Gambling and its digital distribution

2005-2026

Internet growth, consumer behavior, regulation and crime across income groups

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Comparative evidence review and reproducible aggregate analysis

Research cutoff
29 September 2026

Core cases
Australia, Brazil, Great Britain, India, Kenya, South Africa, Spain, Uganda and the United States.

This report distinguishes exposure, participation, intensity, harm and crime. It combines original calculations with an appraisal of published evidence, including mixed and null findings.

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Abstract

The internet changed how gambling is delivered and how gambling-related material circulates. These changes need to be measured separately. Operator revenue measures market activity, while advertising expenditure measures commercial inputs. Neither measures how many people encountered content or suffered harm. This report brings together a structured interdisciplinary evidence review, nine jurisdictional cases across four World Bank income groups, and legal and platform-policy records. It also includes an original analysis of 162 months of Spanish regulatory data. The report preserves a nine-country development panel and demographic evidence without pooling incompatible surveys.

The strongest findings concern expanded access, diverse promotional pathways, substantial exposure in surveyed populations, and concentrated financial vulnerability. There is credible evidence that advertising influences some betting decisions. How much it contributes to population harm remains uncertain. A randomized direct-marketing intervention produced mixed outcomes. US policy studies offer stronger evidence about expanded gambling access than about advertising alone. In Spain, marketing and gross gaming revenue correlate strongly in levels, but weakly in monthly changes. Conditional estimates remain uncertain. Official enforcement records document serious misconduct in licensed and unlicensed settings, but do not support a global content-to-crime elasticity. Historical comparisons show migration online without uniform growth in total participation or typical individual betting. Longitudinal evidence from low-income countries and comparable measures of illegal markets remain particularly scarce.

Executive summary

No single defensible percentage describes the growth of global gambling content. The available indicators cover different countries, products, channels and time windows. This review found no harmonized 2005-2026 global series of gambling posts, impressions, streams, affiliate traffic or unique exposed people. Spain provides a reproducible series of commercial spending. Recorded marketing rose from EUR 111.46 million in 2013 to EUR 664.77 million in 2025, in nominal terms. That increase includes bonuses and cannot be interpreted as a sixfold increase in viewed advertisements. The current data snapshot revises some previously published annual totals. DGOJ, 2026

Moving online and increasing overall participation are different processes. Australian national telephone-survey comparisons report online participation rising from 8.1% in 2010/11 to 17.5% in 2019, while any gambling fell from 64.3% to 56.9%. The same report found increased screened problem gambling. Migration, participation and harm can therefore move in different directions. Gambling Research Australia, 2021

Some causal links are narrower than public debate implies. An experiment can establish a response to an affiliate-labelled offer, but not long-term addiction. A trial restricting direct solicitations can estimate outcomes among self-excluding customers, but not all advertising effects. A legalization study estimates a change in access and its surrounding commercial environment. It does not isolate influencer promotion. These distinctions determine how broadly the findings can be applied. Houghton & Moss, 2021 von Hammerstein et al., 2025 Baker et al., 2026

Demographic patterns depend on the outcome. In Kenya, men reported greater betting participation than women, and participation was higher among tertiary-educated respondents than among those with no education. Those findings do not establish which group experiences the greatest harm relative to income. British youth data demonstrate substantial recalled exposure, but following

content, seeing advertising and gambling are separate survey measures. CBK, KNBS & FSD Kenya, 2024 GC & Ipsos, exposure, 2025 GC & Ipsos, influencers, 2025

Law changes the route to consumers as well as the product market. Licensing, payment controls, sponsorship restrictions, platform policies and influencer disclosure rules apply at different points. Their coverage differs. Rules can take effect in phases, be annulled or not yet be in force. India and Brazil illustrate sharply different recent national responses. Australia shows why a law passed in 2026 must not automatically be coded as fully operative in 2026. India, Online Gaming Act, 2025 Ministry of Electronics and Information Technology, India, 2026 Brazil, Law 14.790, 2023 Secretaria de Comunicação Social, Brazil, 2024 ACMA, 2026

Crime is documented, but its trend is not captured by a generic gambling-crime rate. Case records establish particular laundering-control failures, deceptive endorsements and criminal-network practices. They do not measure the prevalence of all such conduct, and larger fines or more blocked websites may reflect stronger enforcement. A new cross-country correlation between content and crime would be misleading with the assembled data. AUSTRAC, 2023 FTC, 2017 UNODC, casinos report, 2024

1. Questions, definitions and scope

The central question is how the internet changed the availability, visibility and social organization of gambling, and what can be established about the consequences. The review examines two related subjects: gambling services supplied through digital channels, and gambling-related communication circulating through the wider internet. A person can watch a casino stream without gambling, buy a lottery ticket online without following gambling content, or gamble at a physical venue after encountering a digital promotion.

Here, **gambling** refers to staking something of value on an uncertain outcome for the prospect of a return, subject to the definition used by each source and legal system. Sports betting, casino games, poker and lotteries remain distinct products. Raffles, free prize draws, fantasy sports, loot boxes and tradable skins each require their own classification. Mechanics that look similar may differ legally because of differences in payment, chance, skill, prize value or convertibility. The UK government's skins review helps explain this distinction: cashable digital assets cannot simply be treated as ordinary game decoration. Department for Culture, Media and Sport, 2025

The report separates six stages:

Stage	Suitable observations	What the observation does not establish
Exposure	Ad impressions, recalled advertisements, sampled visible branding	Attention, acceptance or subsequent gambling
Engagement	Watching, clicking, following, sharing	Deposits or participation
Participation	A bet, a gambling transaction, survey participation	Intensity, loss of control or harm
Intensity	Frequency, stakes, deposits, duration, net expenditure	Clinical disorder or criminal offending
Harm	Financial difficulty, relationship effects, validated symptom measures	A universal causal role for online content

Stage	Suitable observations	What the observation does not establish
Crime	Defined offences, judgments, recorded cases, victimization measures	Total undetected crime or all regulatory noncompliance

Gross gaming revenue, or GGR, usually means operator receipts after payouts, as defined by the reporting system. It is not the total amount staked, and it is not interchangeable with deposits, company profit or a clinical measure. Marketing totals can combine advertisements, incentives, affiliates and sponsorship. The Spanish files explicitly distinguish these components. Accounting-based GGR can approximate aggregate net player expenditure within the transactions it covers. This report does not equate it with individual financial harm or the losses of everyone in a country. DGOJ, 2026

“Licensed” means an authorization exists for a defined activity and territory. “Regulated” describes applicable rules. “Restricted” identifies conditions or exclusions. “Prohibited” requires an identified legal provision. “Unlicensed” describes missing authorization; its legal consequence depends on whether authorization is required. An offshore operator can hold a valid destination-country licence, while an operator incorporated domestically can act unlawfully. Where product-level legal verification is incomplete, the dataset says **NV, not verified**. It does not label the law ambiguous merely because this review lacks a verified answer.

The report uses consumer for participation evidence, affected person for harm evidence, and victim where a source establishes victimization. Treating every gambler as a victim would prejudge the research question and obscure differences between enjoyment, problematic behavior and fraud.

2. Research design and evidence handling

2.1 Search and selection

This is a structured comparative review with original aggregate analysis, not a registered systematic review or a new meta-analysis. Searches covered accessible scholarly records and full texts through PubMed/Europe PMC, Crossref, publisher and university repositories, NBER, official statistical agencies, gambling regulators, legal repositories and platform-policy pages. Crossref records were used to check that DOIs, titles, authors and publication details matched. The search included working papers, but the final discussion of causal findings gives priority to original studies and verified journal records.

Searches combined gambling, betting, casino, lottery, raffle and skins terms with advertising, social media, affiliates, influencer, streaming, exposure, longitudinal, experiment, legalization, crime, laundering, COVID and demographic terms. Searches by country and regulator supplied legal and administrative records. References in reviews led to selected original studies, especially the historical sportsbook comparison and advertising experiments. Searches of later related records looked for updates and contradictory findings. There was no complete citation-index export or independent second screener. A PRISMA flow count would imply a selection process that was not performed, so no such count is reported.

To be included, a source had to be traceable, identify its population or legal scope, and contribute to at least one question. Affiliate casino pages, unsourced market-size websites and promotional rankings were excluded as authorities. News and university announcements were used mainly to locate original material. Official law and platform policy establish stated rules, but do not show how often those rules are followed. The methods appendix and package contain detailed search strings, retrieval routes, exclusions and the limits of the retained search record.

2.2 Why these jurisdictions?

The nine cases were selected to cover contrasting markets and regulatory systems. Each needed enough accessible evidence for at least one part of the analysis. This is purposive selection, not a representative sample of the world. The gambling statistics cover Great Britain. The WDI contextual series covers the United Kingdom, including Northern Ireland. These geographical boundaries are labelled separately throughout.

Current income group	Selected cases	Analytical contribution and coverage limit
High income	Australia; Spain; United Kingdom, with GB gambling evidence; United States	Repeated surveys, operator series, regulatory change and stronger financial-outcome studies; substantial influence on the literature
Upper middle income	Brazil; South Africa	Different combinations of online authorization, product restrictions and market formalization; limited comparable long-run exposure measures
Lower middle income	India; Kenya	Recent divergent legal responses; mobile-payment context and household participation evidence; incomplete harmonized harm series
Low income	Uganda	A statutory framework and local school evidence; no verified national longitudinal content-harm dataset

The groups use World Bank country metadata retrieved on 29 September 2026, corresponding to the current FY 2027 classification. These labels define comparison strata. They do not mean that countries held the same status throughout 2005-2026. National income and an individual's income, education or financial reserves are different variables. World Bank, income groups, 2026 World Bank, WDI, 2026

2.3 Appraisal and inference

The appraisal considers study design, measurement, coverage, transparency, temporal ordering and conflicts of interest. A large convenience sample can yield a more precise estimate of a relationship within that sample without making the sample nationally representative. Administrative data can accurately describe recorded accounts while omitting unlicensed operators and cross-operator identities. A systematic review inherits weaknesses and overlapping samples from the studies it includes.

The evidence matrix distinguishes confidence in a reported observation from confidence in a causal or international inference. Funding is recorded without automatically excluding a study. For example, the European sportsbook cohort was supported through an Entain/GVC research relationship. Access to detailed records adds value, while dependence on one operator limits coverage. The Ugandan paper reports several authors' gambling-related institutional or consultancy relationships, along with statements that these were unrelated to the study. Unverified declarations are marked as such, rather than recorded as “no conflict.” Nelson et al., 2021 Anyanwu et al., 2023

Five evidence labels guide the synthesis: **descriptive**, **associational**, **experimental**, **quasi-causal under stated assumptions**, and **mechanistic interpretation**. A mechanism can be plausible

without having a measured population effect. Confidence grades in the matrix are reasoned judgments, not a formal GRADE assessment.

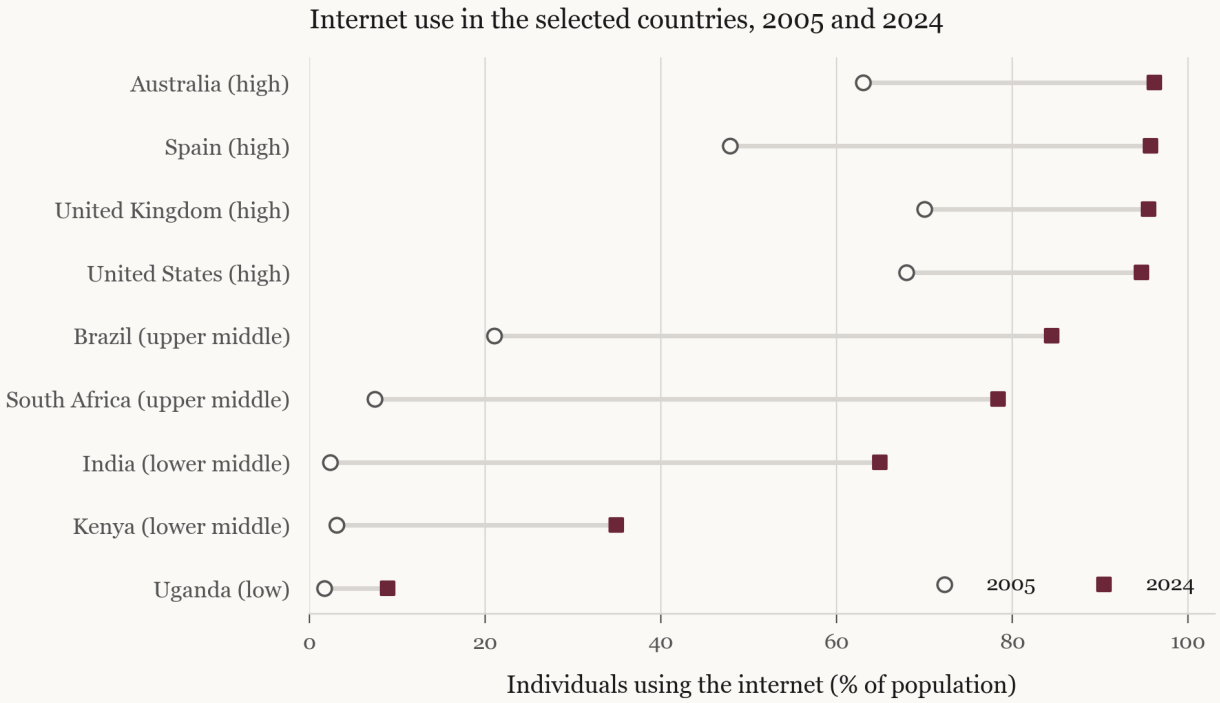
3. Historical development: overlapping transitions

The evidence supports overlapping transitions rather than global eras that began at the same time. A person in one country could still encounter betting mainly through shops while another already used smartphone applications and social promotions. The dates below organize the evidence. They are not measured universal change points.

Approximate period	Documented transition	Interpretation and limit
Before 2005	Gambling already had venue, telephone, broadcast and early internet channels	Digital distribution extended an existing market and existing relationships with financial misconduct; it did not create gambling or casino-related crime
2005-2010	Web-based accounts and cross-border provision became central regulatory questions	UIGEA and the GB licensing framework illustrate competing approaches; early operator records provide a behavioral baseline
2010-2015	Internet adoption broadened; remote licensing increasingly addressed consumers' location	Online participation expanded in Australian comparisons; GB's 2014 territorial change shows the limits of regulating only domestic suppliers
2015-2019	Social content, affiliates, influencers and tradable-game-item promotion drew greater scrutiny	The CSGO Lotto case documented undisclosed commercial interests; US state choices changed after Murphy
2020-2022	Pandemic disruption intersected with marketing restrictions and changing product availability	Sports interruptions and venue closures were different shocks; a universal increase in gambling is not supported
2023-September 2026	More explicit attention to direct marketing, platform links, game definitions and consumer protection	Brazil, India and platform-policy changes show divergent responses; implementation and measurable outcomes remain uneven

The timeline draws on legal records, Australian repeated surveys, the European cohort study and the pandemic review. It identifies changes in access and distribution that a single “online era” category would hide. It cannot establish the independent share of market growth attributable to broadband, smartphones or social media. United States Congress, 2006 UK Parliament, 2005 Gambling Commission, 2026 Murphy v NCAA, 2018 FTC, 2017 Brodeur et al., 2021

Connectivity expanded substantially, but unevenly. The WDI extract uses a common year for comparison. It records internet use in 2024 at roughly 95-96% in the four high-income cases, 84.5% in Brazil, 78.4% in South Africa, 64.9% in India, 35.0% in Kenya and 8.9% in Uganda. These are population internet-use estimates, not rates of smartphone ownership, platform membership or online betting. Differences in national measurement practices and data revisions still matter. World Bank, WDI, 2026



Source: World Bank WDI IT.NET.USER.ZS, snapshot 29 September 2026.
All ages; original values, no interpolation. Current income groups, not historical classifications.

Figure 1. Original WDI observations for 2005 and 2024, the latest year with nonmissing internet-use values for all nine cases in the retrieved snapshot. Current income groups are shown for context. No interpolation or population-weighted group average is used. Source: World Bank WDI, IT.NET.USER.ZS; underlying observations are included in the package.

4. What can be measured about growth?

4.1 A measurement inventory

Indicator	What it can establish	Main obstacle to a long-run content series
Marketing expenditure	Commercial resources committed within a defined reporting market	Inflation, bonuses, accounting changes, buying prices and changing reach per unit spent
Posts, advertisements or broadcast messages	Volume within a specified sampling frame	Deleted material, changing APIs, reposts, sampling hours and platform growth
Impressions and unique reach	Delivery to accounts within the measuring system	Platform access, duplicate identities, bots, viewability and limited independent audit
Streams and viewing hours	Consumption on the measured platform	Category migration, changed platform rules and gambling-adjacent streams
Search interest	Relative query activity within a specified extraction	Normalization, term ambiguity, geography and unstable comparisons across separate downloads
Website traffic or site counts	Measured visits or observable domains	Proprietary panels, mirror domains, VPNs and repeated users
Survey exposure recall	Reported encounters in a defined population and period	Recall, attention and previous gambling influence what is remembered

University content audits show that promotional material can appear as sports discussion, humor or entertainment without an explicit instruction to bet. Counting sampled posts, however, does not tell us how many people saw them. Broadcast audits face a related problem. Message totals change with observation hours, matches, camera views and coding rules. The Bristol research collections support studying these forms separately, but their samples do not supply a global longitudinal denominator. University of Bristol Gambling Harms Research, 2026 University of Bristol Gambling Harms Research, 2026

4.2 Spain: an observed spending series

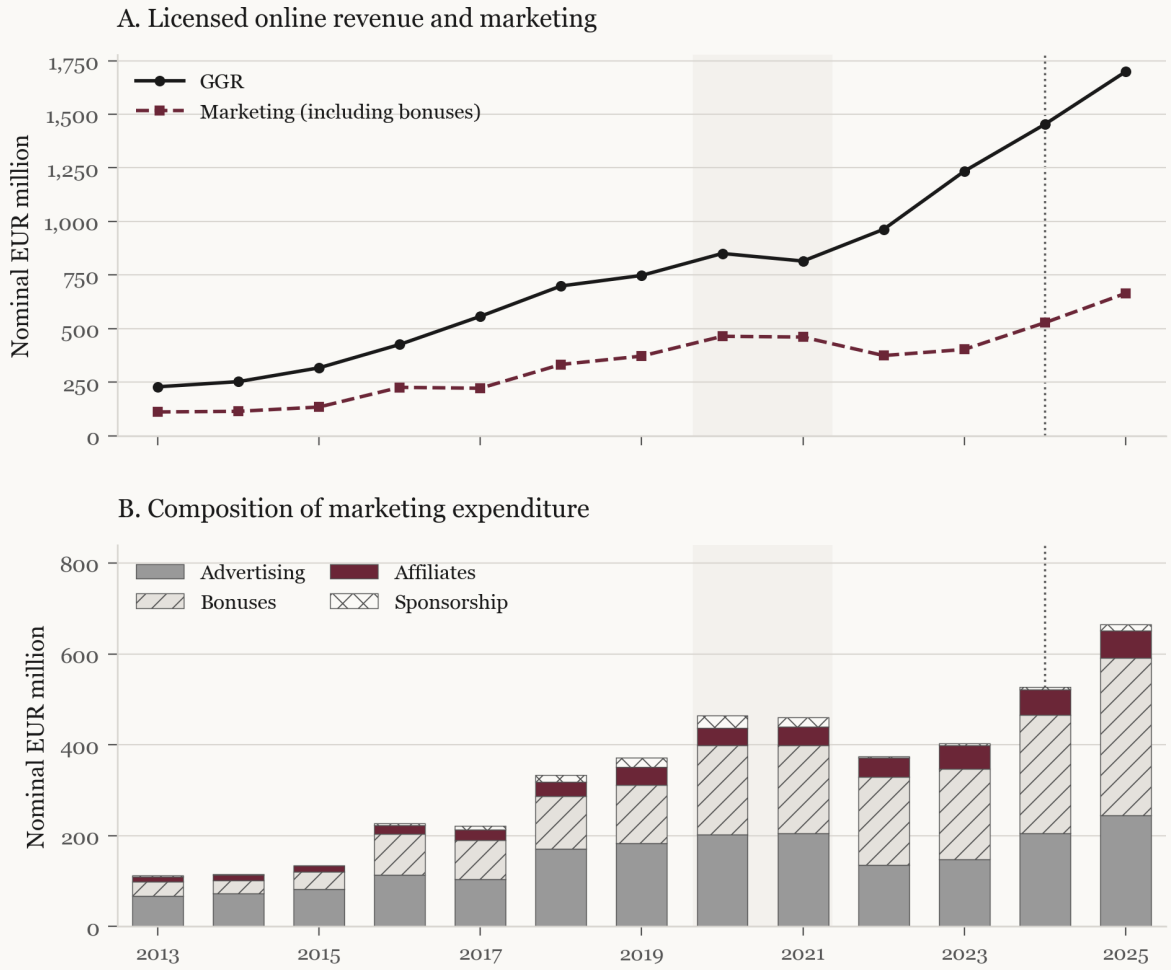
The DGOJ files contain 162 consecutive months from January 2013 through June 2026. They cover advertising, bonuses, affiliate spending, sponsorship, active and new accounts, and stakes and GGR across 17 product rows per month. Product-level rows are summed once. There is no aggregate “total” row to add again. The component-sum check allows only the small rounding discrepancy found in the source files. The original file bytes and source metadata are preserved. DGOJ, 2026

Full-year marketing increased by about 496% between 2013 and 2025, compared with about 643% for GGR. Marketing fell after 2021 before increasing again. This pattern is inconsistent with steady exponential growth. It also does not identify a policy effect. Product expansion, demand, incentives, prices, sporting events, regulation and accounting revisions can all contribute.

Calendar year	Marketing, EUR million	GGR, EUR million	Marketing / GGR	Marketing per active account-month, EUR
2013	111.46	228.64	48.75%	33.31
2019	372.00	748.24	49.72%	34.32
2020	464.33	850.74	54.58%	44.38
2021	460.77	815.24	56.52%	38.45
2022	374.51	963.33	38.88%	29.51
2023	403.14	1,234.37	32.66%	28.97
2024	527.96	1,453.56	36.32%	30.63
2025	664.77	1,698.73	39.13%	32.19

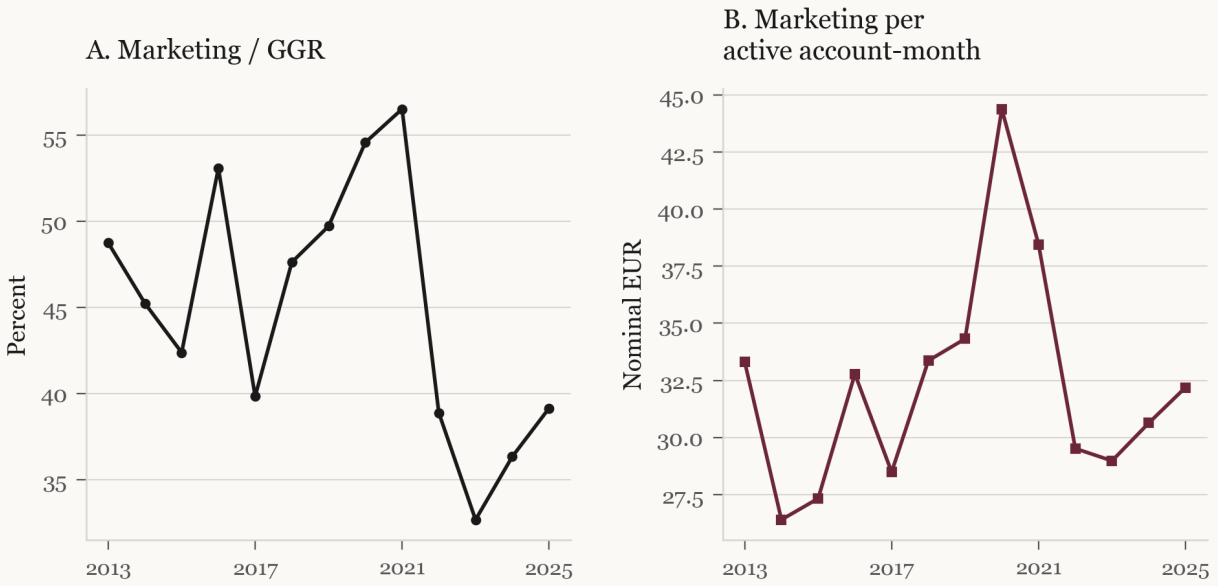
All amounts are nominal and come from one retrieved revision of the files. Active account-months are the sum of monthly active accounts. The same account can therefore appear in several months, and the same person can hold several accounts. This denominator is useful for scaling activity, but it is not a count of unique people. Marketing per account-month changed relatively little between the start and end of the period, despite the much larger increase in total spending.

The first half of 2026 records EUR 373.98 million in marketing and EUR 993.56 million in GGR. These are six-month totals. They are retained in monthly models but excluded from full-calendar-year growth charts. They are not doubled to create a forecast. The snapshot's revised 2024 figures differ from older annual releases. The package lets readers trace exactly which data version produced each result.



Source: DGOJ monthly returns, snapshot to June 2026. Licensed online products; annual sums. 2026 is excluded as incomplete. Shading: 2020-2021; dotted line: 2024 partial court annulment. Marketing measures expenditure, not impressions. GGR is neither turnover nor measured harm.

Figure 2. Calendar-year sums from DGOJ licensed online operator returns, 2013-2025. The shaded period marks 2020-2021; the dotted line marks the year of Spain's partial judicial annulment. These annotations locate events and do not estimate their causal effects.



Source: DGOJ, 2013-2025. Annual marketing / annual GGR or sum of monthly active accounts.
An individual may hold several accounts. Panel B does not measure spending per unique gambler.

Figure 3. Marketing divided by GGR and by active account-months. These ratios answer different descriptive questions. Correlating them with other series sharing the same denominator would create additional mathematical dependence and is not done here.

4.3 Market formalization changes what becomes visible

Great Britain's latest annual return release reports GBP 8.3 billion in remote casino, betting and bingo GGY for April 2025-March 2026, up 6.9% on the preceding year. That is a licensed-market accounting measure, not a count of online content. Reporting arrangements changed in July 2024, which matters for historical comparisons. Gambling Commission, market statistics, 2026

South Africa's FY 2024/25 statistics report total GGR of about R 74.9 billion, with betting accounting for about R 52.3 billion. The betting category is not an online-only measure. Brazil's regulated federal market began operating in January 2025, creating a new administrative system for observing activity. An increase in reported licensed activity can reflect a shift from previously unrecorded activity as well as new participation. Country revenue totals are therefore not pooled into a synthetic growth rate. National Gambling Board, 2025 Secretaria de Comunicação Social, Brazil, 2024

5. How content reaches people

The main commercial distinction is between acquiring a new customer and encouraging an existing customer to return. The same communication can serve both purposes. Personalization also varies. A shirt sponsor reaches a broad audience, while a deposit-linked offer can use an account's prior behavior. The pathways below form an analytical taxonomy based on advertising studies, regulatory definitions and platform rules. The timing is approximate and does not imply that adoption occurred at the same time everywhere. McGrane et al., 2025 Houghton & Moss, 2021 RD 958/2020, 2020

Pathway and period of relevance	Incentive, audience and presentation	Evidence and enforcement issue
Search and affiliate sites; established web period onward	Operators pay for referrals or other agreed outcomes; reaches people actively seeking information, odds or offers	Commercial rankings can resemble independent advice; search intent confounds exposure-behavior associations
Display, video and paid social advertising; broadening with digital media	Purchases audience access; targeting can use interests or prior interaction	Impression data are usually proprietary; a permitted ad and actual age verification are different questions
Sports integration; extends older sponsorship practices online	Odds, branding, broadcasts and clips connect promotion with an existing sports audience	Promotional material can persist in highlights and reposts; front-of-shirt restrictions cover only one surface
Influencers and affiliates on social platforms; prominent by the mid-to-late 2010 s	Personal trust and entertainment carry commercial referrals; disclosure may be inconspicuous	CSGO Lotto provides a documented disclosure case; audience effects cannot be inferred from follower counts
Livestreams and short clips	Wins, losses and commentary are presented as entertainment; sponsorship and referral income can reward engagement	Edited clips may omit the full gambling session; streams cross platform and national boundaries
Direct email, SMS, push messages and bonuses	Existing account relationships permit repeated contact and potentially detailed personalization	The French trial directly tests suppression of solicitations, with mixed outcomes across endpoints
Peer groups, tips, memes and private communities	Status, shared interest and social interaction; commercial and ordinary users may coexist	Closed groups are difficult to observe; peer selection and peer influence are hard to separate
Individual lotteries and raffles	Community familiarity and perceived low cost can lower skepticism	Legal classification depends on payment, chance, prize and exemptions; social trust is not proof of authorization
Payment- and app-mediated return	Stored credentials, notifications and app access can shorten the route from a prompt to participation	Convenience is a mechanism; its separate contribution to harm requires direct evidence

This taxonomy includes content that does not involve an immediate commercial transaction. A meme may create familiarity with a brand, a betting tip may lead to an affiliate site later, and someone who is not paid may repost a clip. People can also encounter educational, critical or recovery-oriented material about gambling. Content classification should therefore record who produced the material, any commercial relationship, its function and its links. Counting every gambling mention as promotion would lose these distinctions.

The causal path is not one-directional. People who already gamble can search for odds, subscribe to streamers and notice more advertisements. Their behavior may also influence what content a service recommends. This creates feedback between interest, exposure and participation. An association between recalled advertising and gambling cannot, by itself, establish which direction dominates.

5.1 Platform differences

Platform or service	Verified policy feature at the research cutoff	Analytical limit
YouTube	March 2025 changes restricted direction to uncertified gambling sites; November changes addressed gambling involving digital goods with monetary value	Links, logos and verbal directions matter; a policy announcement supplies no before-after compliance estimate
Twitch	Current guidelines prohibit specified gambling sites and associated promotion while allowing some betting-related content	Product and operator rules differ; “all gambling banned” is inaccurate
Kick	Current guidance permits gambling subject to applicable law, local licensing for sponsorship and content-classification requirements	Policy is not evidence that every stream complies or that the audience is adult
TikTok	Paid gambling advertising is governed by market-specific eligibility and certification requirements	Paid ads, branded content and ordinary posts have different rules; this review does not collapse them
X/Twitter	Gambling advertising is subject to country-specific restrictions and authorization requirements	Paid-ad policy does not establish the treatment of every ordinary post
Facebook and Instagram	Relevant advertising and content pathways are documented in the wider literature	A current primary policy page was not independently verified in this review; no claim of a universal current ban is made
Reddit	Communities can host discussion, information and links	No measured platform-wide gambling exposure series or verified current policy inventory was assembled
Discord	The reviewed advertising policy excludes gambling-related promotion	An advertising rule is not a census or blanket description of all community servers
Google Search/Ads	Advertising certification and jurisdictional conditions apply	Organic search, paid results and affiliate pages remain different distribution systems
Google Play	Real-money gambling applications require specified eligibility and licensing conditions	Store availability is not independent proof of legality or compliance; Apple's store was not separately audited

Sources: YouTube's dated announcements; current Twitch, Kick, TikTok, X and Discord rules; Google advertising and Play policies. Living pages are recorded as snapshots, not reconstructed historical archives. YouTube, 2025 YouTube, 2025 Twitch, 2026 Kick, 2026 TikTok for Business, 2026 X, 2026 Discord, 2026 Google, 2026 Google, 2026

Recommendation systems matter because ranking determines what people see repeatedly. The assembled evidence cannot, however, quantify the contribution of proprietary algorithms separately from users' choices, commercial targeting and the supply of creator content. A future audit should distinguish logged-in from logged-out delivery, record its observation protocol, repeat measurements and identify paid versus organic material. The presence of gambling clips alone does not establish a secret platform objective.

6. Demographics, exposure and socioeconomic position

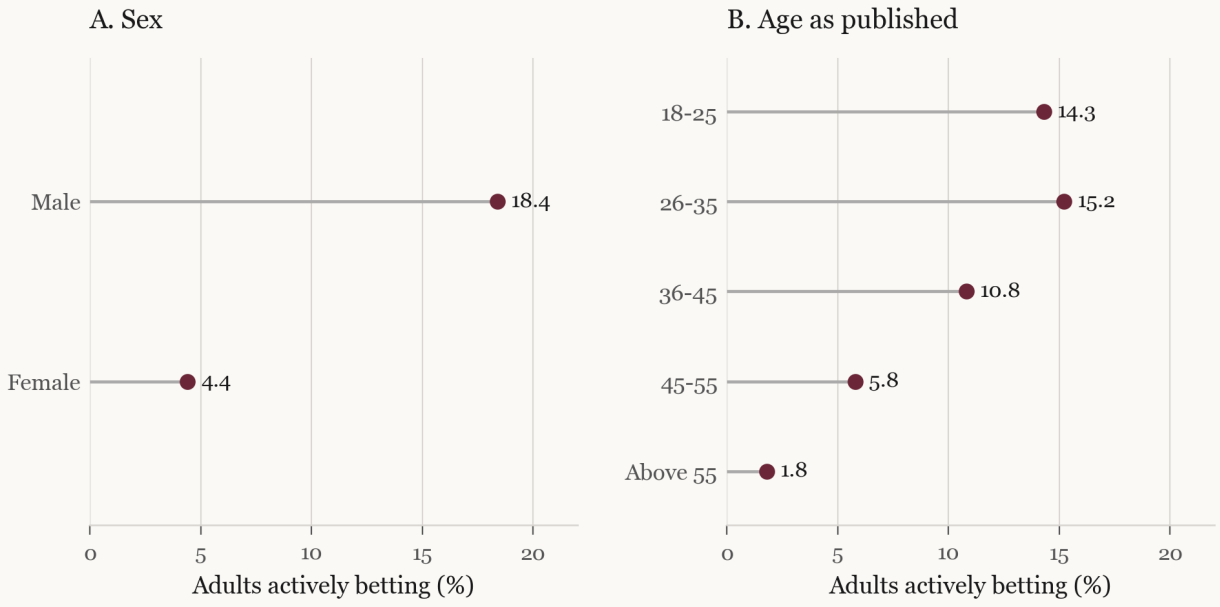
6.1 Sex and age

The 2025 British school survey found that 30% of pupils aged 11-17 had spent their own money on some form of gambling in the previous year. The figures were 34% among boys and 27% among girls. The survey includes activities beyond commercial online betting, so these are not rates of illegal casino use. Its sample of 3,666 pupils describes the school-survey frame, with the usual limitations of self-report and school coverage. GC & Ipsos, youth survey, 2025

Advertising recall was widespread, with 79% recalling some gambling advertising. The online and offline categories overlapped. The questionnaire's influencer item used a more selective denominator. Sixteen percent followed gambling-related content. Among respondents in that subgroup, 31% reported advertising by an influencer, creator or streamer they followed. The subgroup item had 578 respondents. Multiplying rounded percentages would produce only a rough derived figure, so the original conditional measures are retained. GC & Ipsos, exposure, 2025 GC & Ipsos, influencers, 2025

Young adults and adolescents should not be combined. They face different legal access and household circumstances, and the sources use different instruments. Likewise, a school study with respondents aged 11-25 cannot be treated as evidence exclusively about minors. In Mbarara, Uganda, Anyanwu and colleagues analyzed 905 respondents from 12 schools; 59.2% were under 18. The paper's 17.7% screen-positive result concerns this mixed-age local sample, not Uganda's national youth prevalence. Anyanwu et al., 2023

In Kenya's 2024 FinAccess analysis, active betting was reported by 18.4% of men and 4.4% of women. Rates were 14.3% for ages 18-25, 15.2% for 26-35, 10.8% for 36-45, 5.8% for the published 45-55 band, and 1.8% above 55. The overlapping age-45 labels are preserved rather than silently repaired. These figures describe participation, not advertising exposure or harm. Rates for age groups alone do not establish that a particular young-male subgroup has the same rate. CBK, KNBS & FSD Kenya, 2024



Source: FinAccess 2024, Figure 4.21, p. 61. Weighted adults 18+; subgroup sizes unavailable.
Original estimates, not online-only or harm prevalence. Published 45-55 band overlaps its predecessor.
No confidence intervals reconstructed without survey-design information.

Figure 4. Weighted participation estimates from FinAccess 2024, Figure 4.21. Published analysis is for adults aged 18+, although the survey initially sampled people aged 16+. The designed household sample was 28,275 and the overall response rate 84.6%; subgroup achieved sample sizes and design information were not available in this extraction, so new confidence intervals are not manufactured.

6.2 What survives adjustment?

Noble and colleagues examined 6,377 Australian students from a 2017 survey. After adjustment for demographic, peer and school characteristics, recalled online gambling advertising remained associated with past-month gambling, with an odds ratio of 1.37 (95% CI 1.01-1.85). Other channel associations did not all persist, and adjusted associations with the study's harder-gambling outcome were not significant. The paper also classified some missing follow-up participation answers as no past-month gambling. That measurement choice needs to be considered when appraising the findings. This is useful evidence of a channel-specific association, but recall and prior involvement can still confound interpretation. Noble et al., 2022

Evidence about nonbinary or other sex/gender categories was not sufficiently consistent across the extracted sources for comparative estimates. Binary tables reflect the original data, not an assertion that all populations are binary. Migrant status, digital literacy and occupation-specific exposure were also too unevenly measured for a defensible pooled analysis.

6.3 Socioeconomic position and financial vulnerability

Kenyan participation was higher in urban than rural areas and higher among tertiary-educated respondents than among those with no education. This pattern alone refutes the automatic assumption that participation must always be concentrated among the least educated. It does not rule out unequal harm. The same spending can be manageable for a household with reserves and destabilizing for one facing food, rent or debt obligations. Access to income and technology may increase participation, while limited financial buffers increase the consequences. CBK, KNBS & FSD Kenya, 2024

Muggleton and colleagues used records from up to 6.5 million UK bank customers. They observed associations between gambling expenditure relative to income and financial, social and health-related outcomes. The unusually large dataset supports precise descriptions of these relationships, but bank selection, omitted circumstances and expenditure measurement still matter. This is not a randomized comparison of otherwise identical gamblers and nongamblers. Muggleton et al., 2021

The US evidence from Baker and colleagues is stronger on temporal identification. It examines financial consequences using staggered expansion of online sports betting and household financial transactions. The study finds adverse effects concentrated among financially constrained households. Interpretation depends on assumptions about policy timing and comparison groups. Expanded access also comes with changes in products, payments and marketing. This report cites the verified 2026 journal publication and separately identifies the inspected NBER working-paper revision. It does not mix coefficients from different versions. Baker et al., 2026 Baker et al., 2024

7. Comparing income groups without confusing exposure and harm

The high-income cases have richer administrative series and a larger share of the research base. This allows closer observation, but can make their problems appear uniquely prevalent simply because they are measured more often. The global prevalence review included 366 samples from 67 countries, with substantial gaps and differences in methods. A country absent from a comparable survey series cannot be coded as having zero gambling harm. Tran et al., 2024

Brazil and South Africa show that a shared income group does not imply a common legal market. Brazil established a federal regulated fixed-odds market in 2025. South Africa's dated regulatory guidance distinguished authorized online sports betting from prohibited interactive casino gaming. Consequently, an aggregate “online gambling” measure can cover different products and legal populations in the two countries. Secretaria de Comunicação Social, Brazil, 2024 National Gambling Board, 2020

India and Kenya also differ in connectivity and recent legal strategy. India's online-money-game prohibition and Kenya's licensing reform should not be explained simply by their shared income classification. Regulatory history, constitutional allocation of power, domestic politics, product definitions and administrative capacity would each require separate evidence. This review documents the contrast. It does not estimate a national-income effect. India, Online Gaming Act, 2025 Ministry of Electronics and Information Technology, India, 2026 Kenya, Gambling Control Act, 2025

The sub-Saharan African review describes traditional gambling alongside emerging technologies, including mobile access and payment systems. These mechanisms are relevant to distribution, but a regional account cannot replace a national longitudinal panel. Uganda is a useful low-income case because the limits of the evidence are clear. Statutory records and local studies exist, while comparable population exposure and harm trajectories are not established. Bitanihirwe et al., 2022 Uganda, Lotteries and Gaming Act, 2016

Can apparent income-group differences disappear after controlling for internet access, age, law and reporting? **The assembled data cannot identify that counterfactual.** The nine-country WDI panel has suitable contextual variables, but lacks equivalent country-year exposure, participation and harm outcomes. Adding controls to incompatible dependent variables would not solve the measurement problem. No regression purporting to explain national harm by income group is fitted.

The data support a more limited comparison. Connectivity, regulatory scope and data coverage differ, as do the opportunity and reporting denominators behind observed market size and measured harm. A future comparative study should sample both connected and unconnected households, measure disposable resources separately from national income, and estimate effects within countries before attempting cross-country pooling.

8. Does exposure lead to participation and harm?

Advertising research supports some links in the sequence. Certainty decreases as the endpoint moves from immediate response to long-term population harm. McGrane and colleagues' 2025 sports-advertising review included 22 studies, 16 from Australia. It found evidence linking marketing with several gambling behaviors, while emphasizing heterogeneous methods and outcomes. This geographical concentration limits how far the findings can be extended to different sports cultures, products and income groups. McGrane et al., 2025

The 2023 umbrella review and Bouguettaya and colleagues' earlier critical meta-analytic review provide a broader synthesis. They should not be treated as independent replications of every underlying experiment or survey. The reviews overlap, longitudinal evidence is uneven, and attitudes, intentions and observed behavior are distinct outcomes. This report therefore does not calculate a second pooled effect from their summaries. McGrane et al., 2023 Bouguettaya et al., 2020

In Houghton and Moss's experiment, 145 regular football bettors assessed offers that differed in complexity and account type. Affiliate presentation affected confidence and reported likelihood of betting for medium-complexity offers. Effects were not uniform across all bet types. Randomized or controlled presentation strengthens inference about that immediate response. The outcomes, however, were judgments about offers rather than sustained financial losses. Houghton & Moss, 2021

The French trial offers a more direct test of behavior. Among 2,548 customers who self-excluded for up to three months, it compared a nine-month extension of the ban on commercial solicitations with standard practice. The primary outcome, change in four-week net losses at month nine, did not differ significantly. Secondary deposit reductions favored the intervention at months six, nine and twelve, but not eighteen. At month nine, the reported reductions were EUR 451.92 versus EUR 343.21, with $p = .040$. These are reductions, not account balances. Multiple endpoints, observation at one operator and unobserved movement elsewhere limit the inference. von Hammerstein et al., 2025

The reasonable synthesis is that commercial communication can influence some gambling choices and activity. Evidence does not support a universal effect size for every platform, a claim that all exposed people escalate, or a complete quantified chain from ordinary advertising to crime. The randomized null primary result is substantive contradictory evidence, even though some secondary outcomes favor intervention.

9. Legal evolution and enforcement

9.1 What the legal dataset does and does not classify

The package includes 22 dated legal events, six case/enforcement records and a classification table covering nine jurisdictions and 18 activities. The table records verified rules and explicitly marks unverified cells. It is not a fully verified annual inventory of every product in every jurisdiction from 2005 onward. Product exceptions, subnational variation and current consolidated law remain important, especially for the United States, lotteries, fantasy sports, loot boxes and cryptocurrency-funded play.

A dated event record is preferable to carrying a legal label across all years without checking it. A statute may be enacted before commencement, a court may annul only selected clauses, and implementation may depend on regulations or licences. The South African 2008 amendment illustrates the problem. The regulator's 2020 guidance stated that it had not commenced. The dataset retains that dated conclusion and does not present it as a fresh 2026 legal opinion. National Gambling Board, 2020

9.2 Principal changes

Jurisdiction	Verified development	What follows from it
Great Britain	Gambling Act 2005; destination-based remote licensing from 2014; online-slot stake caps introduced in 2025	Product licensing and consumer location matter. The 2025 caps are per stake, not deposit or loss limits
United States	UIGEA in 2006; Murphy v NCAA in 2018	Payment enforcement and state authorization are separate issues; neither event yields one national legal status for every online product
Australia	Interactive Gambling Act framework; 2017 offshore reforms; 2024 payment restrictions; reform bill passed August 2026	Licensed wagering exceptions coexist with prohibited services. Most new 2026 reforms begin in January 2027
Spain	RD 958/2020 phased commercial-communication restrictions; partial annulment in April 2024	Rules vary by channel and changed within the observed series; the whole decree was not nullified
Brazil	Law 14.790/2023; regulated-market launch January 2025; 2026 advertising measures	Authorization concerns defined fixed-odds activities and qualifying online games, not automatic legalization of every gambling product
South Africa	National Gambling Act 2004; 2011 cross-border judgment; 2020 clarification	The dated guidance prohibits online casino-style gaming and distinguishes authorized sports betting; later recertification is incomplete here
Kenya	Gambling Control Act 14/2025, commencing August 2025	A new authority and licensing framework; detailed product implementation should be checked separately
India	Online Gaming Act 2025 and rules effective May 2026	Covered online money games are prohibited; permitted social games/esports require separate classification
Uganda	Lotteries and Gaming Act 2016	A statutory licensing regime exists; the existence of law does not establish enforcement coverage

Primary sources: UK Parliament, 2005 Gambling Commission, 2026 Gambling Commission, 2025 United States Congress, 2006 Murphy v NCAA, 2018 ACMA, 2026 ACMA, 2024 RD 958/2020, 2020 Tribunal Supremo, 2024 Brazil, Law 14.790, 2023 Ministério da Justiça e Segurança Pública, Brazil, 2026 Republic of South Africa, 2004 Casino Enterprises, 2011 Kenya, Gambling Control Act, 2025 India, Online Gaming Act, 2025 Ministry of Electronics and Information Technology, India, 2026 Uganda, Lotteries and Gaming Act, 2016

Spain's restrictions included a limited overnight broadcast window and conditions on other commercial communications. The Supreme Court's 2 April 2024 judgment annulled selected provisions challenged by the Asociación Española de Juego Digital. These included parts of articles 13, 23, 25 and 26, as well as article 15. It did not remove every advertising restriction. A binary “ban from 2021 onward” variable would therefore misdescribe the legal treatment. RD 958/2020, 2020 Tribunal Supremo, 2024

India's framework can also be misclassified if older descriptions of skill-game exemptions are reused without checking the 2025 Act and 2026 rules. The current documents distinguish prohibited online money games from permitted social games and esports. They also provide a determination process. This review did not verify the final outcome of every constitutional or implementation challenge. Nor does it extend that classification to all offline state lotteries. India, Online Gaming Act, 2025 Ministry of Electronics and Information Technology, India, 2026

9.3 Cases and the meaning of sanctions

Murphy v NCAA resolved a constitutional issue about the allocation of power. The US Supreme Court invalidated PASPA's restrictions under the anti-commandeering principle. It did not grant every person in every state an immediate right to use any betting product. Differences in how states subsequently implemented their rules make policy comparisons possible, but also create identification challenges. Murphy v NCAA, 2018

Casino Enterprises v Gauteng Gambling Board concerned a service supplied from outside South Africa. The judgment illustrates that placing infrastructure across a border does not necessarily remove destination-country controls. That legal proposition should be distinguished from the practical ability to identify operators, freeze payments or enforce judgments abroad. Casino Enterprises, 2011

The 2017 FTC CSGO Lotto consent order addressed undisclosed relationships involving the site's owners and promotional content. It is evidence that influencer ownership and payment relationships can be material consumer-protection facts. A consent order should be described by its actual legal outcome, not recast as a criminal conviction. FTC, 2017

Enforcement in licensed markets also matters. AUSTRAC's Crown case resulted in a court-approved A$450 million civil penalty concerning AML/CTF failures at Melbourne and Perth casinos. The Gambling Commission's 2023 William Hill Group action concerned social-responsibility and AML failures and payments totaling GBP 19.2 million. Neither amount measures money laundered or the population prevalence of harmful gambling. AUSTRAC, 2023 Gambling Commission, 2023

9.4 Measuring regulatory effects

Aonso-Diego and colleagues analyzed Spanish quarterly outcomes for 2013-2023 using SARIMA intervention models. They reported declines in new accounts and stakes associated with selected implementation periods. These included approximately 263,000 fewer new accounts and EUR 216 million lower stakes on the study's quarterly scale. The study adjusted monetary values for inflation and considered COVID effects. It also explored different event timings and pulse/step specifications, and had no untreated market comparator. Its findings support a policy hypothesis more strongly than an unadjusted before-after chart, but do not settle attribution. Aonso-Diego et al., 2025

An adequate platform-policy evaluation would need comparable exposure measures before and after a change, along with an unaffected or differently affected comparison group. It would also need evidence about migration to other platforms. A takedown count alone cannot show a net reduction in exposure. The same applies to website blocking, app removal and payment interruption. These measure enforcement activity unless linked to a validated measure of remaining access or participation.

10. Licensed, illegal and offshore systems

Licensed markets generally provide an identifiable regulator, specified authorization, complaint routes and compliance obligations. The existence and scope of these arrangements can be verified. Their effectiveness must be measured separately. The Crown and William Hill cases document failures in licensed markets, ruling out any assumption that authorization guarantees harmless operation. Conversely, an operator's lack of the required licence establishes a legal problem without proving involvement in every other category of crime. AUSTRAC, 2023 Gambling Commission, 2023

The Australian interactive study found substantial offshore use within its online sample, but that sample was explicitly nonrepresentative. Its offshore-use proportion must not be presented as a national adult rate. The evidence also shows a problem with consumer perceptions. Consumers may not reliably distinguish domestic authorization from a site's accessible interface or familiar presentation. Gambling Research Australia, 2021

The British regulator's research on individual online lotteries and raffles describes discovery through social channels and the role of familiarity and perceived trust. Such material can look like a community activity while raising different legal questions from a genuine free draw. The research is useful for identifying pathways and perceptions, not for estimating the national frequency of illegal raffles. Gambling Commission, 2026

Possible routes of adaptation include alternative domains, new promotional intermediaries, private communities, offshore supply and different payment methods. Documented cases and regulator responses support the existence of these routes, but this review does not assign them a global market share. It also cannot estimate how a restriction changes the net balance between substitution into licensed provision, exit from gambling and displacement into prohibited provision. ACMA, 2026 UNODC, convergence report, 2024

Cryptocurrency changes payment and settlement possibilities, including cross-border transfers, but is neither inherently anonymous nor evidence of criminality. The relevant questions concern customer identification, source-of-funds checks, service jurisdiction, asset convertibility and transaction tracing. Australia's specified payment restrictions and UNODC's network investigations provide distinct forms of evidence: one is a legal control, the other concerns documented criminal infrastructure. They should not be combined into a claim that every crypto-gambling customer is offending. ACMA, 2024 UNODC, casinos report, 2024

11. Crime and financial-crime relationships

The unit of crime analysis must be a defined activity. The categories below cannot be added into a single rate without double counting and incompatible denominators.

Category	Relevant evidence	Suitable denominator or comparison	Main inferential limit
Unlawful provision	Licensing records, prosecutions, regulator investigations	Operators, transactions or verified market share within a defined legal scope	Detection intensity and legal definitions change
Operator fraud or nonpayment	Complaints, judgments, adjudicated cases	Customers or transactions exposed to the relevant operator	Reporting and access to remedies differ

Category	Relevant evidence	Suitable denominator or comparison	Main inferential limit
AML-control failure	Regulatory findings and civil penalties	Audited entities or inspected transactions	Failure to maintain controls is not a measured laundering amount
Laundering and underground banking	Case-linked financial investigations	Identified flows or networks, with uncertainty	Seizures and detected flows omit undetected activity
Cyberfraud and coercive criminal networks	Investigative records and victim evidence	Clearly defined offences and affected populations	Case selection prevents global prevalence inference
Identity/payment fraud	Verified account or transaction fraud	Accounts/transactions at risk	Operator reporting systems and definitions differ
Match fixing and corruption	Integrity investigations and adjudicated cases	Events or participants under consistent monitoring	Alerts are not convictions; monitoring changes
Individual theft or embezzlement	Offender, treatment and longitudinal records	Defined at-risk samples or population offence rates	Selection and temporal ordering are often unresolved

Adolphe and colleagues' systematic review of 21 papers found a substantial body of research on gambling problems and income-generating, often nonviolent offending. Treatment and offender samples are informative about pathways, but cannot establish how much national theft would change after a particular advertising policy. Debt, impulsivity, substance use and prior offending can affect both gambling and crime. The review does not justify treating every association with violence as a demonstrated gambling-caused effect. Adolphe et al., 2019

UNODC's 2024 reports document convergence between casino-related systems, underground banking, cyber-enabled fraud and organized-crime networks in East and Southeast Asia. These documented mechanisms are important, particularly where cross-border payment and business structures obstruct enforcement. The evidence comes from cases and intelligence, not a probability sample of all casinos or gambling customers. It cannot be used to calculate a comparable country crime rate. Nor can it be used to infer that ordinary gambling advertising caused the observed networks. UNODC, casinos report, 2024 UNODC, convergence report, 2024

No new content-crime correlation is calculated here. The assembled crime material lacks a consistent exposure measure, common offence definitions, detection-adjusted denominators and matched time coverage. A correlation between Spanish marketing expenditure and unrelated national fraud totals would mix jurisdictions, mechanisms and measurement systems. Even a within-country correlation could mainly reflect changes in internet fraud reporting, policing or payment adoption.

For a credible future test, researchers would need separately coded offences, stable reporting rules, policy or platform variation plausibly independent of crime trends, and measures of displacement. Identity theft, match fixing and operator fraud warrant distinct designs. The absence of such an estimate in this report is a finding about evidentiary limits, not evidence that no relationship exists.

12. Harm, historical cohorts and consumer pathways

12.1 What counts as harm?

Participation is an inadequate proxy for harm because intensity, product design, financial resources and personal circumstances differ. The latest GSGB reports 47% past-four-week participation among adults and a PGSI score of 8+ among 2.4%. Online participation was 38%, falling to 16% when lottery-only participants were excluded. These figures measure different concepts. PGSI also uses a different recall framework. The new push-to-web survey is not directly comparable with earlier health-survey estimates. Ridout et al., 2026

A symptom screen is also different from a clinical diagnosis or a documented financial consequence. Reports of reducing essential spending, relationship breakdown and help-seeking should retain their original denominators. People affected by someone else's gambling constitute another population. A research design focused only on active accounts can miss family consequences and people who have stopped gambling because of harm.

The same British survey also records positive experiences. Among past-year gamblers, 42% rated their last gambling experience positively, 36% neutrally and 22% negatively. Common stated motivations included the chance of a large win and enjoyment. Among past-year gamblers, 3.4% sought support for their own gambling. These findings describe different experiences within the population. Enjoyment does not rule out harm, and help-seeking is not a direct estimate of everyone who needs help. Ridout et al., 2026

Gainsbury's review explains why internet use should not be treated as a sufficient explanation of disorder. Online gamblers may differ in involvement, product variety and pre-existing difficulties. Access, speed and convenience can plausibly affect intensity, but crude online/offline comparisons also reflect selection. Product-specific and within-person studies are needed to separate these pathways. Gainsbury, 2015

12.2 What changed between earlier and later cohorts?

Nelson and colleagues compared a 2015 cohort of 32,262 new European sportsbook subscribers with an earlier 2005 cohort. Across eight months, the later cohort's median activity was 15 bets and EUR 25 net loss. Those in the upper tail were much more involved. Typical behavior was broadly stable despite market changes. The study compares subscriber cohorts at one operator, rather than the same people across the periods. It does not measure all their gambling. Its main contribution is evidence against assuming universal escalation. Nelson et al., 2021



Source: Gambling Research Australia (2021), national telephone survey comparison.
2019 n = 15,000; original percentages. Repeated cross-sections, not the same individuals.
The report separately found increased screened problem gambling; participation is not harm.

Figure 5. Australian repeated cross-sectional national telephone-survey estimates, as reported in the 2021 interactive gambling study. The 2019 telephone sample contained 15,000 adults. The separate online and longitudinal samples are not used as national prevalence denominators. The rise in screened problem gambling reported by the study should be read alongside, not inferred from, these participation bars. Gambling Research Australia, 2021

Dimension	Earlier-period evidence	Contemporary evidence	What remains unresolved
Entry route	Web/desktop and existing gambling interest are prominent in early online research	Search, apps, sports integration, creator content and direct notifications offer more routes	No comparable population history of first digital exposure across all cases
Demographic profile	Earlier online samples often differed from the general population	Sex, age, education and vulnerability patterns vary with product and sample	A single global shift toward one “typical victim” cannot be estimated
Typical betting intensity	Early operator data provide a baseline	The 2005/2015 comparison found stability for typical sportsbook behavior	Later platform migration, other accounts and clinical outcomes were unobserved
Financial consequences	Gambling-related distress predates mobile apps	Bank records and policy studies provide better evidence of financial consequences	How much is attributable specifically to content rather than access/product design
Legal understanding	Cross-border authorization was already complex	Influencer ownership, social raffles and accessible offshore services add classification problems	Representative longitudinal changes in perceived legality and trust
Self-exclusion and return	Operator-based exclusion predates current personalization	Direct-marketing suppression can be evaluated experimentally	Cross-operator return, migration and sustained clinical recovery

The table is a synthesis, not a claim that all people followed the same route. The evidence supports a broader range of entry and return channels. It does **not** establish a universal shift in age of first exposure, a quantified acceleration from first click to harm, or a single new demographic victim profile. Repeated cross-sections confound age, historical period and birth cohort. Only appropriate longitudinal designs can separate them.

13. COVID-19: disruption, substitution and persistence

COVID affected opportunities in opposing directions. Venue closures reduced access to land-based products, and interruptions to sports reduced betting opportunities. Time at home could increase online availability. Changes in income, family presence and stress could also reduce or increase gambling. These are competing mechanisms, not a single treatment shared across countries.

Brodeur and colleagues' early scoping review included 24 articles, of which 14 were original studies. It found that gambling often decreased or remained stable for most participants. Increases were associated with greater problems in a subgroup. The literature was early and geographically concentrated, including several Swedish studies. It cannot settle the persistent post-pandemic effect in every jurisdiction. Brodeur et al., 2021

Spain's series illustrates how difficult it is to attribute changes to a cause. Marketing peaked around 2020-2021, fell in 2022, and later rose. GGR followed a different path. Pandemic conditions overlapped with advertising reforms and changes in products and sports calendars. The analysis therefore includes a defined early-pandemic indicator and an exclusion sensitivity. It does not treat their coefficients as the total effect of COVID. The data support disruption and heterogeneous responses more clearly than a permanent, universal upward break.

14. Original statistical analysis

14.1 Data and specification

The analytical sample covers the Spanish licensed online market, January 2013-June 2026. It contains 162 months in levels, 161 after first differences, and 160 in the main conditional model after a lag. There are no missing months in the three joined series. Values are nominal euros and account counts. No imputation, winsorization, interpolation or conversion to unique people is performed.

The main variables are total marketing, GGR and active accounts. Marketing includes bonuses and is an input proxy, not measured content exposure. Natural logarithms reduce scale differences. First differences compare proportional changes between adjacent months. They remove much of the common long-run trend, but do not remove all confounding or reverse causation.

The primary conditional model is:

```
change in ln(GGR)t = constant + beta * change in ln(marketing)t + gamma * change in ln(active accounts)t + phi * change in ln(GGR)t-1 + calendar-month effects + March-June2020 indicator + error
```

OLS uses Newey-West/HAC covariance with a maximum lag of three months. A sensitivity check uses a 12-month covariance bandwidth. Other checks use lagged marketing and exclude 2020-2021 observations. Differences are calculated before exclusion to avoid a false two-year difference. In the exclusion model, covariance pairs use actual calendar distances. This avoids treating retained

observations on either side of the gap as adjacent months. No single residual autocorrelation test is reported across that discontinuous sample.

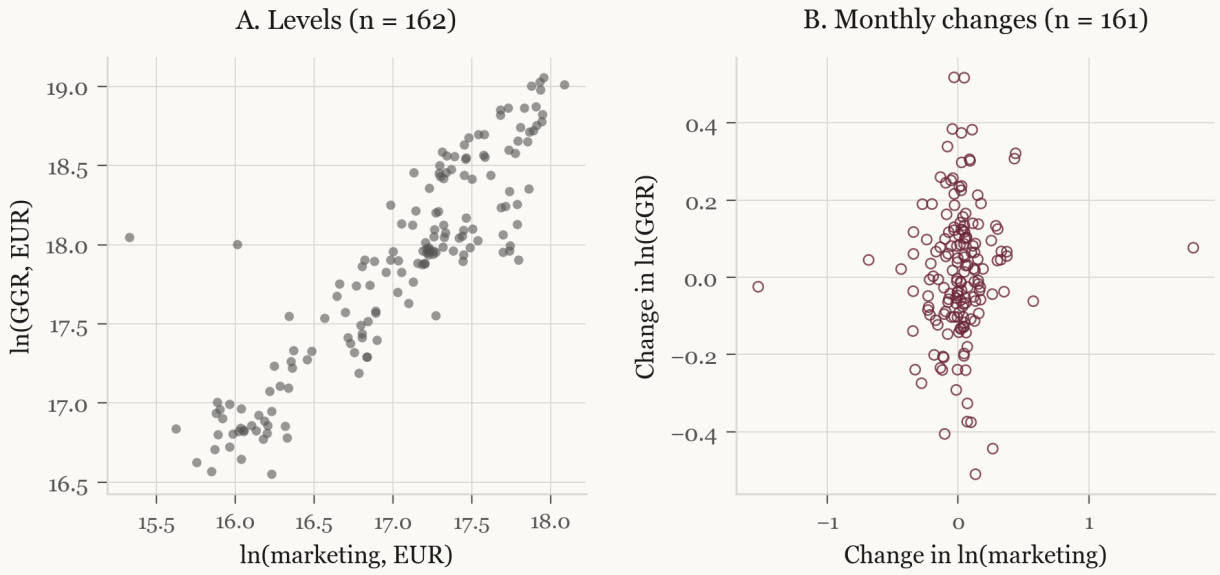
Active accounts may be jointly determined with revenue or lie on a pathway from marketing to spending. Conditioning on active accounts changes the estimand and can remove part of a real effect. The model therefore estimates a conditional association, not the total causal effect of advertising. It does not directly control monthly income, unemployment, product mix, sports schedules, platform reach or all regulatory changes.

14.2 Correlations and uncertainty

Series pair	Statistic	n	Estimate	95% paired block-bootstrap interval, six-month blocks
ln(marketing), ln(GGR)	Pearson	162	0.894	0.773 to 0.956
Monthly change in ln(marketing), change in ln(GGR)	Pearson	161	0.087	approximately 0.000 to 0.222
Same monthly changes	Spearman	161	0.109	-0.026 to 0.246
Previous-month marketing change, current GGR change	Pearson	160	0.007	-0.076 to 0.145

Intervals use 5,000 paired circular block-bootstrap draws with seed 20260929. Subject to its assumptions, the paired procedure preserves joint movement within blocks and some temporal dependence. This does not guarantee stationarity. In particular, intervals around trending levels are descriptive sensitivity summaries. They are not evidence that the relationship in levels represents a stable structural parameter.

The lower bound for the primary change correlation is about 0.0000013 with six-month blocks, but becomes negative with three-month blocks (-0.004) and 12-month blocks (-0.006). The result should not be presented as a robust statistical discovery based on crossing a threshold. The data show a small association in changes that is sensitive to the specification, compared with a strong association in trending levels. The lagged correlation is also close to zero.



Source: DGOJ, January 2013-June 2026. Natural logarithms; one first difference lost.
Points are months, not people. Common trends and simultaneous demand prevent causal inference.

Figure 6. Each point is a month. The panels use different transformations and should not be read as equivalent scales. Source: the preserved DGOJ snapshot; all chart data and code are supplied.

14.3 Conditional estimates and diagnostics

Specification	n	Marketing coefficient	95% interval	Asymptotic p-value
Main model, HAC lag 3	160	0.036	-0.059 to 0.132	0.457
Same coefficients, HAC lag 12	160	0.036	-0.055 to 0.127	0.436
Lagged marketing, HAC lag 3	160	0.010	-0.085 to 0.104	0.844
Exclude 2020-2021, calendar-aware HAC	136	0.134	-0.035 to 0.303	0.121
Post hoc 12-lag outcome dynamics, HAC lag 12	149	0.021	-0.060 to 0.103	0.611

A coefficient near 0.036 means that a 1% monthly increase in marketing is associated with approximately 0.036% higher GGR change, conditional on this specification. The interval includes negative and positive associations. It is not an individual response, a return on advertising expenditure, or a prediction of the effect of an advertising ban.

The primary residual Ljung-Box test at lag 12 rejects residual independence (p approximately 0.000087). HAC standard errors address some covariance misspecification but do not repair omitted dynamics or endogeneity. After this diagnostic, one explicitly **post hoc** sensitivity added outcome lags 2 through 12. Its marketing estimate remained small and uncertain, and the residual test no longer rejected independence. With many regressors and 149 observations, that model is a diagnostic comparison rather than a preferred causal result. The specification history is recorded in the package.

These estimates do not contradict experiments showing a response to particular communications. Aggregate monthly spending is a different exposure, covers a different population and can hide offsetting product or subgroup effects. The estimates also do not prove that marketing is ineffective. The defensible finding is that the strong Spanish correlation in levels does not persist in the same way after basic trend adjustment. The chosen aggregate models do not identify a precise causal effect.

14.4 Why no new difference-in-differences estimate?

A credible design needs a defensible comparison group, treatment timing, stable outcome coverage and plausible parallel trends. The Spanish files cover one treated market, phased rules, pandemic overlap and a later court change. Fitting an intervention line without a convincing counterfactual would overstate what the dataset can establish. The report instead evaluates stronger published designs and provides transparent descriptive and conditional analyses. García-Pérez and colleagues' earlier Spanish marketing study and the later policy analysis provide relevant context. Their model choices are not a substitute for identification. García-Pérez et al., 2024 Aonso-Diego et al., 2025

15. Conclusions, contradictions and remaining uncertainty

Conclusion	Evidence supporting it	Counterevidence or alternative explanation	Assessment
Digital access and commercial distribution expanded	Connectivity series, national online-participation comparisons, Spanish spending and varied platform rules	No global content census; spending growth includes scale and prices	Strongly supported descriptively; magnitude is ind icator-specific
Commercial communication can affect some choices and activity	Experiments, direct-marketing trial and review evidence	Null primary trial outcome; heterogeneous survey results; reverse causation	Moderately supported, with some narrow experimental evidence
More online gambling implies everyone gambles more	No adequate evidence for this universal claim	Australian total participation decline and stable typical sportsbook cohort behavior	Unsupported as a general proposition
Financial consequences can be concentrated	Bank-record associations and US policy studies	Selection, household heterogeneity and policy bundles	Strong association; moderate quasi-causal support for access effects
The typical path to harm has changed everywhere in the same way	More routes for entry and return are documented	Sparse life-course panels and incompatible historical samples	Uncertain; routes broadened, a universal trajectory is unestablished
Income group causes differences in harm	National contexts and data availability differ	Internet access, products, demographics and reporting are not harmonized	Not identified in these data
Content growth causes a measurable global rise in gambling-linked crime	Defined cases demonstrate real criminal and regulatory problems	No matched exposure/crime panel; enforcement intensity changes	Unsupported as a quantified global causal claim
A stated restriction reduces exposure or harm by itself	Some interventions change measured outcomes	Evasion, substitution, implementation gaps and null endpoints	Must be evaluated intervention by intervention

The internet has made gambling available through more routes and placed promotional material inside activities that are not themselves gambling, including sports viewing, entertainment and social interaction. The clearest changes concern access, distribution and which market activity can be observed. Conclusions about harm require closer attention to product, intensity and financial circumstances. Conclusions about crime also require a distinction between illegal provision, financial-control failures and specific criminal offences.

The evidence is least useful for reaching a single worldwide numerical verdict. It is more useful for identifying which mechanisms have direct support, which findings depend on a local sample, and where an appealing narrative goes beyond the data. Contradictory findings need to remain part of the synthesis. They show why growth in a channel, total participation, ordinary behavior and severe harm must be measured independently.

16. Research priorities and limitations

The most useful next studies would link exposure measurements collected with consent to longitudinal behavior and outcomes across operators, while protecting privacy and retaining a clear sampling frame. They should include paid and organic content, record platform changes and measure people who never gamble as well as active customers. Self-reported recall alone cannot fully separate targeting from selective attention.

For low- and middle-income settings, repeated probability surveys and transparent regulatory statistics would add more than importing an effect size from Australian sports bettors. Such work should record mobile-money access, product legality, disposable resources, urban/rural conditions and household consequences. Local school samples are valuable for detecting concerns, but cannot replace national denominators.

Policy evaluation should distinguish announced, enacted and effective dates. It should measure implementation and test displacement into other products, operators or platforms. Direct-marketing experiments should specify primary endpoints in advance and follow cross-operator outcomes where lawful and feasible. Financial studies should report heterogeneity without equating low national income with individual vulnerability.

Crime research needs stable offence definitions and transparency about detection. A blocked domain, suspicious-transaction report, customer complaint, civil penalty and conviction represent different observations. Comparable indicators should be published separately. Each should state the population or transaction base and any changes in investigative effort.

Retrieval in this review was English-led, and access to full texts and proprietary data was uneven. The review used a single synthesis process and a purposive country sample. The review lacks complete product-by-year legal verification and harmonized platform exposure data. Some funding details and sample-specific denominators could not be independently extracted and are flagged. The report does not claim an exhaustive legal opinion, a registered systematic review, or a causal model of global gambling harm.

17. Reproducibility statement

The accompanying package preserves the original DGOJ files and WDI JSON, along with manually transcribed survey observations and their denominators. It includes cleaned and analytical datasets, six chart source tables, analysis and chart code, model results, source and evidence registries, a legal timeline and product classifications. It also contains a variable dictionary, transformation records and software versions. Raw, processed and analytical material are separated. Deterministic bootstrap seeds and checksums identify the calculation inputs.

The main numerical analysis runs without a network connection after installing the recorded dependencies. The Spanish calculations and six figures can be reproduced from the preserved inputs. Full copyrighted reports and proprietary person-level datasets are not redistributed. Stable retrieval

links and version notes identify the material used. The qualitative synthesis and source selection require human review. They are not claimed to be mechanically reproducible merely because tables and code are supplied.

Appendix A. Search strategy and exclusions

The search was organized by concepts rather than by a desired conclusion. Example query families included `(gambl* OR betting OR casino) AND (advertising OR affiliate OR influencer OR livestream*)`; `(online gambling) AND (longitudinal OR experiment OR randomized OR difference-in-differences)`; `(gambl*) AND (crime OR fraud OR money laundering OR embezzlement)`; and `(gambling) AND (COVID OR pandemic)`. Each was supplemented with country names, youth/age/sex/education terms, and searches restricted to regulators, courts or government domains where relevant.

The review checked accessible records against original abstracts or full texts, and followed selected references and later publications. Crossref metadata matches are archived. The package's search document records representative queries and retrieval routes. It does not claim a complete machine-preserved log of every discovery result. No database export counts, deduplication totals or inter-rater agreement statistics are invented.

The review excluded affiliate-site market estimates without transparent methods. It did not combine search-interest downloads with incompatible normalization or count domain blocks as illegal-market prevalence. It also avoided treating a convenience offshore-user sample as national prevalence, pooling GB's older survey series with the new GSGB baseline, or using South African betting GGR as online-only. Incomplete 2026 totals were not treated as annual observations, and unmatched gambling and crime series were not correlated.

Appendix B. Demographic harmonization rules

All original age bands are retained. British 11-17, Kenyan 18-25 and Ugandan 11-25 samples are not recoded into a common youth band without underlying microdata. Sex/gender labels follow the source and are not treated as interchangeable across questionnaires. Education categories are compared within a survey, not as standardized qualifications across countries.

Recall windows remain explicit: past four weeks, past month, past year, current activity and lifetime participation are different measures. Denominators distinguish all adults, all pupils, gamblers and content-following subgroups. Account counts remain accounts. PGSI and DSM-based screening thresholds remain instrument-specific. Survey estimates are not weighted together unless their original designs and target populations permit it. No such pooling is attempted here.

Appendix C. Common concerns

Concern	Evidence-based answer and location
1. How much did internet gambling content grow?	No defensible global percentage; Spain's commercial spending and local content audits provide partial indicators (Section 4).
2. When were the largest transformations?	Overlapping web, mobile/social, pandemic and recent policy transitions; no universal change point (Section 3).
3. Which technologies mattered?	Internet access, smartphones, payments, platforms and account communication; separate contributions not identified (Sections 3,5).
4. How did distribution change?	More search, affiliate, creator, community and retention pathways (Section 5).
5. Who receives greatest exposure?	Depends on channel and sample; no global demographic ranking (Section 6).
6. Sex differences?	Some male-skewed participation and conditional creator-ad measures; not universal exposure effects (Section 6).
7. Age differences?	Youth exposure is substantial in GB; Kenyan participation peaks in younger adult bands; age definitions differ (Section 6).
8. Socioeconomic differences?	Participation and financial vulnerability need separate analysis (Section 6.3).
9. Income-group differences?	Connectivity, law and observation differ; an independent income effect is unestablished (Section 7).
10. Did participation move with content?	Sometimes; Australian overall participation declined while online participation rose (Sections 4,12).
11. Did harm change?	Some source-specific increases and financial effects; incompatible instruments prevent a global trend (Sections 8,12).
12. Did crime indicators change?	Serious cases are documented; no consistent content-linked crime trajectory assembled (Section 11).
13. What survives controls?	Some youth online-ad associations; Spanish monthly aggregate coefficients remain uncertain (Sections 6.2,14).
14. Credible causal evidence?	Narrow experiments and a mixed RCT; stronger policy evidence concerns access, not all content (Sections 8,14).
15. How did regulation respond?	Licensing, prohibition, payments, promotion rules and platform controls (Section 9).
16. Which interventions changed outcomes?	Direct-solicitation trial secondary outcomes; suggestive Spanish policy estimates; identification limits retained (Sections 8,9.4).
17. Illegal-operator adaptation?	Cross-border supply and alternative distribution/payment routes documented, with unknown aggregate shares (Section 10).
18. Legal-operator adaptation?	Recorded spending composition and compliance obligations changed; motive or net policy effect cannot be inferred from totals (Sections 4,9).

Question	Evidence-based answer and location
19. Platform influence?	Rules structure permitted links, promotion and categories; actual enforcement effects remain less measured (Section 5.1).
20. Did the affected profile change?	Access broadened; no single statistically defensible global victim profile (Section 12).
21. Did paths to harm change?	Entry/return routes broadened; longitudinal progression is inadequately measured (Section 12.2).
22. Influencers, sports, affiliates, algorithms?	Distinct incentives and pathways, with unequal causal evidence (Sections 5,8).
23. Permanent COVID effect?	Heterogeneous disruption established more clearly than universal persistence (Section 13).
24. Offshore/illegal-market size?	Important documented activity, but no harmonized global share established (Section 10).
25. Largest research gaps?	Exposure panels, low-income longitudinal evidence, displacement, legal implementation and detection-adjusted crime measures (Section 16).

Appendix D. Statistical audit and package map

The analysis specification was written before model estimation, but after source inspection and some descriptive totals. It was not prospectively registered. The 12-lag dynamic sensitivity was added only after residual dependence appeared, and its post hoc status is retained. Bootstrap intervals assume that the selected block procedure reasonably represents dependence. The near-zero bound is explicitly tested against alternative block lengths. All p-values are exploratory, with no claim of familywise error control across the sensitivity set.

`raw/` holds original statistical files and authored transcription records. `processed/` holds monthly and annual Spain data, country-year context, demographic evidence and legal tables. `analysis/` holds model inputs, correlations, coefficients, residuals and software-readable results. `chart_data/` and `charts/` hold plotted values and PNG/SVG figures. `sources/` holds the bibliography, source registry and evidence/quality matrices. `documentation/` holds the dictionary, transformations, methods, protocol and versions. `code/` contains the reproducible analysis and demographic-figure scripts. The README specifies commands and limitations.

Appendix E. Selected evidence and source-quality matrix

The full 26-row evidence matrix and its source-quality companion are included in the package. This printed selection emphasizes studies that materially affect interpretation. Confidence concerns the stated inference, not a numerical quality score.

Source and design	Population and result	Limitation; funding/COI	Confidence
McGrane et al., 2025. Systematic review	22 studies; 16 Australia; Mostly Australian sports bettors. Positive associations across several behavior outcomes; heterogeneous measures. No pooled estimate used	Self-report; reverse causation; geographic concentration; reviews overlap. Wellcome doctoral funding; original studies require separate checks	Moderate for influence, low for population harm magnitude
Nelson et al., 2021. Operator-account cohort comparison	32,262 in contemporary cohort; New European sportsbook subscribers. Typical activity broadly stable; concentrated upper tail. Median 15 bets, EUR 25 net loss over 8 months	One operator; not same-person panel; no cross-operator clinical harm. Entain/GVC research contract; authors state independent analysis	Moderate within operator; low generalizability
Houghton & Moss, 2021. Repeated-measures 3 x 2 experiment	145; Regular football bettors. Affiliate label increased confidence/likelihood for medium-complexity bets. No long-term harm effect measured	Hypothetical judgments; selected bettors; interaction not uniform. GambleAware PhD support; no funder design role reported	Moderate for immediate response
Noble et al., 2022. Cross-sectional school survey	6,377 final sample; model-specific complete cases differ; Victoria and Queensland students aged 12-17. Online promotion exposure associated with past-month gambling; adjusted harder-gambling associations null. Adjusted OR 1.37,95%CI 1.01-1.85 for past-month gambling	Self-report; reverse causation; school coverage; 272 missing follow-up responses treated as no past-month gambling. Victorian Responsible Gambling Foundation and public health/cancer bodies; no competing interests declared	Moderate association; low causality
von Hammerstein et al., 2025. Randomized controlled trial	2,548 (1,265 intervention; 1,283 control); French operator customers self-excluding up to 3 months. Primary net-loss outcome null; secondary deposit reductions larger at months 6,9,12. Month 9 deposit reduction EUR 451.92 vs EUR 343.21; p=.040	Multiple secondary endpoints; one operator; migration unobserved. Luquiens declares data agreements with FDJ and Winamax; no funding under those conventions	High design quality; mixed endpoint evidence
Baker et al., 2026. Staggered policy DiD, household transactions	Large panel; version-specific n not transcribed; US households in financial transaction panel. Reduced investment and worsened financial constraints after access expansion. Directional findings only; no version-mixed coefficients	Parallel trends; policy bundles; selected financial-data panel; not ad-specific. Full working paper acknowledged research support; complete journal COI not separately verified	Moderate quasi-causal evidence for access

Source and design	Population and result	Limitation; funding/COI	Confidence
Aonso-Diego et al., 2025. SARIMA intervention analysis	44 quarters; Spanish licensed online market. Lower new accounts and stakes in selected intervention models. New accounts -263 k (p=.003); stakes -EUR 216 m (p=.034), study scale	Exploratory event/model selection; COVID overlap; no untreated comparator; later court change. Spanish public predoctoral/postdoctoral grants; no funder role reported	Suggestive policy attribution
Anyanwu et al., 2023. Cross-sectional school survey	905 analyzed; 921 recruited; 12 schools, Mbarara; ages 11-25. 40% ever gambled; 17.7% screen positive at DSM-IV threshold 4+. 44.2% screen positive among ever gamblers	Mixed minor/adult sample; local schools; no national generalization. Public research grants; several authors disclose gambling-industry relationships unrelated to this study	Moderate within sample; low population generalizability
DGOJ, 2026. Operator regulatory returns	162 months; Spain licensed online market. Original calculations in analysis folder. See results.json	No illegal market, reach or unique persons; nominal EUR. Government regulator; operator self-report obligations	High for recorded market; low as content exposure proxy
CBK, KNBS & FSD Kenya, 2024. Weighted multi-stage household survey	28,275 designed households; 84.6%response; subgroup n unavailable; Kenyan adults 18+ in published analysis. Betting 11.2%; men 18.4%,women 4.4%; urban 14.4%,rural 8.9%. Age 26-35 15.2%; education tertiary 17.5%	Not online-only; no causal SES effect; retain original age bands. CBK/KNBS/FSD Kenya	High descriptive within definition
Adolphe et al., 2019. Systematic review	21 papers; Often treatment, offender or selected samples. Income-generating nonviolent offending prominent; violence pathway uncertain. No national crime elasticity	Selection; temporal ordering; heterogeneous crimes. Not independently verified in accessed record	Moderate association; low national causal inference
UNODC, casinos report, 2024. Investigative and enforcement synthesis	Case-based, no representative denominator; East/Southeast Asian documented networks. Documented convergence of casinos, underground banking and crime. No population rate	Detection/selection bias; cannot infer all offshore operators criminal. UNODC institutional report	High for documented cases; low prevalence inference

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