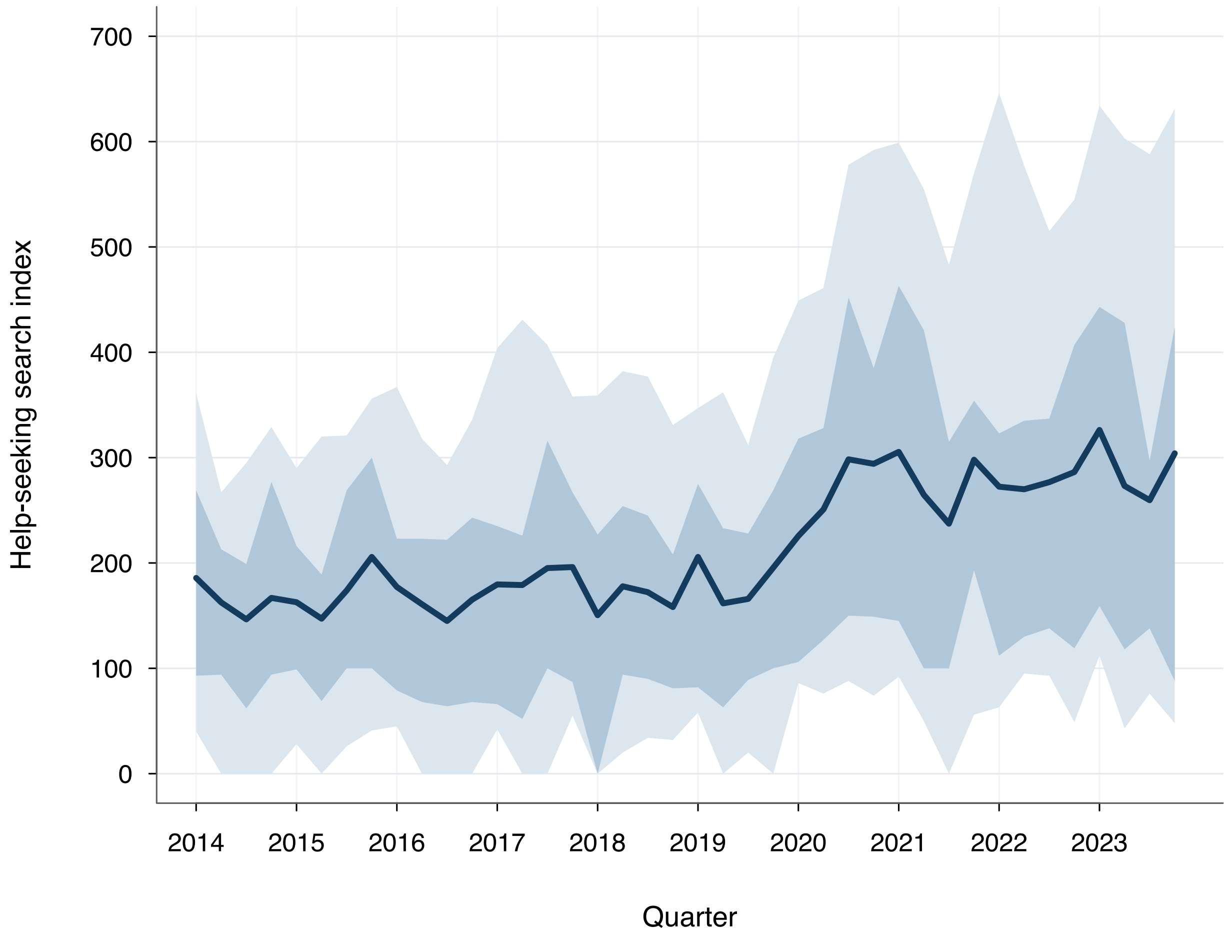


**FIGURE 1** | Quarterly state-level composite Google Trends help-seeking search index, 2014–2023. The solid line shows the quarterly mean across the 41-state panel; shaded bands show the interquartile range and the 10th–90th percentile range.

some leverage caution at the top of the spend distribution. Supplementary lag, placebo, and transformed-spend checks are reported in Appendix Table B5.

## 2.2 | Results

### 2.2.1 | Rising trends in problem-gambling help-seeking searches (H1)

Model estimates showed a significant positive effect of time on problem-gambling help-seeking search volume, indicating a general upward trend in gambling-related help-seeking queries ($b = 4.05$, $p < .001$ in Model 2). Figure 1 displays the rising quarterly pattern in the state-level composite search value index over the past decade.

### 2.2.2 | Advertising expenditures and problem-gambling help-seeking searches (H2)

Advertising expenditure was positively associated with problem-gambling help-seeking search volume in the models that introduced ad spend (Table 1, Models 3–4). This pattern supports H2 and is consistent with the expectation that higher advertising intensity is associated with more help-seeking interest at the population level even after accounting for state-quarter sports-betting legal status and demographic composition. Supplementary fixed-effects, lag, and placebo checks reported in Appendix Table B5 point in the same direction.

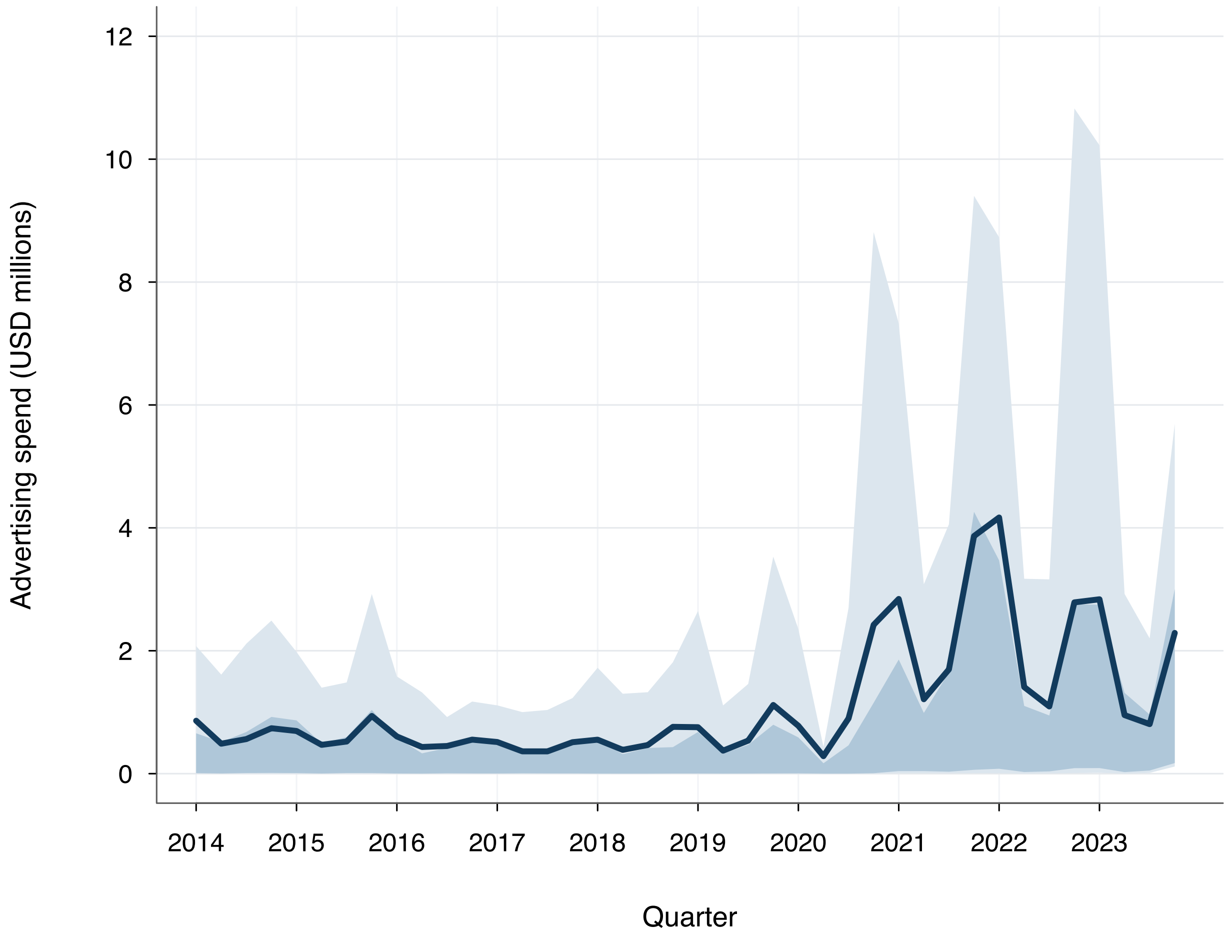


**FIGURE 2** | Quarterly state-level gambling advertising spend, 2014–2023. The solid line shows the quarterly mean across the 41-state panel in U.S. millions; shaded bands show the interquartile range and the 10th–90th percentile range.

### 2.2.3 | Advertising and help-seeking over time (H3)

H3 was supported. In the full interaction model, the relationship between advertising spend and problem-gambling help-seeking searches became stronger over the study period (Time × Ad Spend: $b = 0.08$, $p < .001$; Table 1, Model 5). Appendix Table B5 shows that this interaction is consistent across the supplementary sensitivity checks. Figure 2 provides descriptive context by showing the sharp rise in mean state-level gambling advertising outlays across the same period.

### 2.2.4 | Sports-betting legal status and help-seeking searches (H4)

H4 was not supported. Consistent with mixed prior evidence on access and adaptation, state-quarter sports-betting legal status did not emerge as a statistically significant predictor of problem-gambling help-seeking searches in the covariate-adjusted models (Model 4: $b = 4.81$, $SE = 10.23$, $p = .638$; Model 5: $b = 5.19$, $SE = 10.18$, $p = .610$; Table 1). States with legal sports betting did not exhibit systematically higher help-seeking search intensity once time, advertising, and covariates were considered.

## 3 | Study 2

Study 2 complements the state-level evidence by linking advertising exposure to gambling engagement and problem-gambling symptoms at the individual level. It pairs survey responses with passive digital traces to observe gambling-ad exposure within major mobile social platforms and to measure online gambling engagement in participants' digital behavior.

RealityMine records ad exposure only within the mobile Facebook, TikTok, and X environments captured in the trace data. Gambling marketing also occurs across television, web display, search, websites, affiliate channels, direct marketing, sponsorship, and other social or creator ecosystems (Fisher et al., 2025; Guillou-Landreat et al., 2021; Wardle et al., 2024). No valid denominator is available for the share of all gambling advertising represented by Facebook, TikTok, and X mobile-social exposure, so this measure should be treated as platform-specific rather than representative of total exposure. Even so, an ad seen in one feed can lead to a website visit, an app opening, or later gambling on another device. We therefore model broader engagement outcomes while treating observed exposure as a bounded measure of one delivery channel.
Accordingly, Study 2 tests the following hypotheses:
H5: Higher exposure to gambling advertisements within mobile social platforms is associated with higher odds of self-identifying as a gambler.
H6: Higher exposure to gambling advertisements within mobile social platforms is associated with higher self-reported gambling frequency.
H7: Higher exposure to gambling advertisements within mobile social platforms is associated with greater observed gambling-site engagement, reflected in a higher probability of any observed visit and greater visit volume among respondents with at least one visit.
H8: Among self-identified gamblers, higher exposure to gambling advertisements within mobile social platforms is associated with higher self-reported problem-gambling scores.
H9: Among self-identified gamblers, higher gambling engagement (observed gambling-site visits and self-reported gambling frequency) is associated with higher self-reported problem-gambling scores.

## 3.1 | Method

### 3.1.1 | Data Sources and Field Definitions

Study 2 used YouGov's Pulse Panel, which pairs survey responses with passive digital-trace data collected through RealityMine. RealityMine provides timestamped web logs, app-activity logs, and mobile social-feed records from consenting panelists. We use those logs to measure gambling-ad exposure within Facebook, TikTok, and X and to observe gambling-related web and app behavior during the study window. A small number of labeled Instagram gambling ads appeared in the social-media screening stage, but these were excluded so the exposure measure matches the platform scope analyzed here.

### 3.1.2 | Data Collection Procedures, Sample Size, and Weights

YouGov completed 1,251 interviews and matched 1,100 respondents into the final survey dataset. The completed-interview count was not a prespecified power target. Our Study 2 analytic sample is the subset of those 1,100 respondents with supported-platform eligibility during the observation window. The app-visit and social-trace records make it possible to identify that eligibility rule based on observed presence in the Facebook/Facebook Lite/Messenger, TikTok, or X mobile environments during the study window. Applying that rule yields 419 respondents with valid target-platform ad-capture eligibility. Regression models use 404 respondents because 15 otherwise eligible respondents did not report household income, which is included as a covariate in every specification; no eligible cases were lost on age, sex, race, education, gambling-frequency items, or survey weights. Problem-gambling models are limited to the 193 eligible respondents who self-identified as gamblers, of whom 185 remain after the same income-item nonresponse.
No preregistration or a priori power analysis was available. The achieved analytic sample was set by the documented vendor fielding and matching process, together with the study's supported-platform eligibility rule, rather than by a documented pre-specified power target. All weighted models use YouGov's survey weight. YouGov's study documentation indicates that respondents were matched to a politically representative modeled frame on gender, age, race, and education, then weighted to that frame using propensity scores. The final weight further post-stratified matched cases on 2020 presidential vote choice and on gender, age, race, and education. These adjustments improve alignment with population benchmarks, but the sample remains a restricted nonprobability panel and weighting cannot remove all selection bias.

### 3.1.3 | Identifying Gambling Advertisements

To classify which advertisements qualified as gambling advertising, we used criteria specifying that any advertisement referencing real-money wagers, betting odds, payouts, or directing users to platforms enabling such behavior would be designated as a gambling ad. Ads mentioning sports-betting lines, casino promotions, poker tournaments with cash prizes, lottery opportunities, fantasy sports with cash prizes, or financial betting platforms were included. Purely free-to-play games were excluded unless they implied a conversion to real-money prizes.

### 3.1.4 | Identifying Gambling Websites

To identify gambling websites in the RealityMine data, we began with domains associated with the gambling advertisers identified from the Vivvix platform. We then expanded that candidate list through iterative searches and manually reviewed the resulting domains against predefined coding criteria. Two independent researchers assessed whether each candidate domain explicitly enabled or promoted real-money wagers or skill-based gaming with cash payouts. This process produced 333 unique, functional gambling domains used to label observed web interactions.

### 3.1.5 | Social Media and Video Browsing Data Annotation

The RealityMine social-media and video-browsing data contained large numbers of free-text descriptions that were inconsistently encoded. Several large language models were benchmarked against human-coded labels on an initial balanced set of 200 posts. Qwen 2.5-32B performed best in that benchmark (Cohen's $\kappa = .74$ against human labels) and was then used as the base for a fine-tuned 32B screening model. All machine-predicted positives were manually reviewed and assigned final gambling categories by a researcher before aggregation.

Within the 419 eligible respondents, observed gambling-ad exposure averaged 15.51 encounters ($SD = 50.96$). Observed gambling-web visits averaged 16.87 visits ($SD = 140.02$). App-visit logs were used to identify supported-platform eligibility, but gambling-app visits were not treated as a primary outcome in the hypothesis tests reported here.

### 3.1.6 | Self-Reported Data and Measures

The exposure-eligible Study 2 sample had a mean age of 47.76 years ($SD = 14.00$); 56.8% were female, 66.8% identified as white, 27.0% reported at least a bachelor's degree, and 19.3% reported household income at or above $100,000. The traced analytic sample is not a youth-skewed sample: 9.5% of exposure-eligible respondents were ages 18–29, while 33.7% were ages 55 or older. Forty-six percent of eligible respondents self-identified as gamblers. Among the gambler-only analytic sample ($n = 185$), the observed problem-gambling score averaged 2.01 ($SD = 1.24$), and 18.4% reported gambling at least monthly.

Respondents who identified as gamblers completed an adapted eight-item self-reported problem-gambling symptom inventory derived from Moore and Ohtsuka (1997). The problem-gambling score is defined as the mean of PGAM1–PGAM8 on the original 1–7 response scale. We refer to this outcome as a *problem-gambling score* rather than a diagnostic measure. The measure is a legacy adapted symptom index rather than DSM-5 criteria or the Problem Gambling Severity Index. The survey grid introduced these items with the prompt, "Please answer the following questions as honestly as possible," and used a 1–7 agree-disagree scale. The final two rows were sports-gambling-specific: "Sports gambling apps make gambling too easy for me" and "Sports betting." The item-level data also permit a fuller psychometric assessment. Across all survey respondents who received the battery and had complete item responses ($n = 464$), the eight items showed high internal consistency ($\alpha = .89$; $\omega = .90$), corrected item-total correlations ranging from .55 to .76, and a dominant first factor (first eigenvalue = 4.75; one-factor loadings = .57 to .81). Results were similar when the sports-gambling-app item (PGAM7) was excluded ($\alpha = .90$). In the analytic gambler subsample, a sensitivity model excluding that item yielded the same substantive conclusions as the main models reported below. Excluding PGAM8 gave an H8 ad-exposure coefficient of $b = 0.0014$ ($p = .056$), while the H9 site-visits coefficient remained null ($b = 0.00004$, $p = .825$) and the H9 gambling-frequency coefficient remained positive ($b = 0.178$, $p < .001$). We treat the score as a useful symptom indicator, while acknowledging that it is not a formal diagnostic screen.

### 3.1.7 | Analytic Strategy

Study 2 outcomes have different data-generating forms, so we use model families tailored to each outcome. We estimate weighted logistic regression for gambler status (H5), weighted ordered logit for self-reported gambling frequency (H6), weighted hurdle negative binomial models for observed gambling-site visits (H7), and weighted gamma regressions with log links for the positive, right-skewed problem-gambling score (H8–H9). The hurdle model separates the probability of any observed site visit from visit volume among respondents with at least one visit. All models adjust for age, sex, race, education, and income and apply the YouGov survey weight described above.

#### 3.1.8 | Temporal Activation and Observed Touchpoints

The cross-sectional trace models show that heavier observed advertising exposure is associated with broader gambling engagement, but they do not show whether ads precede short-run gambling activity. To test that time order, we reconstructed full timestamps for labeled gambling ads by linking those ad records to the underlying RealityMine Facebook, TikTok, and X sponsored-post records using shared event fields and normalized clock times. We then paired those ad timestamps with timestamped gambling-web and gambling-app traces.

The temporal extension focuses on *clean* sponsored exposures, defined as social ad events with no observed gambling web or app touchpoint in the prior 24 hours. Non-clean exposures are sponsored exposures observed after a gambling web or app touchpoint in the preceding 24 hours; the clean-exposure restriction removes those post-touchpoint cases from the temporal test. This restriction is intended to reduce obvious retargeting or same-session endogeneity. For the user fixed-effects comparison set, we kept all clean gambling-ad exposures and the first 100 clean non-gambling sponsored exposures per user, ordered chronologically, so that ordinary sponsored content on the same platforms serves as the comparison condition without letting highly active feed users dominate the estimation sample. We then estimated user fixed-effects logistic models with platform controls for whether a clean sponsored exposure was followed by any observed gambling touchpoint within 24 hours, by a gambling-web touchpoint within 24 hours, or by a gambling-app touchpoint within 24 hours. Appendix Table B7 reports an additional sensitivity that adds hour-of-day-bin and day-of-week controls to the 24-hour models.

To connect this temporal test to harm-related symptoms, we also aggregated clean gambling-ad exposures to the respondent level. Among eligible gamblers, we counted how many clean gambling-ad exposures were followed by any gambling touchpoint within 24 hours and entered that triggered-touchpoint count into a weighted gamma model of the self-reported problem-gambling score alongside the total number of clean gambling-ad exposures and the same sociodemographic covariates used in the main H8–H9 models.

### 3.2 | Results

#### 3.2.1 | Digital gambling ad exposure and gambling engagement (H5–H7)

H5 and H6 were supported. Gambling-ad exposure on mobile social media was positively associated with self-identifying as a gambler (weighted logistic regression: $b = 0.011$, $SE = 0.004$, $p = .008$) and with higher ordered self-reported gambling frequency (weighted ordered logit: $b = 0.006$, $SE = 0.002$, $p = .008$), controlling for sociodemographics (Table 2). The ordered outcome is heavily concentrated in the lower categories, with only 34 respondents in the top three frequency levels combined, so we interpret H6 as evidence of movement toward higher reported gambling-frequency categories. Additional model checks are reported in Appendix Table B4.

H7 received partial support. In the weighted hurdle model, gambling-ad exposure was positively associated with the probability of any observed gambling-site visit (logit component: $b = 0.013$, $SE = 0.004$, $p < .001$), but not with visit volume among respondents who visited at least one gambling site (count component: $b = 0.022$, $SE = 0.021$, $p = .284$). We place more weight on the any-visit component than on the positive-count component. Table 3 reports the full hurdle-model estimates; Appendix Table B4 reports additional model checks.

#### 3.2.2 | Digital gambling ad exposure and self-reported problem-gambling score (H8)

H8 was supported in the weighted gamma model estimated among self-identified gamblers ($n = 185$). Higher gambling-ad exposure was associated with higher self-reported problem-gambling scores ($b = 0.0015$, $SE = 0.0007$, $p = .048$), controlling for sociodemographics. The association is modest. When PGAM8 was excluded, the ad-exposure coefficient was $b = 0.0014$ and $p = .056$. Table 4 reports the full estimates, and Appendix Table B4 reports the corresponding model checks.

#### 3.2.3 | Gambling engagement and self-reported problem-gambling score (H9)

H9 was supported only for self-reported gambling frequency. Among gamblers, self-reported gambling frequency was positively associated with higher problem-gambling scores in both the single-predictor model ($b = 0.169$, $SE = 0.041$, $p < .001$) and the combined model ($b = 0.169$, $SE = 0.041$, $p < .001$). Observed gambling-site visits were not associated with higher problem-gambling scores in either the single-predictor model ($b = 0.00006$, $SE = 0.00021$, $p = .771$) or the combined model ($b = 0.00007$, $SE = 0.00020$, $p = .740$). Table 4 reports the full estimates, and Appendix Table B4 reports the corresponding model checks.

### 3.2.4 | Temporal activation and downstream symptom linkage

The timestamped analyses add behavioral timing to Study 2. In user fixed-effects logistic models comparing clean gambling-ad exposures with clean non-gambling sponsored exposures on the same platforms, gambling ads were associated with higher odds of any observed gambling touchpoint within 24 hours ($b = 0.416$, $SE = 0.106$, $p < .001$) and higher odds of an observed gambling-web touchpoint within 24 hours ($b = 0.872$, $SE = 0.133$, $p < .001$; Table 5). By contrast, an app-specific 24-hour model was not statistically distinguishable from zero ($b = -0.278$, $SE = 0.179$, $p = .120$). These models use 9,140 clean sponsored exposures from 74 users who experienced both clean gambling and clean non-gambling sponsored exposures, so the comparison is not between ad days and no-ad days but between gambling and non-gambling sponsored content in the same users' feeds. Additional timing-control checks are reported in Appendix Table B7.

The event counts point in the same direction. Across 4,621 clean gambling-ad exposures from 208 users, 8.3% were followed by any observed gambling touchpoint within 24 hours. When a downstream touchpoint occurred, the first observed action was more often a gambling-web visit than a gambling-app visit (286 vs. 96 events; Appendix Table B6). Only seven clean gambling-ad exposures showed a web-to-app chain within 24 hours. The trace data therefore show the clearest short-run routing into gambling websites rather than immediate app openings.

At the respondent level, we then tested whether this short-run activation count was related to symptoms. Among eligible gamblers ($n = 185$), the count of clean gambling-ad exposures that were followed by any gambling touchpoint within 24 hours was positively associated with higher problem-gambling scores even when total clean gambling-ad volume was entered simultaneously (triggered-touchpoint count: $b = 0.032$, $SE = 0.010$, $p = .001$; total clean ad count: $b = -0.001$, $SE = 0.001$, $p = .430$; Table 5). In this extension, the key distinction was not simply how many clean gambling ads a respondent saw, but how often those ads were followed by gambling behavior.

**Table 5**
*Temporal activation and symptom models (Study 2)*

| Outcome | Predictor | *b* (*SE*) | *p* | *N* |
|---|---|---|---|---|
| 24-hour observed gambling touchpoint after clean sponsored exposure | Gambling ad (vs. clean non-gambling sponsored exposure) | 0.416 (0.106) | <.001 | 9,140 |
| 24-hour observed gambling-web touchpoint after clean sponsored exposure | Gambling ad (vs. clean non-gambling sponsored exposure) | 0.872 (0.133) | <.001 | 9,140 |
| 24-hour observed gambling-app touchpoint after clean sponsored exposure | Gambling ad (vs. clean non-gambling sponsored exposure) | -0.278 (0.179) | .120 | 9,140 |
| Problem-gambling score among eligible gamblers | Triggered touchpoints within 24 hours | 0.032 (0.010) | .001 | 185 |
| Problem-gambling score among eligible gamblers | Total clean gambling-ad exposures | -0.001 (0.001) | .430 | 185 |

*Note:* The first three rows report user fixed-effects logistic models with platform controls, estimated on 9,140 clean sponsored exposures from 74 users who experienced both clean gambling and clean non-gambling sponsored exposures. "Clean" means no observed gambling web or app touchpoint in the prior 24 hours. To avoid domination by users with thousands of ordinary sponsored posts, the control set retains the first 100 clean non-gambling sponsored exposures per user, ordered chronologically, while keeping all clean gambling-ad exposures. The last two rows report a weighted gamma model among eligible gamblers using the PGAM1–PGAM8 mean score; the model includes both activation variables simultaneously and adjusts for age, sex, race, education, and income.

## 4 | General Discussion

The two studies point to the same conclusion. In Study 1, advertising intensity was positively associated with problem-gambling help-seeking searches, whereas state-quarter sports-betting legal status was not; the supplementary lag models indicate that the advertising signal is concentrated mainly in the same quarter rather than in a long transfer effect. In Study 2, platform-specific gambling-ad exposure was associated with gambler status, higher self-reported gambling frequency, the probability of any observed gambling-site visit, and higher self-reported problem-gambling scores. The temporal extension further shows that clean gambling ads were more likely than clean non-gambling sponsored posts to be followed by short-run gambling touchpoints, and that ad-triggered touchpoints were more closely related to problem-gambling scores than raw clean ad volume alone. Together, these results support an advertising-centered account rather than a claim that sports-betting legal status alone explains harm-related patterns.

Study 2 also narrows the behavioral interpretation. Exposure was associated with participation-oriented outcomes more consistently than with downstream behavioral intensity. Higher ad exposure was linked to self-identifying as a gambler, reporting more frequent gambling, and crossing the hurdle into at least one observed gambling-site visit. When we isolate clean sponsored exposures and compare gambling ads with other sponsored content in the same users' feeds, the strongest signal appears within 24 hours and runs mainly through subsequent gambling-web touchpoints, not through app openings. The pattern is consistent with a higher likelihood of a later web-based gambling action after exposure without an immediate same-session app opening. Among gamblers, problem-gambling scores track how often clean gambling ads are followed by downstream gambling behavior more closely than they track raw clean ad volume. This sequence is consistent with advertising exposure, behavioral activation, and symptoms occurring along the same pathway, while also making clear that the trace measure does not capture every route from exposure to harm.

Both studies are observational. Study 1 uses a proxy outcome based on help-seeking search behavior rather than treatment records or clinical diagnoses, and a small number of very high-spend quarters have disproportionate influence in the state-panel models. The Study 1 relationship is concentrated mainly in same-quarter exposure rather than in a long transfer effect. Study 2 measures mobile-social advertising exposure on three platforms, relies on a restricted nonprobability panel, and was not accompanied by an a priori power analysis. The Study 2 traced analytic sample includes 9.5% ages 18–29 and 33.7% ages 55 or older, so the trace-based estimates should not be read as youth-specific evidence. The ordered-frequency outcome is sparse at the upper end, and the positive-count component of the H7 hurdle model is based on only 114 respondents with any observed site visit and an extremely skewed count distribution, so inferences about visit intensity are weaker than inferences about crossing the any-visit hurdle. The temporal extension reduces some obvious endogeneity by comparing clean gambling ads with clean non-gambling sponsored posts within the same users, but it still cannot rule out rapidly changing latent intent or platform targeting rules that operate inside the 24-hour window. Additional event-window checks indicate broader temporal clustering around nominally clean gambling-ad exposures, so the temporal models should be interpreted as evidence of short-run activation rather than as a rebound-free design. The strongest temporal signal was on a 24-hour horizon and mainly through web touchpoints rather than immediate app openings. The problem-gambling outcome is an adapted self-report symptom score rather than a DSM-5-aligned diagnostic assessment or a PGSI score. Because PGAM8 is a sports-betting-specific item, we also estimated the problem-gambling model without it; the H8 ad-exposure coefficient was $b = 0.0014$ with $p = .056$. Future trace-based studies should field the Problem Gambling Severity Index alongside passive measures to provide a widely recognized severity benchmark and improve cross-study comparability (Orford et al., 2010). Appendix Tables B4, B5, and B7 report the full sensitivity analyses. These constraints call for caution, but they do not erase the policy relevance of the observed advertising-to-engagement association.

Online gambling policy cannot be reduced to market access alone. Across both studies, advertising intensity and exposure were associated with harm-related searches or gambling engagement after sports-betting legal status was taken into account. That conclusion is consistent with the commercial-determinants perspective, which emphasizes how firms shape demand and policy environments, and with platform-governance research, which emphasizes that intermediaries structure visibility, targeting, and enforcement (DeNardis & Hackl, 2015; Gorwa, 2019; Kickbusch et al., 2016; Mialon, 2020). It is also consistent with recent reviews concluding that advertising policy is a plausible and important lever for reducing gambling harms, even where stronger evaluation designs are still needed (Fisher et al., 2025; McGrane et al., 2023).

### 4.1 | Public Policy Implications

These findings support stronger restrictions on gambling advertising and other high-risk marketing practices. From a public-health perspective, comprehensive or near-comprehensive bans on *gambling advertising* are already on the policy menu internationally and are defensible options where legally feasible. Comparative evidence shows that jurisdictions are increasingly experimenting with broad marketing restrictions, inducement controls, and system-level approaches rather than relying only on individual responsibility messaging (Aonso-Diego et al., 2025; García-Pérez et al., 2024; Ukhova et al., 2024). The same logic underlies contemporary calls to better protect children and young people from pervasive gambling marketing (Thomas et al., 2023).

In the United States, however, full bans on truthful advertising for lawful gambling face more serious commercial-speech constraints than in some other jurisdictions. *Central Hudson Gas & Electric Corp. v. Public Service Commission of New York* (1980) requires the government to show that restrictions on lawful commercial speech directly advance a substantial interest and are not more extensive than necessary. *44 Liquormart, Inc. v. Rhode Island* (1996) and *Lorillard Tobacco Co. v. Reilly* (2001) reinforce that broad speech restraints on lawful products face substantial constitutional scrutiny. Our results are relevant to the "direct advancement" question because they show that heavier gambling advertising is associated with greater help-seeking search activity and greater gambling engagement. But because the evidence is observational rather than experimental, it is better read as support for stronger restrictions than as a definitive legal demonstration that any one policy will satisfy constitutional review.

That legal context makes targeted limits especially important. In the U.S. setting, the most immediately defensible path is a package of restrictions on advertising volume, placement, timing, content, inducements, and targeting, alongside prominent safer-gambling disclosures and stronger oversight of affiliate and social-platform marketing. Inducements deserve particular attention because "free bet" and bonus framing can mislead consumers about actual cost and risk

(Challet-Bouju et al., 2020; Hing et al., 2019). Enforcement must also account for illegal or offshore marketing and affiliate ecosystems, which can route users around formal restrictions and complicate attribution and sanctioning (Australian Communications and Media Authority, 2022; European Commission, 2018; Fisher et al., 2025). Policy tools such as advertiser verification, public ad libraries, website and payment blocking where lawful, and better access for independent researchers to ad-delivery data could improve both enforcement and evaluation.

Future research should build on these results with stronger temporal designs, platform-specific audits, and better exposure transparency. Work that distinguishes advertising content, inducement structure, targeting strategy, and delivery context would help policymakers identify which practices are most harmful. The policy implication is concrete: gambling advertising is not passive information about a lawful product; it is part of the infrastructure that makes wagering visible, timely, and easy to initiate. Advertising bans or near-bans warrant serious consideration where legally feasible, and in the United States, stronger targeted restrictions on gambling advertising are justified now.

## ETHICS APPROVAL

This research was reviewed under the authors' institutional review board procedures. Study 1 analyzed secondary, state-aggregated advertising and Google Trends data and was treated as non-human-subjects research. Study 2 involved consenting adult panelists whose mobile and web activity were passively observed via YouGov's RealityMine platform. Study 2 was covered by exempt IRB protocol #24-0685, approved October 31, 2024. Data were de-identified prior to analysis and access was restricted to the research team.

## CONSENT TO PARTICIPATE

All Study 2 participants were adults and provided informed consent to YouGov/RealityMine for passive digital-trace collection and survey linkage. Participants could withdraw from panel participation under the vendor's standard procedures, and no minors were enrolled in the traced sample used here.

## DECLARATION OF CONFLICTING INTEREST

The authors declare no conflicts of interest related to this research. No author has received funding, consulting income, or other consideration from gambling operators, advertising platforms, or industry associations in connection with this study.


## FUNDING STATEMENT

This project received no external funding.


## DATA AVAILABILITY STATEMENT

The raw Vivvix advertising microdata and YouGov/RealityMine event-level logs cannot be posted publicly because of licensing restrictions and participant-privacy protections. Derived non-identifiable analytic materials sufficient to reproduce the reported tables and figures are available from the corresponding author on reasonable request. Access to the original commercial sources is available directly from Vivvix/Kantar and YouGov/RealityMine under their standard research agreements.

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**Table 1**
*Mixed-effects models showing the relationship between advertising spend and the composite Google Trends search value index for problem-gambling help-seeking queries (Study 1)*

| | Model 1 | Model 2 | Model 3 | Model 4 | Model 5 |
|---|---|---|---|---|---|
| **Fixed Effects** | ***b* (*SE*)** | ***b* (*SE*)** | ***b* (*SE*)** | ***b* (*SE*)** | ***b* (*SE*)** |
| Intercept | 214.49 (20.13) | 214.49 (20.13) | 214.49 (17.90) | 213.79 (5.96) | 208.07 (6.46) |
| Time | | 4.05 (0.24) | 3.43 (0.24) | 2.00 (0.44) | 2.46 (0.46) |
| Ad Spend ($100,000 Units) | | | 1.24 (0.11) | 1.23 (0.11) | 0.21 (0.20) |
| Time × Ad Spend | | | | | 0.08 (0.01) |
| Sports-Betting Legal Status | | | | 4.81 (10.23) | 5.19 (10.18) |
| State Population (Millions) | | | | 13.82 (0.82) | 14.69 (0.89) |
| State Median Age | | | | 2.47 (2.85) | 2.10 (3.03) |
| State Male Population % | | | | 9.30 (10.66) | 9.58 (11.25) |
| State White % | | | | -0.96 (0.45) | -0.95 (0.47) |
| State Median Income ($10,000) | | | | 15.31 (9.39) | 13.49 (9.71) |
| State Bachelor Degree % | | | | -1.82 (3.34) | -1.76 (3.48) |
| | | | | | |
| **Random Effects** | **Variance** | **Variance** | **Variance** | **Variance** | **Variance** |
| $\sigma^2$ | 14860.11 | 12622.40 | 11795.69 | 11610.55 | 11332.23 |
| $\tau_{00}$ State | 16235.04 | 16290.98 | 12838.43 | 1076.24 | 1297.10 |
| ICC | .52 | .56 | .52 | .08 | .10 |
| Marginal/Conditional $R^2$ | .00/.52 | .07/.59 | .13/.58 | .59/.63 | .59/.64 |
| Observations | 1640 | 1640 | 1640 | 1640 | 1640 |
| Groups | 41 | 41 | 41 | 41 | 41 |

Predictors grand-mean centered; advertising spend covers internet and traditional paid media; state population is expressed in millions and state median income in $10,000 units; the panel is restricted to states with geolocatable spend in the available Vivvix extract; standard errors in parentheses.

**Table 2**
*Weighted models showing the relationship between digital gambling-ad exposure, gambler status, and self-reported gambling frequency (Study 2)*

| Predictor | H5 Logistic *b* (*SE*) | H6 Ordered Logit *b* (*SE*) |
|---|---|---|
| Gambling-Ad Exposure | 0.011 (0.004) | 0.006 (0.002) |
| Age | -0.002 (0.007) | -0.004 (0.007) |
| Sex (1 = Female) | 0.196 (0.220) | 0.035 (0.207) |
| Race (1 = White) | -0.023 (0.221) | -0.120 (0.210) |
| Education Level | -0.054 (0.083) | -0.101 (0.079) |
| Income | -0.026 (0.035) | -0.013 (0.033) |
| Observations | 404 | 404 |

*Note:* Both models apply the YouGov survey weight. H6 is estimated as ordered logit because gambling frequency is a six-category ordered outcome. Ordered-logit thresholds are omitted for brevity; threshold-specific binary-logit checks are reported in Appendix Table B4. Non-exponentiated coefficients are reported; standard errors are in parentheses.

**Table 3**
*Weighted hurdle model showing the relationship between digital gambling-ad exposure and observed gambling-site visits (Study 2, H7)*

| Predictor | Any Visit Logit *b* (*SE*) | Positive Visit Count *b* (*SE*) |
|---|---|---|
| Gambling-Ad Exposure | 0.013 (0.004) | 0.022 (0.021) |
| Age | -0.010 (0.009) | -0.067 (0.029) |
| Sex (1 = Female) | 0.548 (0.262) | -0.394 (1.078) |
| Race (1 = White) | -0.548 (0.253) | -1.212 (0.784) |
| Education Level | 0.010 (0.096) | -0.796 (0.319) |
| Income | -0.021 (0.040) | 0.093 (0.099) |
| log($\theta$) | | -12.963 (95.474) |
| Observations | 404 | 114 |

*Note:* The hurdle specification separates the probability of any observed gambling-site visit from visit volume among respondents with at least one visit. Both components apply the YouGov survey weight. The positive-count component is estimated on 114 respondents with any observed visit, and the dispersion estimate is unstable; Appendix Table B4 reports model-family sensitivity checks. Non-exponentiated coefficients are reported; standard errors are in parentheses.

**Table 4**
*Weighted gamma models showing the relationship between advertising exposure, gambling engagement, and self-reported problem-gambling score among gamblers (Study 2)*

| Predictor | H8 Ad Exposure | H9a Site Visits | H9b Gambling Frequency | H9c Combined |
|---|---|---|---|---|
| Gambling-Ad Exposure | 0.0015 (0.0007) | | | |
| Observed Gambling-Site Visits | | 0.00006 (0.00021) | | 0.00007 (0.00020) |
| Self-Reported Gambling Frequency | | | 0.169 (0.041) | 0.169 (0.041) |
| Age | -0.009 (0.003) | -0.009 (0.003) | -0.008 (0.003) | -0.008 (0.003) |
| Sex (1 = Female) | 0.082 (0.093) | 0.058 (0.096) | 0.092 (0.089) | 0.089 (0.090) |
| Race (1 = White) | 0.025 (0.091) | 0.012 (0.093) | 0.054 (0.087) | 0.055 (0.088) |
| Education Level | 0.113 (0.037) | 0.108 (0.038) | 0.136 (0.036) | 0.137 (0.036) |
| Income | -0.020 (0.015) | -0.019 (0.016) | -0.028 (0.015) | -0.028 (0.015) |
| Observations | 185 | 185 | 185 | 185 |

*Note:* All models apply the YouGov survey weight and use gamma regressions with log links. The problem-gambling outcome is the mean of PGAM1–PGAM8 on the original 1–7 response scale. A sensitivity model that excluded the sports-gambling-app item (PGAM7) yielded the same positive H8 ad-exposure association and the same null/positive H9 pattern for site visits and gambling frequency. A separate sensitivity excluding PGAM8 produced $b = 0.0014$ and $p = .056$ for the H8 ad-exposure coefficient; H9 site visits remained null ($b = 0.00004$, $p = .825$), and H9 gambling frequency remained positive ($b = 0.178$, $p < .001$). Appendix Table B4 reports additional family-sensitivity checks. Non-exponentiated coefficients are reported; standard errors are in parentheses.

## APPENDIX

### A | Gambling Definition and Categories (Study 2)

A piece of content is classified as **gambling/betting** if it directly or indirectly promotes **real-money wagers** (or other items of value) on events or games involving chance or skill, with the possibility of winning **cash or material rewards**. This includes ads that do one or more of the following:

1. **Mentions betting or wagering**, including phrases such as "bet now," "win real cash," or explicit displays of odds, lines, or payouts.
2. **Encourages participation in gambling services**, including links or prompts that direct users to platforms where real-money wagering is enabled or promoted.
3. **Promotes a gambling category**, including sports betting, casino games, lottery games, bingo, fantasy sports with cash prizes, financial betting, skill-based games with cash prizes, or hybrid platforms.

### A.1 | Exclusions

- Ads for purely free-to-play games with no real-money or prize-based outcomes are **not** gambling ads unless they include or imply a path to real cash or prizes.
- General entertainment ads (for example, ads for hotels that happen to contain casinos) are excluded unless they explicitly promote wagering.

### B | Observed Ad Exposure and Site-Visit Counts (Study 2)

**Table B1**

*Observed gambling advertising exposures by category in the exposure-eligible sample (Study 2, n = 419)*

| Category | Summed Exposures |
|---|---|
| Sports Betting | 2394 |
| Casino Games | 2359 |
| Skill-based Games | 683 |
| Lottery Games | 310 |
| Financial Betting | 309 |
| Fantasy Sports | 190 |
| Hybrid Platforms | 163 |
| Bingo | 90 |

**Table B2**

*Observed gambling-web visits by category in the exposure-eligible sample (Study 2, n = 419)*

| Category | Summed Visits |
|---|---|
| Sports Betting | 6498 |
| Lottery Games | 517 |
| Bingo | 47 |
| Casino Games | 4 |
| Hybrid Platforms | 3 |

### C | Problem-Gambling Inventory Wording and Psychometrics (Study 2)

The adapted problem-gambling outcome used the following item wording:

1. **To some extent, I have a gambling problem** (1 = strongly disagree, 7 = strongly agree)
2. **At times I feel guilty about my level of gambling** (1 = strongly disagree, 7 = strongly agree)
3. **Sometimes I try to keep the amount I gamble secret from family or friends** (1 = strongly disagree, 7 = strongly agree)
4. **I have at times gambled more than I meant to** (1 = strongly disagree, 7 = strongly agree)
5. **I would like to cut down my level of gambling but it's difficult** (1 = strongly disagree, 7 = strongly agree)
6. **On occasions I have borrowed money to gamble or pay gambling debts** (1 = strongly disagree, 7 = strongly agree)
7. **Sports gambling apps make gambling too easy for me** (1 = strongly disagree, 7 = strongly agree)
8. **Sports betting** (1 = strongly disagree, 7 = strongly agree)

**Table B3**

*Psychometric properties of the adapted eight-item problem-gambling battery among survey respondents with complete item responses (n = 464)*

| Item | Corrected Item-Total $r$ | One-Factor Loading ($\lambda$) |
|---|---|---|
| PGAM1 | .749 | .806 |
| PGAM2 | .755 | .811 |
| PGAM3 | .699 | .749 |
| PGAM4 | .638 | .675 |
| PGAM5 | .759 | .815 |
| PGAM6 | .668 | .714 |
| PGAM7 | .550 | .572 |
| PGAM8 | .655 | .692 |
| $\alpha$ | .891 | |
| $\omega_{\text{total}}$ | .902 | |
| First Eigenvalue | 4.748 | |

**Table B4**

*Model diagnostics and sensitivity checks for Study 2*

| Check | Specification | Ad-Exposure $b$ ($SE$) | $p$ |
|---|---|---|---|
| H6 threshold logit | Frequency > 1 | 0.011 (0.004) | .008 |
| H6 threshold logit | Frequency > 2 | 0.005 (0.002) | .025 |
| H6 threshold logit | Frequency > 3 | 0.004 (0.003) | .133 |
| H6 threshold logit | Frequency > 4 | 0.004 (0.003) | .129 |
| H6 threshold logit | Frequency > 5 | 0.004 (0.015) | .815 |
| H7 hurdle NB | Any-visit component | 0.013 (0.004) | <.001 |
| H7 hurdle NB | Positive-count component | 0.022 (0.021) | .284 |
| H7 hurdle Poisson | Any-visit component | 0.013 (0.004) | <.001 |
| H7 hurdle Poisson | Positive-count component | 0.002 (0.0001) | <.001 |
| H7 log-normal | OLS on log positive visits | 0.003 (0.002) | .102 |
| H8 gamma | Log-link GLM | 0.0015 (0.0007) | .048 |
| H8 inverse Gaussian | Log-link GLM | 0.0028 (0.0011) | .011 |
| H8 log-normal | OLS on log score | 0.0008 (0.0006) | .236 |
| H9 gamma | Site visits in combined model | 0.00007 (0.00020) | .740 |
| H9 gamma | Gambling frequency in combined model | 0.169 (0.041) | <.001 |
| H9 inverse Gaussian | Site visits in combined model | 0.00007 (0.00021) | .720 |
| H9 inverse Gaussian | Gambling frequency in combined model | 0.177 (0.046) | <.001 |
| H9 log-normal | Site visits in combined model | 0.00013 (0.00017) | .466 |
| H9 log-normal | Gambling frequency in combined model | 0.162 (0.036) | <.001 |

*Note:* Threshold logits use the same covariates and weights as the ordered-logit model, but the upper gambling-frequency categories are sparse. For H7, the positive-count distribution is extremely skewed; the hurdle Poisson fit was much worse than the hurdle negative-binomial fit (AIC = 24,554.2 vs. 1,153.7), and a zero-inflated negative-binomial alternative was numerically unstable for the key exposure term. Greater substantive weight is therefore placed on the any-visit component than on the positive-count component. For H8/H9, the ad-exposure association remains positive across alternative families but is statistically sensitive to the assumed outcome distribution, whereas the H9 pattern remains stable: self-reported gambling frequency stays positive and observed site visits remain null across gamma, inverse-Gaussian, and log-normal specifications.

## D | Study 1 Lag and Carryover Robustness Checks

**Table B5**

*Study 1 lag, placebo, and leverage sensitivity checks with state and quarter fixed effects*

| Check | Specification | $b$ ($SE$) | $p$ |
|---|---|---|---|
| Mixed-effects AR(1) | Time × Ad Spend | 0.076 (0.016) | <.001 |
| Contemporaneous FE | Current-quarter ad spend | 1.121 (0.149) | <.001 |
| Distributed lag FE | Current-quarter ad spend | 1.055 (0.132) | <.001 |
| Distributed lag FE | One-quarter lag | 0.080 (0.093) | .398 |
| Distributed lag FE | Two-quarter lag | 0.160 (0.114) | .169 |
| Distributed lag FE | Cumulative spend, quarters $t$ to $t-2$ | 1.294 (0.150) | <.001 |
| Placebo-lead FE | One-quarter lead | 0.081 (0.181) | .658 |
| Log-spend FE | Cumulative log spend, quarters $t$ to $t-2$ | 18.172 (10.897) | .103 |
| Winsorized FE | Cumulative spend, quarters $t$ to $t-2$ | 1.752 (0.283) | <.001 |

*Note:* The first row reports the full mixed-effects interaction model with an AR(1) error structure ($\phi = .30$). The remaining models use the 41-state panel and include state and quarter fixed effects, state-clustered standard errors, state-quarter sports-betting legal status, and the same time-varying state covariates as the main Study 1 models (population size, median age, male share, white share, median income, and bachelor's share). Advertising spend is expressed in $100,000 units except in the log-spend specification. Distributed-lag models lose the first one or two quarters per state when lagged terms are added ($n = 1{,}558$ for the 0–2 quarter lag models; $n = 1{,}517$ for the placebo-lead model). The placebo lead is included as a diagnostic for trend contamination rather than as a substantive effect. The winsorized model caps ad spend at the 99th percentile of the panel.

## E | Study 2 Temporal Activation Touchpoint Summary

**Table B6**

*Summary of clean gambling-ad touchpoint counts used in the Study 2 temporal extension*

| Metric | Value |
|---|---|
| Clean gambling-ad exposures | 4,621 |
| Users with at least one clean gambling-ad exposure | 208 |
| Share followed by any gambling touchpoint within 24 hours | 8.3% |
| First downstream gambling-web touchpoints within 24 hours | 286 |
| First downstream gambling-app touchpoints within 24 hours | 96 |
| No downstream gambling touchpoint within 24 hours | 4,239 |
| Observed web-to-app chains within 24 hours | 7 |

*Note:* "Clean" exposures are gambling ads on Facebook, TikTok, or X with no observed gambling web or app touchpoint in the prior 24 hours. First downstream touchpoints are assigned from the earliest observed gambling-web or gambling-app event in the next 24 hours.

**Table B7**

*Study 2 temporal activation sensitivity checks with additional time controls*

| Check | Predictor | ***b*** **(*SE*)** | ***p*** |
|---|---|---|---|
| 24-hour any-touchpoint model with hour-bin and day-of-week controls | Gambling ad (vs. clean non-gambling sponsored exposure) | 0.548 (0.117) | <.001 |
| 24-hour gambling-web model with hour-bin and day-of-week controls | Gambling ad (vs. clean non-gambling sponsored exposure) | 0.859 (0.149) | <.001 |
| 24-hour gambling-app model with hour-bin and day-of-week controls | Gambling ad (vs. clean non-gambling sponsored exposure) | -0.147 (0.233) | .527 |

*Note:* These user fixed-effects logistic models use the same 9,140 clean sponsored exposures and 74 users as Table 5, but add service, hour-of-day-bin, and day-of-week controls. The gambling-ad association remains positive for any downstream touchpoint and for gambling-web touchpoints, while the app-specific touchpoint model remains null.