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Gambling With Money You Don't Have: Understanding the Interaction Between Credit Cards and Sports Gambling.

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Summary

In June 2024, the Australian government banned the use of credit cards for online gambling. They argued the ban would prevent people from borrowing to fund gambling and, in doing so, reduce gambling harms. Using anonymised, consented and aggregated transaction data, we evaluate the policy's effect. We focus on its impact on online sports gambling, which is the majority of the online market.

We find that:

- **Credit cards made up only a small share of online sports gambling pre-ban.** By the beginning of 2024, only 2% of credit card accounts were used for gambling. This likely reflects financial disincentives to doing so: gambling on credit cards was treated as a cash advance – subject to punitive fees and higher rates of interest.
- **Gambling with a credit card was associated with a stronger financial position.** On average, gambling with a credit card was associated with higher incomes and more cash on hand.
- **The policy reduced online gambling for this group.** On average, gambling expenditure fell by roughly $50 per fortnight. This was driven by lower participation - specifically a 15% fall in the probability of gambling per fortnight. We find that around one third of the affected group stopped gambling altogether - that is they had no recorded gambling transactions in the six weeks following the ban.
- **These effects reflect inconvenience rather than credit constraints.** Declines in participation were largest for small bets (spending less than $10 per week) and were not correlated with financial outcomes such as liquidity constraints or debt holding.

Our results suggest that gambling behaviour can be responsive to policy change, particularly policies that make gambling more inconvenient. However, if the aim of policy is to reduce gambling harms, greater returns may come from targeting other forms of gambling – particularly poker machines – where harms appear to be more concentrated.

Gambling receives significant public attention. Estimates suggest that Australians gamble the most per capita in the world (Peake, 2025), while ranking around the middle globally on metrics of problem gambling (Figure C.1). Problem gambling has well documented harms across a multitude of dimensions - including financial stress, mental health, family outcomes and welfare (Adolphe et al., 2019; Dowling et al., 2014; Langham et al., 2016). Given this, policymakers have come under public pressure to act to reduce gambling harms.

Different types of gambling exhibit different patterns of use, risk and harm. Accordingly, the efficacy of policy interventions is highest when targeting the sections of the market where the most harm occurs. Increasingly, concern has centred on the rapid rise of online gambling, particularly sports wagering, with a popular narrative that the expansion of this market is associated with more gambling harms.[1] From 2021 to 2023, there were three Parliamentary inquiries into features of the online gambling landscape and potential policy responses (Parliamentary Joint Committee on Corporations and Financial Services, 2020, 2021; Senate Select Committee on Gambling Reform, 2020). One recommendation acted on and implemented by the current Federal government was to ban the use of credit cards for online sports betting (with the exception of lotteries[2]) - despite there being limited empirical evidence on the relationship between credit card use and problem gambling.[3] At the time, the Communications Minister Michelle Rowland explained that: "People should not be betting with money they don't have" (Rowland, 2024).

1 Aggregate data shows that wagering (the category including sports betting) is the only category of gambling that is not in long run decline. Survey evidence shows that online sports betting has increased, but remains a small part of the overall market (Australian Institute of Health and Welfare, 2025; GambleAware NSW, 2024). Poker machine use remains more correlated with problem gambling than sports betting (Maltman, 2025).
2 Credit cards were already banned for in-person gambling prior to the policy, such that the change brought the two in line with one another.
3 The Joint Parliamentary Committee noted the limited evidence on the topic, stating "While the data is limited on the links between the use of credit cards and digital wallets and gambling behaviour and gambling harm, it is clear that for a certain cohort of citizens (and their families), the harmful consequences can be significant and life changing." (Parliamentary Joint Committee on Corporations and Financial Services, 2021, Page 19)

So, what is the role of credit cards in driving problematic gambling behaviour? In principle, people may use their credit card to gamble either to fund expenditures through borrowing or simply as a matter of convenience. As such, the policy may affect behaviour through two channels.

1. **For those borrowing to gamble,** it imposes a borrowing constraint that may result in reduced gambling.
2. **For those using it for convenience,** it imposes frictions associated with changing behaviour. These frictions could take many forms, but the data do not allow us to clearly separate them.[4]

The effects of the ban, and associated implications for gambling harm and future policy decisions, depend crucially on the mechanism through which gambling is affected.

More generally, while there is a substantial body of research on gambling harms and policy, there remains limited empirical evidence on the effectiveness of specific reforms. This reflects the difficulty of accessing granular, non-self reported financial data that allow such policies to be evaluated.

We assess the effectiveness of this policy using anonymised, consented, and aggregated transaction data. We first analyse pre-policy differences in gambling patterns across groups. This allows us to understand the role credit cards were playing in this market. We then assess how the policy change affects gambling activity and financial outcomes.

1. Do credit cards drive gambling harms?

Whether credit cards contributed to gambling harms in Australia is ultimately an empirical question, but there is little evidence currently to draw on.[5] There are reasons to think credit card use may be more or less exposed to gambling harm (Box 1). For instance, in the Household, Income and Labour Dynamics in Australia (HILDA) survey credit card ownership is positively correlated with spending on sports betting but negatively correlated with problem gambling.

Box 1: Understanding the rationale for the policy change

There are a number of reasons to believe that credit cards may contribute to problematic gambling and adverse financial outcomes, on both sides of the market:

1. For individuals with problematic gambling habits, access to credit extends their liquidity, enabling them to gamble beyond available funds and finance this through high-interest short term debt. This can intensify the harms associated with addiction - exacerbating financial stress and impacts on loved ones.
2. For those who casually gamble, credit cards could act as a gateway into riskier behaviour by removing the salience of a natural spending cap from a transaction account.

But there are also reasons to think that credit cards would not drive problem gambling:

1. Individuals holding credit cards typically must pass financial checks, implying that they typically have stronger finances than other groups. For instance, HILDA survey data suggests credit card holders have higher incomes than non-holders.
2. Prior to the ban, there were already disincentives to gamble using a credit card. Gambling transactions on credit cards were treated as cash advances - subject to punitive fees and higher rates of interest.

Prior to the policy, credit cards represented only a minority of the online sports betting market.[6] Industry estimates from the late 2010s suggest around 20% of deposits were made using credit cards (Australian Banking Association, 2020). The transaction data, beginning in 2019, finds that credit cards accounted for a small share of sports gambling, declining steadily from 8% in late 2019 to 2% in the months preceding the ban.[7] This decline mirrors broader trends in falling credit card use

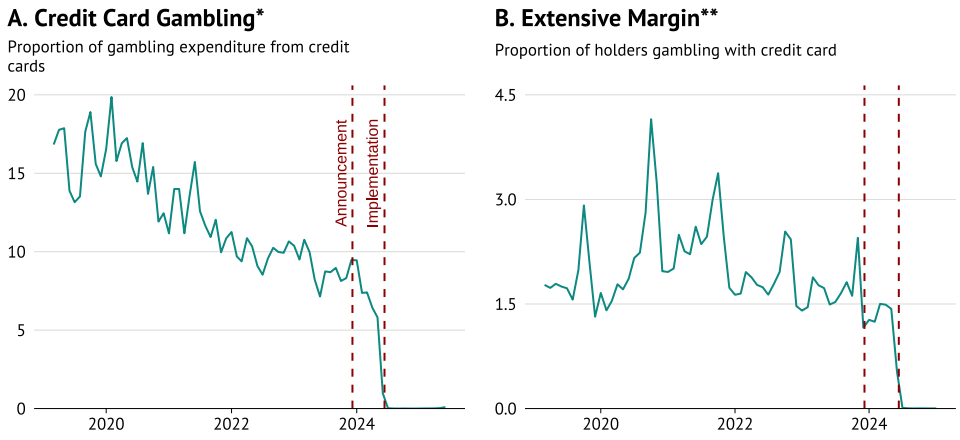
4 Some of this group may not have a debit card. For them, the friction is to set up a debit card account and begin using it for this purpose. Others may have a debit card but not have its details uploaded to their sports gambling account; in this case, the friction is the one-off step of doing so. A final group may already have debit card details loaded into their account. However, even for this group there may be a friction associated with switching to their debit card if for some idiosyncratic reason they prefer using a credit card. Generally, if all these groups typically manage their finances through their credit card, the policy adds a layer of difficulty in doing so, which may in turn discourage gambling. Clearly the form and magnitude of the friction differs, but in each case a friction exists associated with the most convenient option (using a credit card) no longer being available.

5 And the existing evidence remains inconclusive. The only state-level gambling survey to ask about card credit debt, the ACT Gambling Survey, found that just 0.8% of respondents reported increased credit card debt from gambling in 2024 (0.6% in 2019) (Rockloff et al., 2025). A 2018 national survey of online gamblers reported that 7.7% had taken on increased debt due to gambling, though it is uncertain how representative this sample was of the broader population, or gamblers as a whole (Jenkinson et al., 2019). Beyond these studies, the evidence base is extremely limited.

6 Noting that as credit cards were already banned for in-person betting, they represented a smaller minority of the overall market.

7 The sample used in this analysis may skew toward younger Australians, which could explain why we record lower levels of credit card use in 2019 compared with industry estimates (Australian Banking Association, 2020). We discuss this further in Appendix A.2

Figure 1: The use of credit cards for sports gambling



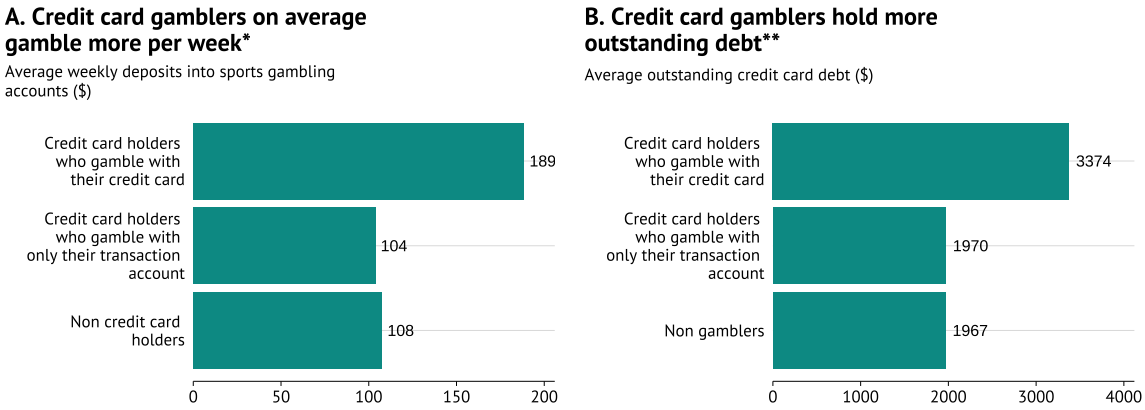
* Figure A plots the proportion of sports gambling expenditures from credit card holders coming from a credit card account in each month
** Figure B plots the proportion of credit card holders using their credit card to gamble each month

in Australia, particularly among younger cohorts.[8] By early 2024, only 2% of cardholders recorded any gambling transactions from that account, and less than 10% of gambling expenditure from those holding a credit card came from that account. This suggests that any harms associated with credit card gambling were already diminishing prior to the policy intervention (although perhaps not zero). After the ban, credit card expenditure dropped to zero, consistent with the policy being successfully implemented.

Accounts using a credit card to gamble on average have stronger financial positions than other groups

While a small proportion of the overall market, accounts using a credit card for sports gambling on average gambled more per week and carried higher levels of outstanding debt than other groups (Figure 2).

Figure 2: Sports gambling and credit card debt across groups

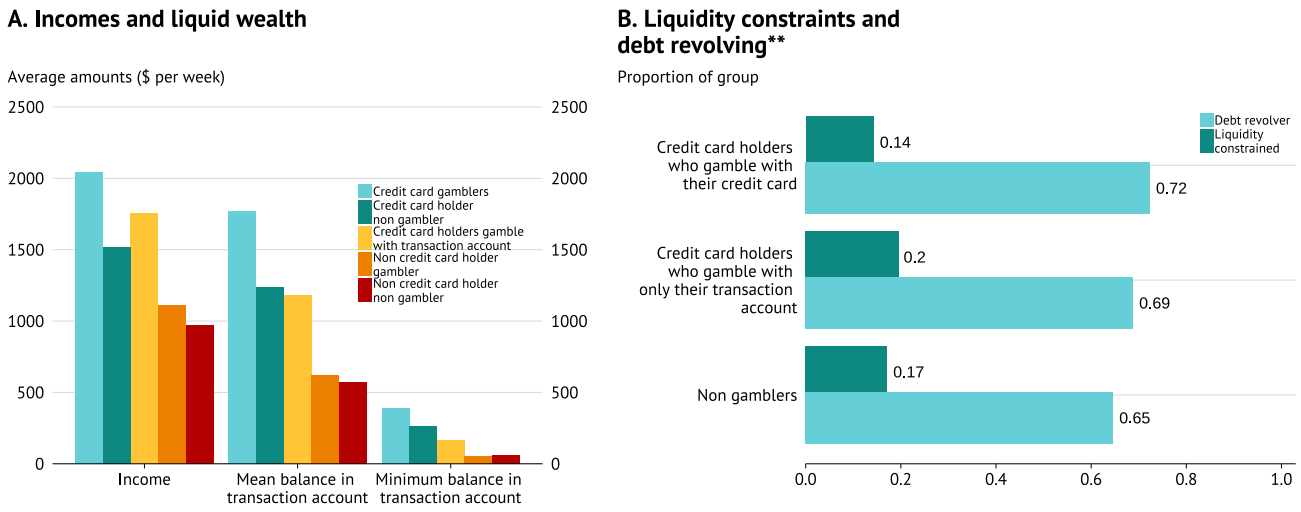


* Figure A shows average weekly deposits into sports gambling accounts across groups. This measure tracks spending - i.e. is not net of winnings.
** Figure B shows the average level of outstanding credit card debt across groups.

However, it is unclear whether these patterns reflected problematic gambling habits or merely differences in incomes, preferences, and characteristics across groups. On average, those who gambled with their credit card had higher incomes and liquid wealth than their counterparts (Figure 3). Additionally, there was no significant difference in the likelihood of them revolving their credit card debt or being liquidity constrained relative to other groups. Finally, a relatively large proportion of their weekly gambling came from their transaction accounts. These patterns suggest that for this group, credit cards may have served as a means of convenience as much as a means of credit provision.

8 According to the HILDA Survey, the share of 25–34 year olds holding a credit card fell from 53% in 2010 to 28% in 2023.

Figure 3: Income and asset holding across groups

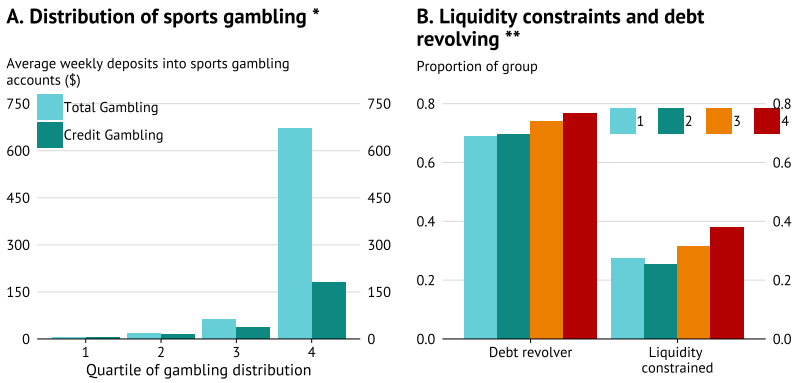


* Figure A shows average levels of income and liquid wealth across groups. Two measures of liquid wealth are provided. The first is the mean balance on an individuals transaction account throughout the week, and the second is the average minimum balance.
** Figure B shows proportions of each group that are classified as being liquidity constrained or as having revolving credit card debt. An individual is liquidity constrained if we observe the balance on their transaction account fall below $100. An individual is a revolver if the outstanding debt on their credit card never falls below $100.

The right-tail of gambling does not appear driven by credit card use

It is difficult to directly identify problem gambling in the data, as it is typically determined by surveys[9]. The above evidence suggests that the average member of this cohort was not in financial stress or debt due their gambling activity, but we cannot rule out that there may be some people within the group that were still exposed to financial harm from gambling.

Figure 4: Outcomes across distribution of credit card gamblers



* Figure A shows average weekly deposits into sports gambling accounts by quartile of the gambling distribution. Quartile 4 is top sports gamblers from the group who gamble with a credit card.
** Figure B shows proportions of each group that are classified as being liquidity constrained or as having revolving credit card debt. An individual is liquidity constrained if we observe the balance on their transaction account fall below $100. An individual is a revolver if the outstanding debt on their credit card never falls below $100.

Within the group using their credit card to gamble, there was large heterogeneity in gambling amounts. Dividing this group into quartiles of weekly gambling expenditure shows that average spending ranges from $10 per week in the lowest quartile to $600 per week in the highest quartile. This large right-tail mirrors what we see across many forms of gambling, as well as sports gambling in the general population.

However, for accounts in the highest quartile, most gambling expenditure originated from transaction accounts rather than credit cards. It therefore seems unlikely that access to short-term debt was driving these large gambling expenditures. Furthermore, there were no clear differences across the gambling expenditure distribution in measures of liquidity constraints

9 The Problem Gambling Severity Index (PGSI) which asks the person 9 questions; for example whether the person has borrowed money or sold anything to get money to gamble, or bet more than they could afford to lose.

or debt revolving behaviour. Taken together, these findings indicate that, on average, credit cards were not driving greater gambling expenditure, nor strongly associated with more negative financial outcomes.

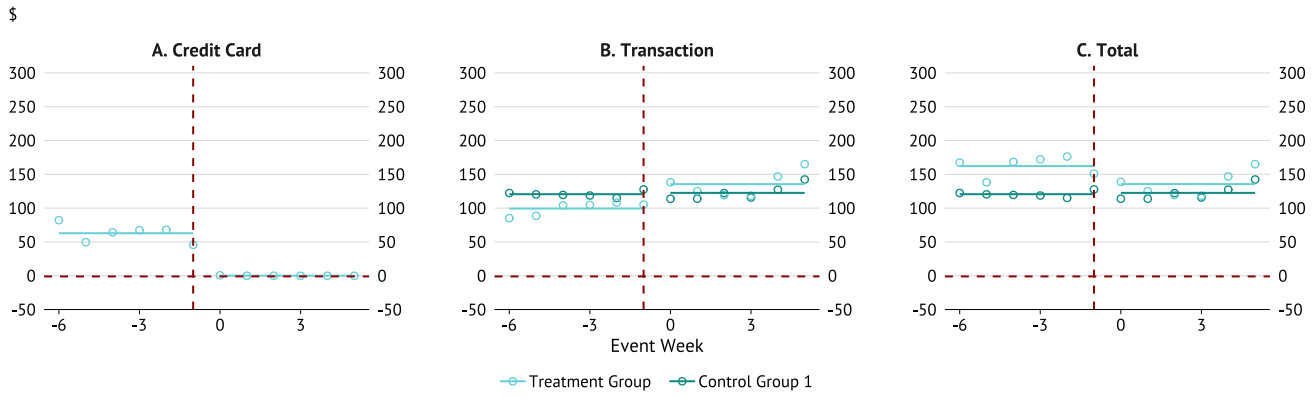
2. What was the impact of the policy?

Approach

To evaluate the effect of banning credit card use for online gambling on gambling behaviour, we construct a balanced panel six weeks before and after the policy change, restricting the sample to where at least one gambling transaction is observed pre-ban. We focus on sports betting as it is the primary form of online gambling.[10] A key strength of the dataset is that it links information across different account types (such as transaction and credit card accounts) at an aggregate level, allowing us to examine how activity shifts between account types following the policy change. Gambling activity is measured as aggregate deposits into sports-betting accounts. The dataset does not capture bets funded from existing balances or winnings that remain in betting accounts.

The data are further aggregated to the fortnightly level, yielding three periods before and three after the policy. The 'treated' group comprises accounts that used a credit card to gamble in the pre-period. Figure 5 plots mean gambling expenditure for this group, showing a modest decline in total sports gambling following the policy change.

Figure 5: Raw data on policy response



* Panel A shows sports gambling expenditures from a credit card account. Panel B shows sports gambling from a transaction account. Panel C shows total sports gambling expenditures. Figures show the treatment group and the first control group - 2024 transaction account gamblers.

Sports gambling expenditure is subject to time trends and seasonality. As such, a credible counterfactual is required to isolate the effect of the policy. We construct two control groups for this purpose:

1. **2024 transaction account gamblers** - accounts that don't hold a credit card and record gambling transactions pre policy change in 2024. Non credit card holders are unaffected by the policy. As such, the gambling expenditure of this group should reflect normal sporting cycles and serve as a counterfactual for treated accounts. One challenge here is that accounts without credit cards differ in observable characteristics - on average gambling and earning less (Figures 2 & 3). To address this, we also compare treated accounts to a matched sample of this group - matching based on pre-treatment income, expenditure and gambling activity. We report results relative to both the matched and unmatched groups.
2. **2023 credit-card gamblers.** Observed over the same calendar period, they provide a proxy for what 2024 outcomes would have been absent the policy, assuming comparable seasonal betting cycles.

Each control has limitations. Transaction account funded and credit card funded gambling may differ in their sensitivities to sporting events, whilst comparisons across years may be confounded by changes in the event-calendar.[11] To address these

10 The data are a rolling panel in which accounts are observed for an average of 90 days, hence attrition becomes an issue when we extend the panel over a longer time frame. Appendix B.3 shows that our results are robust to alternative sample definitions.
11 For example, in 2023 the Ashes and FIFA Women's World Cup occurred, whereas in 2024 the T20 World Cup and the Euros were played. In Appendix B we run placebo tests to further validate the comparability of our treatment and control groups.

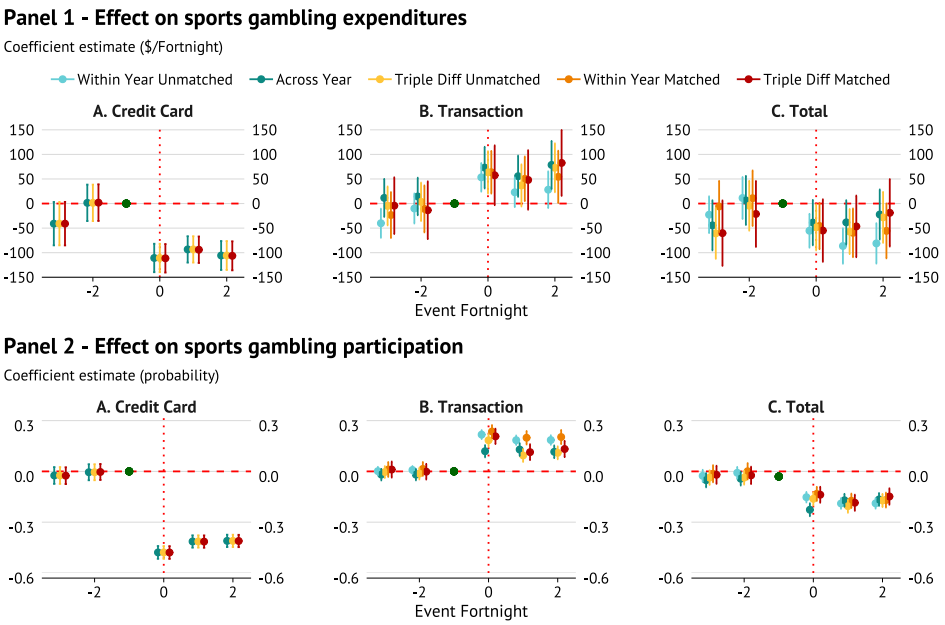
concerns, we also estimate a triple-difference specification that combines both control groups. This approach uses variation between transaction-account and credit card gamblers in 2023 to net out differences in how the two groups respond to sporting events, and variation between transaction account-gamblers in 2023 and 2024 to control for differences in the event calendar across years. By controlling for these differences, the resulting estimates more cleanly isolate the effect of the policy change.

We implement an event-study regression with individual fixed effects (see Appendix B for the specification and more details). Our primary outcome is fortnightly sports-betting spend. We also report (i) total online gambling to capture substitution across methods,[12] (ii) any participation in sports betting (i.e. the extensive margin), and (iii) expenditure on other goods and services as a measure of overall spending.[13]

Online sports gambling expenditures and probabilities fall following the ban

We find that as a result of the policy, average online sports gambling falls by around $50 per fortnight (Figure 6, Panel 1C).[14] Panel 1B shows significant substitution to transaction accounts, but the increase is not enough to fully offset the fall from credit cards (Panel 1A). These results are robust to our choice of control group. Following the policy change, treated accounts are roughly 10% less likely to sports gamble over a given fortnight (Panel 2C). Again, we see evidence of account substitution on the extensive margin, with a roughly 10% increase in the probability of gambling with a transaction account over the fortnight.[15]

Figure 6: Event-study estimates for policy response



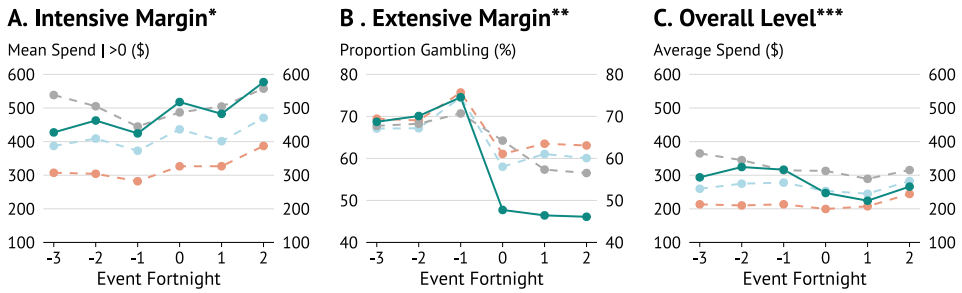
* Panel 1 shows event-study coefficient estimates where the outcome variable is sports gambling expenditure in $ per fortnight. Panel 2 shows estimates where the outcome variable is a dummy for if the individual gambles in the fortnight. In each specification the treatment group is given by individuals who gamble with their credit card. Within Year Unmatched compares the treatment group to those gambling with a transaction account in 2024. Across Year compares them to credit card gamblers in 2023. Triple Diff Unmatched is a triple difference specification comparing the treatment group to both controls.

Figure 7 shows that the average fall in spending is almost entirely driven by this participation effect. Conditional on gambling in a fortnight, there is no change to the amount gambled following the policy change. In other words, the average effect is

12 Figure B.5 reports the results of this and finds no meaningful difference to solely observing sports gambling. We cannot observe in-person cash gambling such as the use of poker machines. However, we find no meaningful change in cash withdrawals - either amounts or frequency - after the policy (Figure B.7).
13 We limit to non-durables and services, i.e. excluding durable purchases to avoid lumpy expenditure.
14 The percentage change effects of this dollar reduction are on average large due to the large proportion of 'casual' gamblers' in the treatment group who have small baseline gambling expenditures. We show this in Figures B.6 & B.8.
15 A note on Paypal and other third party intermediaries. Individuals may deposit funds into their sports betting account via PayPal. In this instance, the payment is initiated through PayPal, but the funds are drawn from their linked account. As a result, this activity appears in our data as a sports betting transaction from that account, and is therefore captured in our analysis. Note that the use of credit cards to deposit into sports betting accounts via PayPal was also prohibited under the policy. However, if an individual first deposits funds into their PayPal balance and subsequently uses those funds for gambling, we would not directly observe this activity in our data. To test whether this could be occurring, we also look at the effect of the policy on deposits into PayPal accounts around the policy implementation. We find no effect (Figure B.7).

driven by treated accounts becoming less likely to gamble following the policy change (the extensive margin), rather than reducing betting amounts for those that do gamble (the intensive margin).

Figure 7: Extensive vs intensive margin responses to the policy



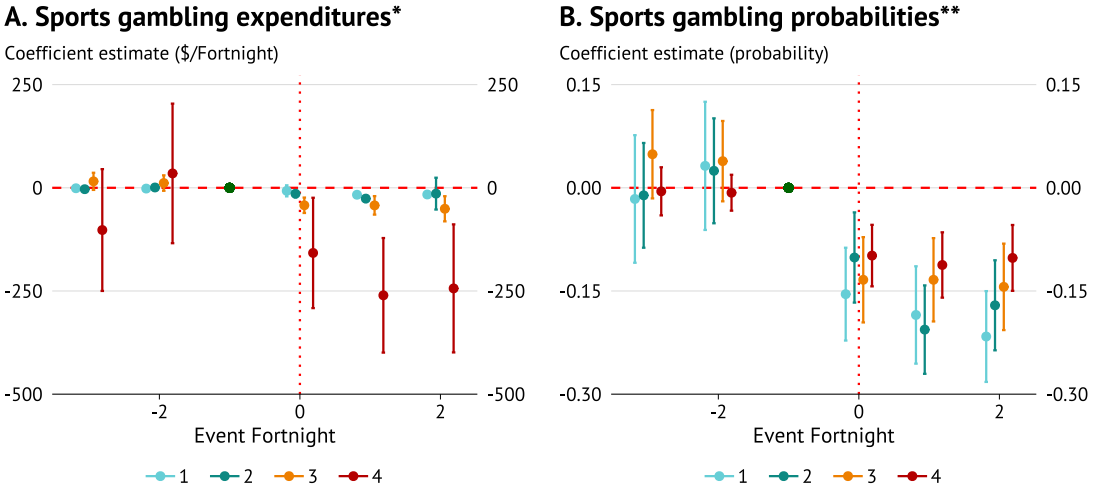
* Panel A shows average gambling expenditure per fortnight in ($), conditional on gambling in the fortnight.
** Panel B shows the proportion of group gambling in the fortnight (%)
*** Panel C shows average gambling expenditure per fortnight in ($)
**** Green shows timeseries of the treatment group. Other colours give timeseries for various specifications of the control group. Gray is 2023 credit card gamblers, orange is 2024 transaction gamblers (unmatched), and light blue is 2024 transaction gamblers (matched).

These patterns are consistent across the distribution of gambling

There is substantial heterogeneity in gambling intensity among the treated group prior to the policy. Average fortnightly gambling expenditure ranged from $10 in the lowest quartile to approximately $1,000 in the highest quartile. Since the policy was likely aimed at larger or more problematic gamblers, it is natural to ask whether impacts varied across the distribution.

We find broadly consistent effects. All quartiles show a decline in participation on the *extensive* margin. This participation effect is larger for those more casually attached to the market (quartiles 1 and 2) and smaller for larger spenders (quartile 4). The dollar reductions in gambling are mechanically larger in the fourth quartile given higher baseline spending.[16] Across each quartile, the average effects of the policy are driven primarily by the extensive margin effect of lower participation (Figure 8, Panel A). These consistent effects suggest that the policy was effective at reducing some activity from heavy gamblers. In the Appendix (Figures B.10-B.15) we also show broadly consistent effects of the policy when cutting the sample by income, liquidity, and credit card debt.

Figure 8: Effect of policy across distribution of gambling



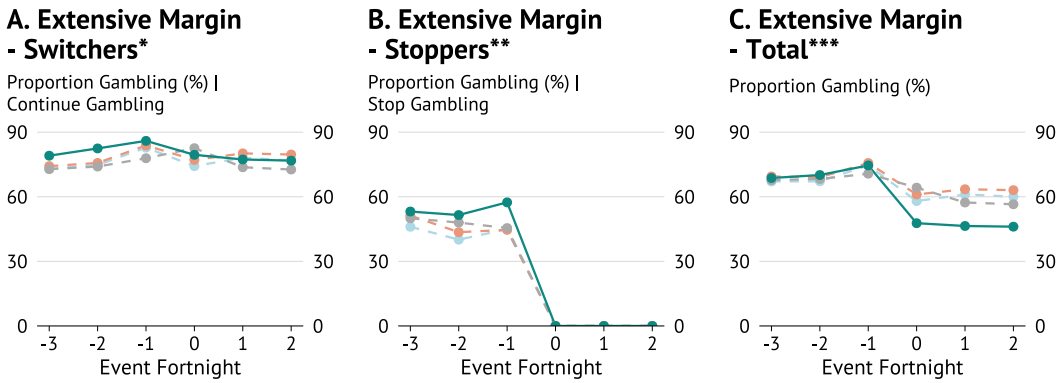
* Panel A shows event-study estimates by pre-policy gambling amount quartiles where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates by pre-policy gambling amount quartiles where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** Both panels compare the treatment group to 2024 transaction account sports gamblers (the within year unmatched specification). Quartile 1 are those with the lowest pre-period gambling amounts whilst quartile 4 are those with the highest pre-period gambling amounts

16 (Figure B.8) shows that the fourth quartile has the smallest percentage reduction in gambling.

There is no apparent correlation between gambling reductions and financial constraints

We further decompose the participation effect by categorising responses as stoppers (cease gambling entirely following the ban) and switchers (observed gambling post-policy change with their transaction account). Figure 9 shows that the overall reduction in participation is primarily driven by stoppers - the set of accounts that entirely stop gambling.

Figure 9: Decomposition of participation effects



* Figure A shows the extensive margin effect conditional on being a switcher - i.e. the proportion of the group gambling each fortnight conditional on observing gambling post policy change
** Figure B shows the extensive margin effect for stoppers.
*** Figure C shows the total extensive margin effect - given by the proportion of the overall group gambling in the fortnight (%)
**** Green shows timeseries of the treatment group. Other colours give timeseries for various specifications of the control group. Gray is 2023 credit card gamblers, orange is 2024 transaction gamblers (unmatched), and light blue is 2024 transaction gamblers (matched).

This set of 'stoppers' comprises approximately 38% of the overall treatment group. If the observed effect reflects only a short-run friction, e.g. delays in switching to transaction accounts, or a temporary "nudge" for those already inclined to quit, gambling could return to baseline once these frictions diminish or self-control problems resurface. Our design only allows observation for six weeks post-policy, so we cannot test persistence. Nonetheless, given that 60% of the group of stoppers gamble in given fortnight pre-policy, their cessation for the entire post-period suggests a meaningful medium-run effect, at a minimum.

The policy's effect in reducing participation could be driven by inconvenience (the friction of eliminating one means of payment) or by banning the use of debt to fund gambling. To test which mechanism appears most likely, we test how the probability of being a stopper varies with pre-policy characteristics, and also compare how the financial outcomes of stoppers evolve post-policy in comparison to the rest of the treatment group.

We find that there is no statistically significant relationship between pre-period liquidity constraints or revolving debt and the probability of being a stopper. Additionally, we find no significant differences between groups on average in post-policy financial outcomes (Figure B.16). We do, however, find a reduction in credit card debt and an increase in transaction account balances among those who both stop gambling and were quartile-4 gamblers pre-policy. This group is small and noisy however, and these effects are only marginally significant (Figure B.17).

Two factors do correlate with the probability of stopping rather than switching: (i) the number of accounts used for gambling pre-policy and (ii) pre-policy attachment to the market. Those who gamble with both a transaction account and a credit card prior to the policy change are less likely to stop than those who gamble with only a credit card. Likewise, accounts more securely attached to the market pre-policy (in quartile 4 of the gambling distribution) are less likely to stop than those more casually attached (in quartile 1; Table B.1). These patterns suggest the treatment effect we identified is not through constraining gamblers' liquidity, but instead creating a convenience friction.

The response of cash-advance fees supports a story of convenience frictions

An interesting quirk of the policy is that it does not truly constrain the liquidity of credit card gamblers, as a simple workaround exists. It is possible to transfer funds from a credit card to a transaction account and then deposit into betting accounts. While this process incurs a cash advance fee - typically a fixed amount or a small percentage - gambling with a credit card was already treated as a cash advance prior to the policy, so the financial cost is unchanged. The main effect of the policy, therefore, was to

introduce additional frictions. In order to continue gambling, one would have to register a new card or depositing mechanism with their betting provider, and initiate a transfer from credit to debit.

If those gambling with their credit card pre-policy were truly liquidity constrained, and using their credit card to extend their constraint, we might expect to see this workaround being used. If they had, cash-advance fees should have remained steady post-policy. Instead, we find that cash-advance fees declined by around $4 per fortnight (Appendix B.18), suggesting treated accounts did not systematically shift to this channel. In an unexpected way, the policy may have improved financial outcomes by saving members of this group a few dollars per week in avoided fees. However, it is worth noting that our results suggest that the majority of gamblers using a credit card could have funded their gambling expenditures through the liquid balance in their transaction account. Given this, it is puzzling that they would choose to use their credit card and incur these not insignificant fees. It is possible that they preferred the convenience of using their credit card and were willing to pay for this. The welfare implications of these reduced fees are therefore complicated by this observed pre-policy behaviour.

On average the policy does not affect other expenditure or account balances

If self-control problems were driving gambling in this cohort, one possibility is that expenditures on gambling crowded out spending on other goods and services or reduced liquid savings and account balances. If so, reduced gambling expenditure would increase non-gambling expenditure or improve net financial positions.

We observe no rise in expenditure in the month after the ban (Figure B.19). Precision is limited by sample size, and the average policy effect itself was modest (about $50 per fortnight, depending on specification). Even if fully redirected, such amounts are unlikely to produce detectable changes given normal week-to-week volatility in spending.

Similarly, we find little effect on transaction account balances—whether minimum, mean, or maximum values (Figure B.20). Credit card debt declines gradually in the three fortnight post-ban (about $500 on average), concentrated in the right tail, but this result is imprecise and not statistically significant.

Overall, we cannot conclude that the policy materially affected expenditure, savings, or debt in the short run. Mechanically, reduced gambling must translate into either higher savings or other spending, absent income changes. Yet the effect size is too small to generate detectable improvements in financial well-being within six weeks, including among large gamblers or those with larger amounts of credit card debt. Longer-run accumulation of gambling savings remains possible but lies beyond our horizon.

3. Policy considerations

Our findings provide novel empirical evidence on the relationship between gambling and credit card use in Australia. We show that banning credit card use for online gambling produced a modest reduction in gambling, driven by reduced participation.

The mechanism appears to be inconvenience rather than liquidity. The policy did not prevent "gambling with money you don't have," since equivalent credit-financed gambling was possible through transfers. Instead, the ban created frictions - such as registering a new card or making transfers between accounts - that appear to have prompted some to stop gambling. Why these frictions were effective is unclear. Possible explanations include:

1. Some may already have been looking for a reason to quit and the policy nudged them to do so;
2. People lacked debit cards to deposit in a sports betting site; or
3. There were idiosyncratic circumstances, such as the person was using their credit cards to conceal gambling from partners or meaningfully preferred their security for online purchases.

Beyond these specific channels, it is also possible that seemingly trivial frictions are behaviourally important in their own right. Indeed, that mild convenience frictions can reduce gambling is consistent with other Australian evidence, for example, a ban on smoking in gaming areas of clubs and hotels resulted in a decline in poker machine expenditure across several states (Productivity Commission, 2010).

Our results are estimated over a short horizon of six weeks post-ban, and it is possible that some resumed gambling once these frictions were resolved. We cannot rule out that the policy had important benefits for a small group who would otherwise

have accumulated substantial credit card debt, which the Parliamentary Joint Committee who recommended the ban were most concerned about.[17] At the same time, credit card use was already a declining and small share of the online gambling market, the median affected account in our sample gambled only $14 per week, and the participation effects of the policy disproportionately affected the more casual component of the market. The policy therefore imposed inconvenience on many low or moderate levels of gambling while only directly targeting a small subset of the market.

Overall, our results show that gambling behaviour is responsive to policy. That reductions were driven by frictions suggests that impulse control problems play an important role and can be shaped by government intervention. Yet survey evidence consistently shows that most gambling harm in Australia is driven by poker machines (GambleAware NSW, 2024; Maltman, 2025; Rockloff et al., 2025). Policymakers seeking to reduce harm may achieve greater impact by focusing on that sector.

References

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17 For example, the Parliamentary Joint Committee notes "while the number of people suffering substantial harm from the use of credit to gamble online may be comparatively small, the consequences are significant and life changing." (Parliamentary Joint Committee on Corporations and Financial Services, 2021, Page 40)

A.1. Overview of the transaction data

The analysis in this research note is based on consented, de-identified and aggregated bank transaction data.The data provides a backwards looking window of financial history (typically 90 days of transactions across relevant accounts). The data provider cleans and aggregates the data: categorising transactions, constructing demographic variables and removing personally identifiable information.

The final sample used includes accounts reporting transactions over a 20 week period from the 4th of April 2024 to the 14th of August in both 2023 and 2024. We restrict this sample in three ways for our analysis.

First, we only include aggregated accounts that meet the following conditions:

- Have at least one inflow categorised as wages per month over the period they are observed.
- Have at least five expenditure outflows per month over the period they are observed.
- Gamble at least once in the period before June 11 (the date of the ban in 2024).[18]

Second, we classify accounts based on whether they hold a credit card. To ensure accurate classification, we exclude accounts without an observed credit card but with evidence of credit card use - for example, repayments made from the transaction account.

Third, we split our sample into two groups based on activity prior to 11 June. Group one ("credit card gamblers") are those that held a credit card and report sports gambling transactions from that card, while group two ("transaction account gamblers") are those without a credit card that reported sports gambling transactions from their transaction account.

The final sample consists of a set of transaction gambling accounts, and a set of credit card gambling accounts. For this sample we use the following variables in our analysis:

- Gambling and sports gambling transactions.
- Spending
- Income
- Account balances

A.2. Representativeness of the sample

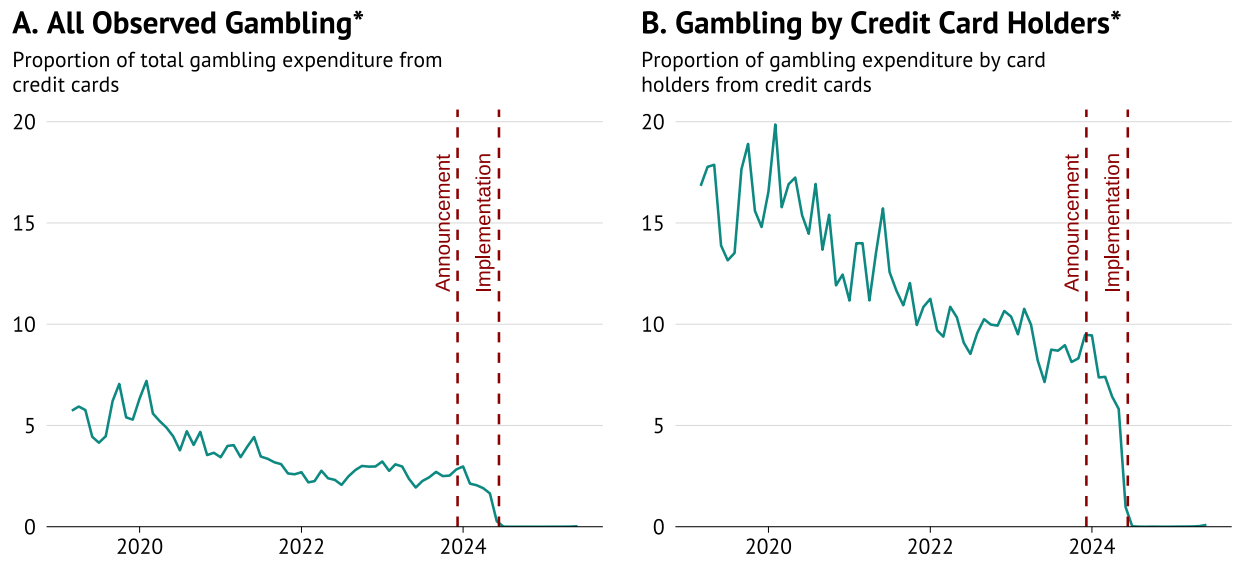
As with any sample, the transaction data may be a biased sample with which to assess the relationship between credit card use and gambling. Specifically, accounts in the data tend to be younger and may be more liquidity constrained than the general population. It is not clear what direction this bias may act in. On the one hand, more problematic gambling behaviour may be concentrated amongst more liquidity constrained people. On the other, those with the most problematic gambling habits may not appear in the sample. Other research suggests that the transaction data co-moves closely with official statistics on aggregate consumption, mitigating concerns of bias in the sample.

A lack of available data in this area makes it challenging to benchmark the patterns we see in the transactions to public sources of information. In a submission to the Australian Banking Association's 2020 consultation report (Australian Banking Association, 2020), Responsible Wagering Australia reported that in the late 2010s "approximately 20% of deposits into wagering accounts are transacted by credit card". In the 2021 Parliamentary Joint Committee Inquiry, TAB's CEO reported that 14% of TAB account deposits are credit cards, down from 15.7% in the previous year (Parliamentary Joint Committee on Corporations and Financial Services, 2021).

18 We identify sports gambling based on the categorisation of transactions from the data-provider as well as the third party reported on the transaction.

In the transaction data, we find a lower proportion of sports gambling deposits from a credit card account (Panel 1, Figure A.1). However there are a number of reasons to believe that the two sets of numbers are consistent. First, the use of credit cards in general, and for gambling has fallen over time. As such, credit cards are likely to have been a larger part of the market in the later 2010s than in the period of our study. Second, the rate of credit card holding in the transaction data is lower than in the general population. The share of sports gambling deposits from credit card accounts, conditional on an individual having a credit card is closer to the industry estimates above (Panel 2, Figure A.1).

Figure A.1: Size of the credit card market



* Figure A plots the proportion of all observed sports gambling expenditures coming from a credit card in each month
** Figure B plots the proportion of sports gambling expenditures from credit card holders coming from a credit card account in each month

As a final point, the lower rate of card-holding in our sample relative to the population is not a threat to the internal validity of our study. Provided that the credit card holders we observe are not systematically different to unobserved credit card holders, then the policy response we observe is representative of the policy response across the Australian population.

B.1. Empirical approach

In this analysis we are interested in estimating the response of gambling to the policy change. To estimate this response we employ an event-study approach with a difference-in-differences design.

(1) $$y_{it} = \alpha_i + \gamma_t + \sum_t \left(\beta_t \times Treat_i \times W_t\right) + \epsilon_{it}$$

where

- y_{it} is the outcome variable for individual *i* in period *t*. We consider a range of outcome variables. Our baseline specifications are sports gambling expenditures in dollars and a dummy for sports gambling participation. We separately estimate the event-study based on each account type and aggregated over accounts. We also estimate specifications with other forms of spending, fees, and interest charges, and account balances as outcome variables.
- α_i is an individual fixed-effect.
- γ_t is a period *t* fixed effect.
- $Treat_i$ is the treatment dummy - taking value 1 if individual *i* is in the treatment group and 0 otherwise.
- W_t is a period *t* dummy.
- β_t gives the treatment effect of the policy in period *t*.

In each specification, the treatment group consists of accounts who use their credit card to sports gamble in the period preceding the policy change in 2024. We construct two distinct control groups.

1. Non-credit card holders who gamble with a transaction account in the period prior to the policy change.
2. 2023 credit card holders who use their credit card to sports gamble in the weeks prior to the policy change date (but in 2023).

To get an unbiased estimate of the policy's effect we must ensure that the control group represents a valid counterfactual for the outcomes of treated units in the absence of the policy. There are credible reasons to believe each control group represents such a valid counterfactual.

The first group - 2024 transaction gamblers - are untreated by the policy but subject to the same time trends in sporting cycles over the post-policy period. As such, their total sports gambling provides a counterfactual for how credit card gamblers would have acted without the policy. One issue here is that this control group differs in observable characteristics relative to the treatment group, on average gambling more and earning less. To address this, we also compare the treatment group to a matched sample of this control group. Specifically, we pair each treated individual with a transaction-account gambler with similar pre-treatment income, expenditure, and gambling activity. Even post matching there is a possible confounder to this specification - transaction account gamblers may differ in their unobservable sensitivity to the sporting cycle.

As a result we consider a second group - 2023 credit card gamblers. This group is much closer to the treatment group on observable characteristics, and is not subject to the confounder above. Taken in the same calendar period in 2023, the sports gambling of this group provides an alternative counterfactual for credit card gamblers the year after. An extra benefit of this specification is that it provides a counterfactual for gambling expenditure from a credit card account as well as total expenditure. The drawback of this control group is that over different years the two groups will be exposed to different sporting cycles.

To account for the possible confounders to both groups, we estimate a triple-difference event-study which combines both controls:

(2) $$y_{it} = \alpha_i + \gamma_t + \sum_t(\rho_t \times 2024_i \times W_t) + \sum_t(\delta_t \times Card_i \times W_t) + \sum_t(\beta_t \times 2024_i \times Card_i \times W_t) + \epsilon_{it}$$

where 2024_i is a dummy taking value 1 if individual *i* is observed in 2024, and 0 if they are observed in 2023, and $Card_i$ is a dummy taking value 1 if individual *i* is a credit card gambler, and 0 otherwise. Under the triple difference specification:

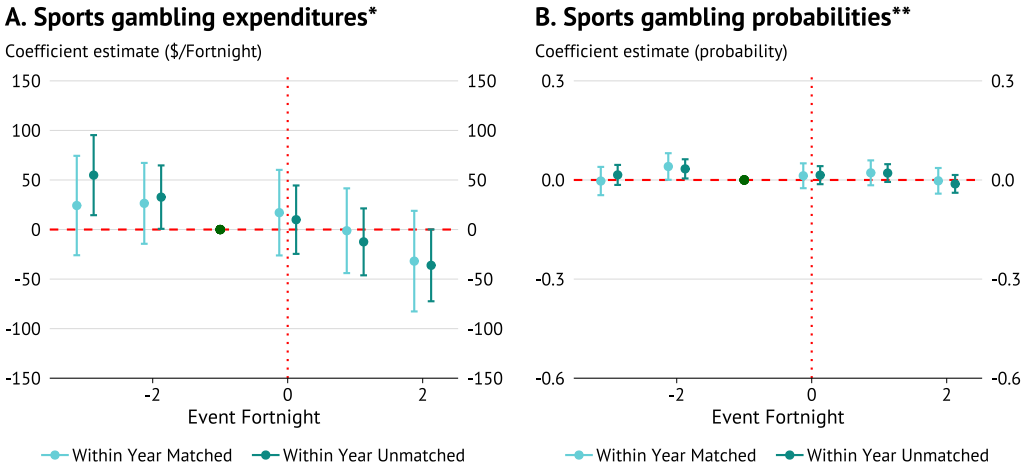
- ρ_t captures the difference in outcomes for transaction-account gamblers in 2024 vs 2023. This controls for differences in unobserved time-trends across each year.
- δ_t captures the difference in outcomes for credit card gamblers vs transaction account gamblers in 2023. This controls for differences in the sensitivity to sporting cycles across groups.
- β_t is the triple-difference estimator for the policy's treatment effect. It gives the effect of the policy on credit-card gamblers in 2024, whilst controlling for both of the unobserved confounders above.

B.2. Placebo tests

The central identifying assumption for our treatment effect estimates is parallel trends - in the absence of the policy, the treatment and control units should change in the same way. We test the credibility of this assumption via a set of placebo tests.

First, we test whether 2024 transaction account gamblers are an appropriate counterfactual for the treatment group in 2024 (i.e. testing the assumption behind the within year specifications of our baseline event-studies). The key threat to this is if the two groups differ in their sensitivity to unobserved sporting events. We test if this is the case by comparing 2023 credit card gamblers to 2023 transaction account gamblers. Figure B.1 shows no statistically significant differences between these two groups around the placebo treatment date, validating this first choice of counterfactual.

Figure B.1: Placebo test 1 - 2023 credit card gamblers vs 2023 transaction gamblers

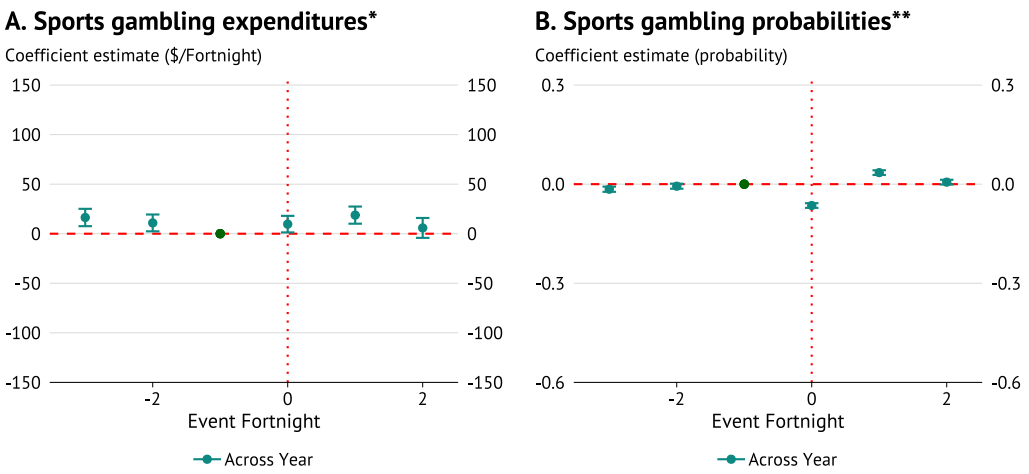


* Panel A shows event-study estimates where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** The placebo treatment group here are 2023 credit card gamblers. The placebo control group are 2023 transaction gamblers (matched and unmatched). Both groups are untreated by the actual policy. The placebo tests the parallel trends assumption used for the within year specifications of the baseline event-studies

Second, we test whether 2023 credit card gamblers form a valid counterfactual for the treatment group in 2024 (i.e. testing the assumption behind the across year specification of our baseline event-studies). The threat to this is that the two groups are exposed to different sporting calendars. To test if this drives our estimated treatment effects, we compare 2024 transaction account gamblers to 2023 transaction account gamblers. Figure B.2 shows no significant differences across these groups, suggesting that different sporting events is unlikely to be driving differences in outcomes across years.

It is worth noting that if there were significant differences in the placebos they would be accounted for in the triple difference design. For instance, suppose that gambling post the policy change date is lower in 2024 than 2023 due to exogenous sporting events. This would bias our across year specification treatment effects downwards. However the triple difference estimates would control for these differences across years provided that we see them in our placebo groups (transaction account gamblers). The fact that the triple difference estimates in Figure 6 are similar to the other specifications implies that the placebo tests should be insignificant, as seen here.

Figure B.2: Placebo test 2 - 2024 transaction vs 2023 transaction gamblers



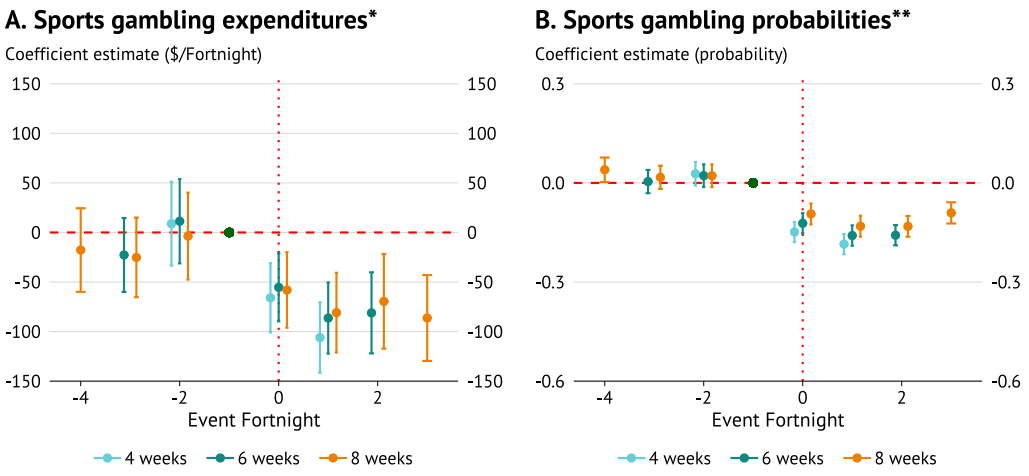
* Panel A shows event-study estimates where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** The placebo treatment group here are 2024 transaction account gamblers. The placebo control group are 2023 transaction gamblers. Both groups are untreated by the actual policy. The placebo tests the parallel trends assumption used for the across year specifications of the baseline event-studies

B.3. Sample choice

Our baseline results take a balanced sample of accounts observed for six weeks before and after the policy change. Our key results are qualitatively unchanged when constructing alternative samples of shorter and longer length. There are two counteracting effects on sample size when increasing the panel length. First, there is attrition out of the sample from people who are not observed for the entirety of the longer period. Second, because we define our treatment and control groups based on pre-policy gambling, a longer pre-period includes more individuals (for instance those who gamble in period $t - 1$ but not in periods t to 0. On average, we observe accounts for a period of 90 days. As such, once we go past the 12 week window the attrition effect dominates and the treatment group becomes relatively small in size.

In Figure B.3 we re-estimate the event-study for total sports-gambling expenditure and participation by different sample lengths. We consider balanced samples of 4 weeks, 6 weeks (the baseline), and 8 weeks before and after the policy. Our baseline results are unchanged under these alternative sample definitions.

Figure B.3: Event-study estimates by alternative sample definitions



* Panel A shows event-study estimates where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** $ weeks uses a balanced panel of 4 weeks before and after the policy change. 6 weeks uses a balanced panel of 6 weeks before and after the policy change - the baseline. 8 weeks uses a balanced panel of 8 weeks before and after the policy change. All reported event-studies use the Within Year Unmatched specification - comparing 2024 credit card gamblers to 2024 transaction account gamblers.

B.4. Additional results

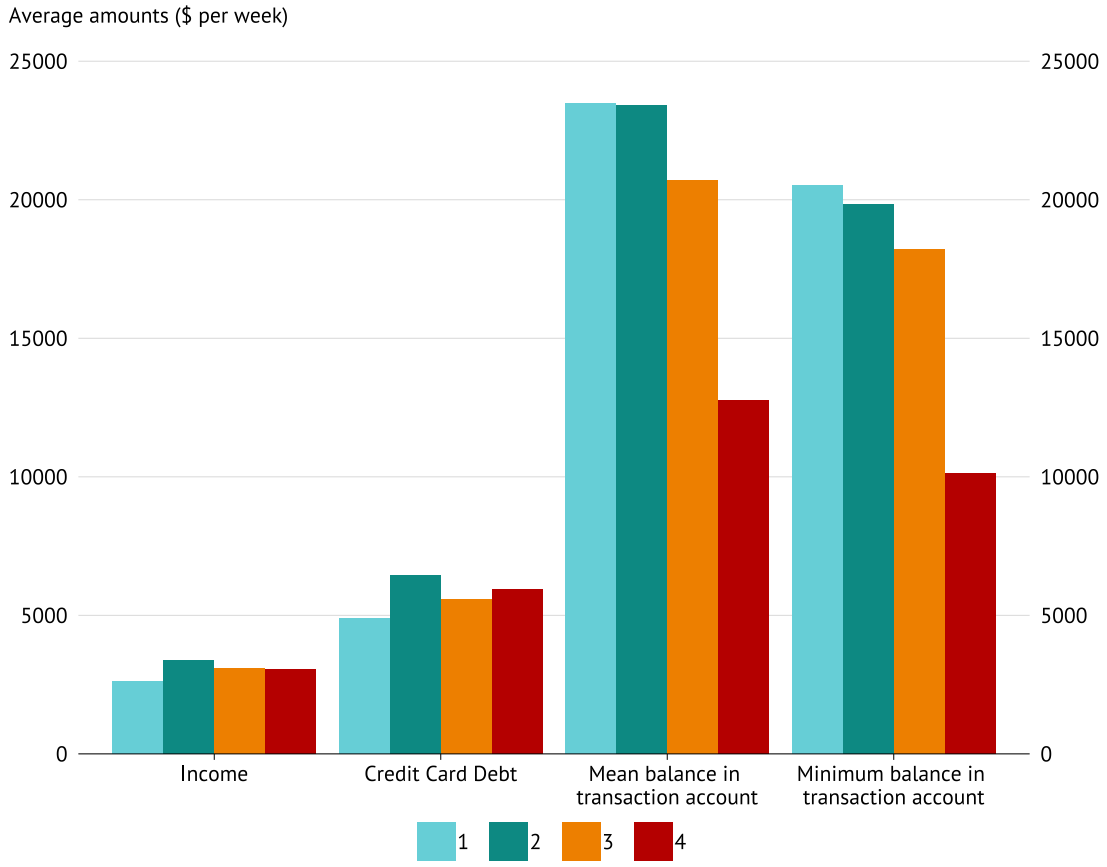
In this section we provide a set of additional results that support points made in the main body of the research note.

- Figure B.4 shows average levels of income, credit card debt and liquid wealth by quartiles of the sports gambling distribution. The sample consists of those who use a credit card to gamble. There is no clear relationship with gambling quartiles and income, nor credit card debt. On average, those who gamble more have lower levels of liquid wealth. As shown in Figure 8, gambling in the right tail is mainly driven by the transaction account and so it's not clear that credit card usage is driving this relationship.
- Figure B.5 estimates the average effect of the policy on total online gambling (not just sports gambling). This includes sports betting, lotteries, and online casino type wagering. The event-study estimates here are similar to the baseline results for sports betting. There is a mechanical reduction in gambling from a credit card account. We see evidence of reallocation towards transaction accounts, and total online gambling falls. The fall in credit card gambling here is similar to our baseline results. This provides suggestive evidence that credit card holders are not substituting towards other forms of gambling not subject to the ban (such as lotteries). The reduction in expenditures is matched by a reduction in participation.
- Figure B.6 estimates the policy effect on sports gambling in percentage change terms. The outcome variable y_{it} in this specification is the percentage change in individual i's period t outcome, relative to their mean outcome in the pre-policy period. The average percentage change effect of the policy is large in magnitude - driven by the relatively large proportion of casual gamblers in the treatment group who have small baseline sports gambling expenditures.
- It is possible that credit card holders substitute towards other mechanisms of funding gambling. This could include cash withdrawals or deposits into paypal accounts, from their credit card. Both could in turn be used for gambling. Figure B.7 estimates the average effect of the policy on PayPal deposits and cash withdrawals from credit card accounts. The figure shows no significant increase in the amounts withdrawn in cash / deposited in a Paypal account, nor in the probability of doing so. This suggests that on average there is no substitution to using other mechanisms with a credit card to fund gambling.
- Figure B.8 estimates heterogeneity in the treatment effect based on pre-policy gambling amounts in percentage change terms. The outcome variable y_{it} in this specification is the percentage change in individual i's period t outcome, relative to their mean outcome in the pre-policy period. The figure shows that the percentage reduction in gamblers is largest for the most casual gamblers - due to the small pre-period baseline amongst this group.
- Figure B.9 decomposes the extensive and intensive margin effects of the policy for the first and fourth quartile of the pre-policy gambling expenditure distribution. Across both quartiles, the average response is driven by the extensive margin (participation effect). This participation effect is visibly larger for the first quartile of gamblers (the more casual segment of the market).
- Figures B.10, B.11, B.12, B.13, B.14, and B.15 estimate heterogeneity in the response to the policy by measures of income, average liquidity, proxies for liquidity constraints, outstanding credit card debt, and a measure of if an individual revolves their credit card debt. There is limited heterogeneity in the response to the policy across each of these dimensions.
- Table B.1 shows correlations between the probability of stopping and pre-policy observable characteristics. When controlling for pre-period gambling spending and number of accounts used, there is no significant relationship between liquidity constraints, debt revolving or account balances, and the probability of stopping. On the other hand, using both accounts pre-policy is associated with a 40% lower probability of stopping, whilst being in the 4th quartile of sports gambling spending is associated with a 20% lower probability of stopping than being in the 1st quartile.
- In Figure B.16 we estimate heterogeneity in post-policy financial outcomes based on stopping vs switching. Panel A shows the clear difference in post policy gambling deposits across these groups. Panels B-D show no obvious differences in the average level of outstanding credit card debt, the average balance in the transaction account, nor the average minimum balance in the transaction account between these groups post policy.
- To account for the fact that the largest effects are likely to have been seen amongst large gamblers, we re-estimate the specification above but only for treated individuals who were in the fourth quartile of pre-policy gambling spending. The results from this are shown in Figure B.17. Panel A again shows the clear difference in post-policy gambling deposits for stoppers vs switchers. Panel B suggests that on average levels of credit card debt decreased for stoppers post policy, whilst

remaining unchanged for switchers. Panels C and D suggest that liquidity of stoppers may have increased more than for switchers. However all of these estimates have large standard errors, so we do not wish to interpret them too strongly.

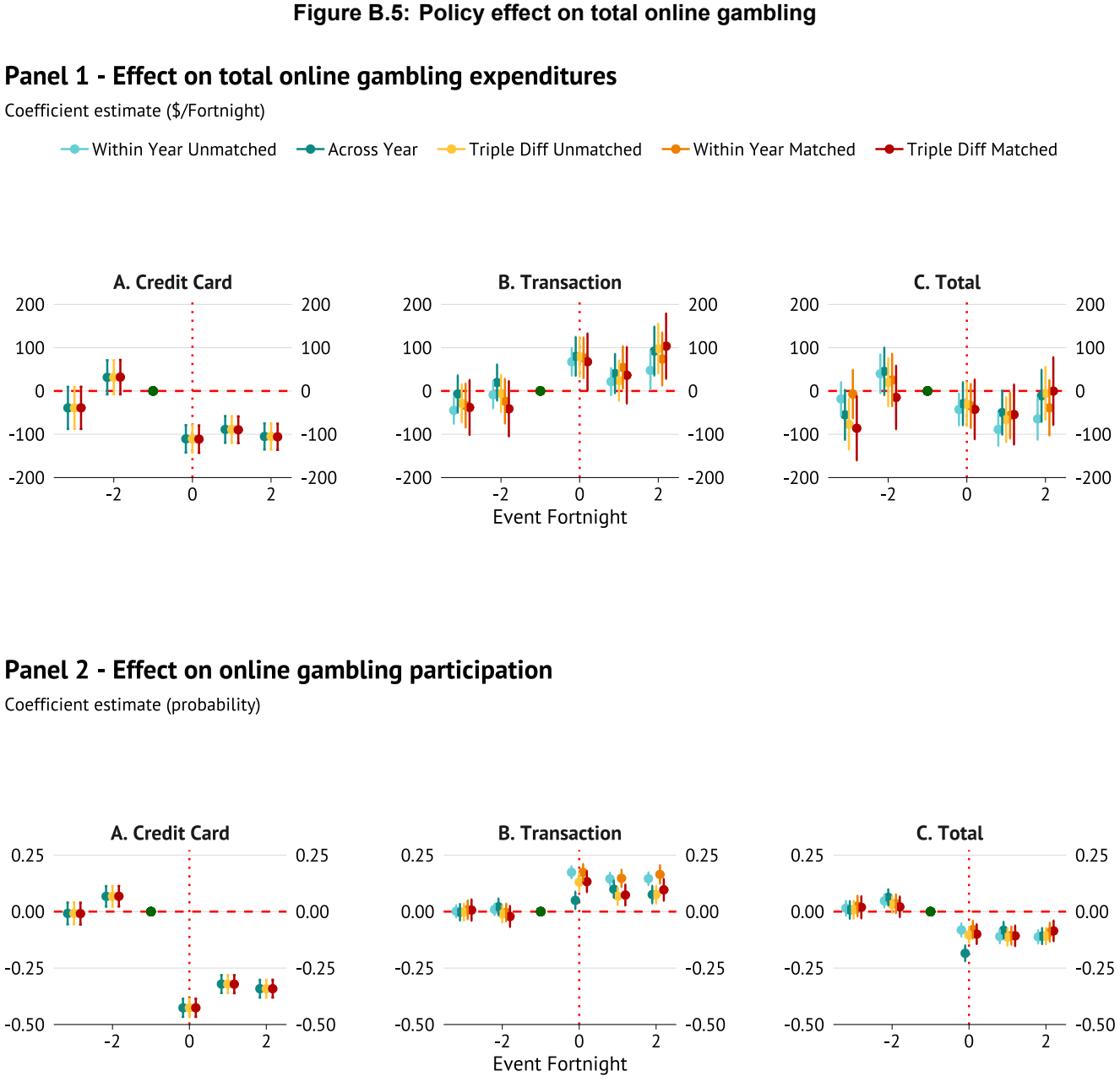
- Figure B.18 estimates the average effect of the policy on cash-advance fees. On average, cash advance fees fall by around $4 per fortnight, following the policy change, and those in the treated group become around 30% less likely to incur a cash advance fee in a given fortnight.
- Figures B.19 and B.20 estimate the average effect of the policy on services and non-durables spending, as well as on account balances. The event-studies show no significant effects on either of these variables, however as we note in the body, the estimates are noisy.

Figure B.4: Incomes, debt and liquid wealth across credit card gambling distribution



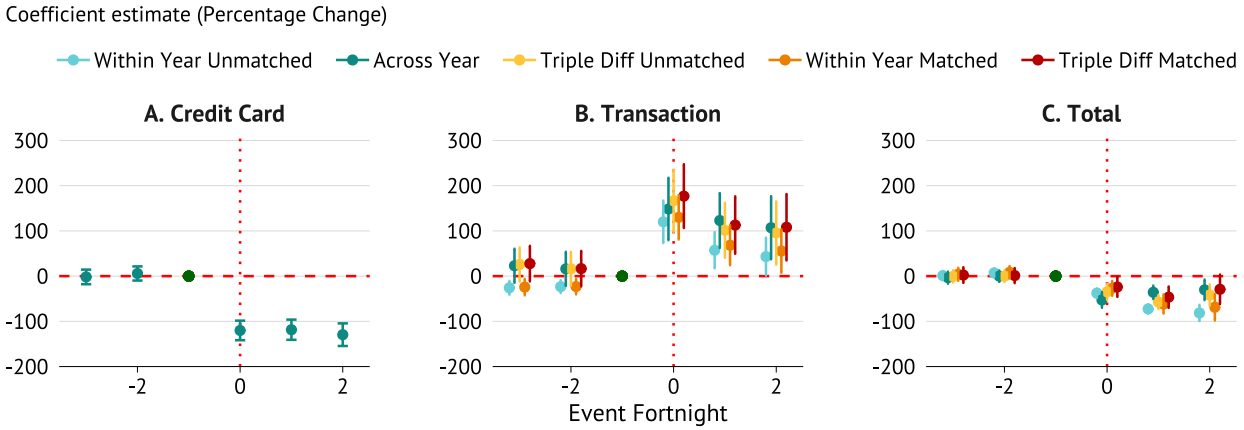
* Figure shows average levels of income, credit card debt, and liquid wealth across the distribution of pre-period gambling amounts. The sample are credit card gamblers. Two measures of liquid wealth are provided. The first is the mean balance in the transaction account during the week. The second is the average minimum balance. Colours give quartiles of sports gambling amongst credit card gamblers. Quartile 1 are those who gamble the least, quartile 4 gamble the most.

Figure B.5: Policy effect on total online gambling



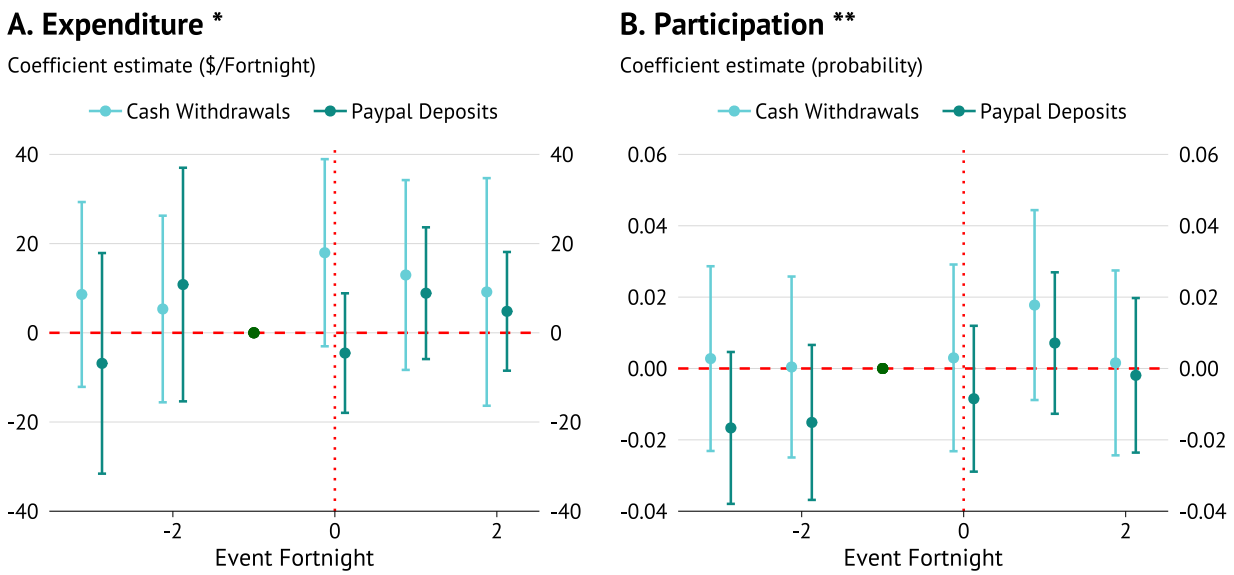
* Panel 1 shows event-study coefficient estimates where the outcome variable is total online gambling expenditure in $ per fortnight. Panel 2 shows estimates where the outcome variable is a dummy for if the individual gambles in the fortnight. In each specification the treatment group is given by individuals who gamble with their credit card. Within Year Unmatched compares the treatment group to those gambling with a transaction account in 2024. Across Year compares them to credit card gamblers in 2023. Triple Diff Unmatched is a triple difference specification comparing the treatment group to both controls.

Figure B.6: Event-study estimates in percentage changes



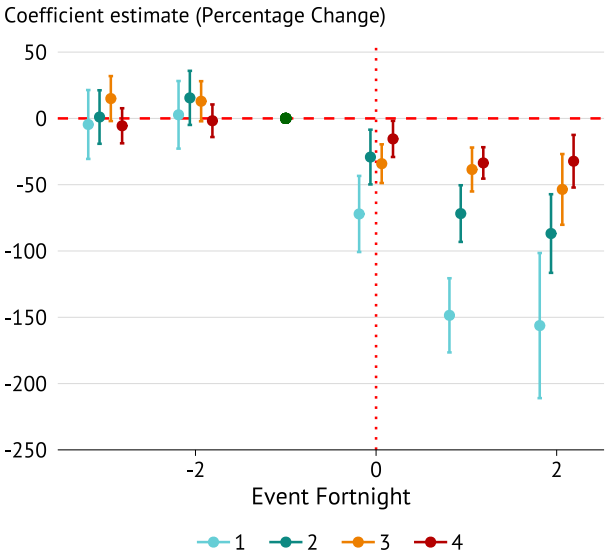
* Figure shows event-study estimates where the outcome variable is the percentage change relative to the mean in the pre-period. Panel A shows credit card deposits, Panel B shows deposits from a transaction account, and Panel C shows total deposits, aggregated across accounts. The coefficient estimates here represent the average effect across the treatment group. The average percentage changes are large in magnitude due to the large proportion of casual gamblers in the group for whom the baseline is small in magnitude. Colours show alternative specifications.

Figure B.7: Policy effect on cash withdrawals and paypal deposits from credit card accounts



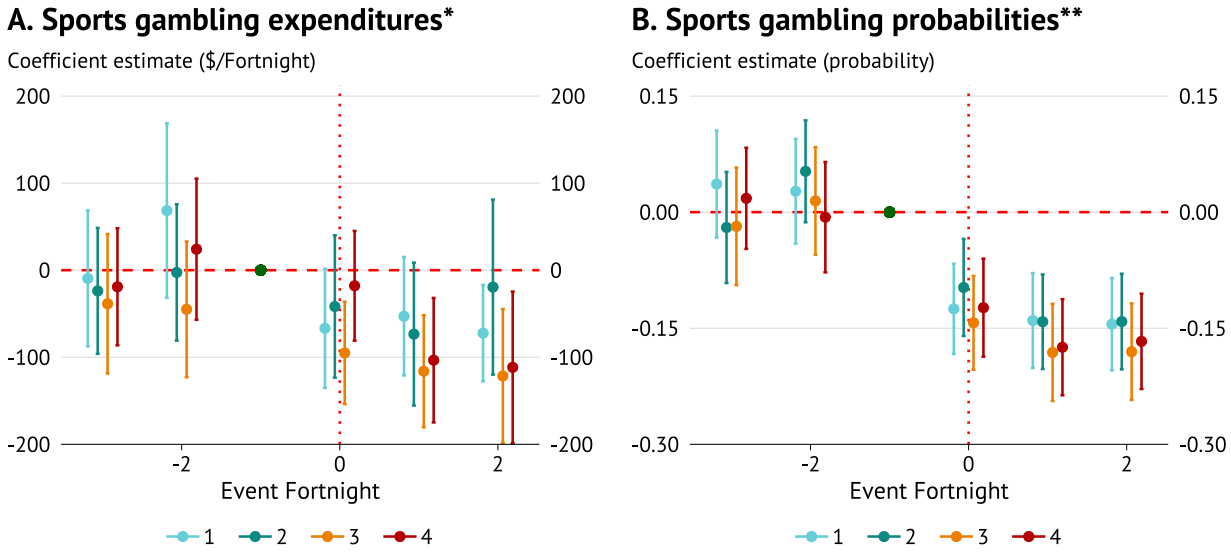
* Panel A has outcome variables for fortnightly cash withdrawals and paypal deposits in $ from credit card accounts
** Panel B gives the probability of a cash withdrawal or paypal deposit from a credit card account in the fortnight
*** All event-studies are estimated using the across year specification, meaning that the control group are 2023 credit card gamblers

Figure B.8: Effect of policy across distribution of gamblers - percentage changes



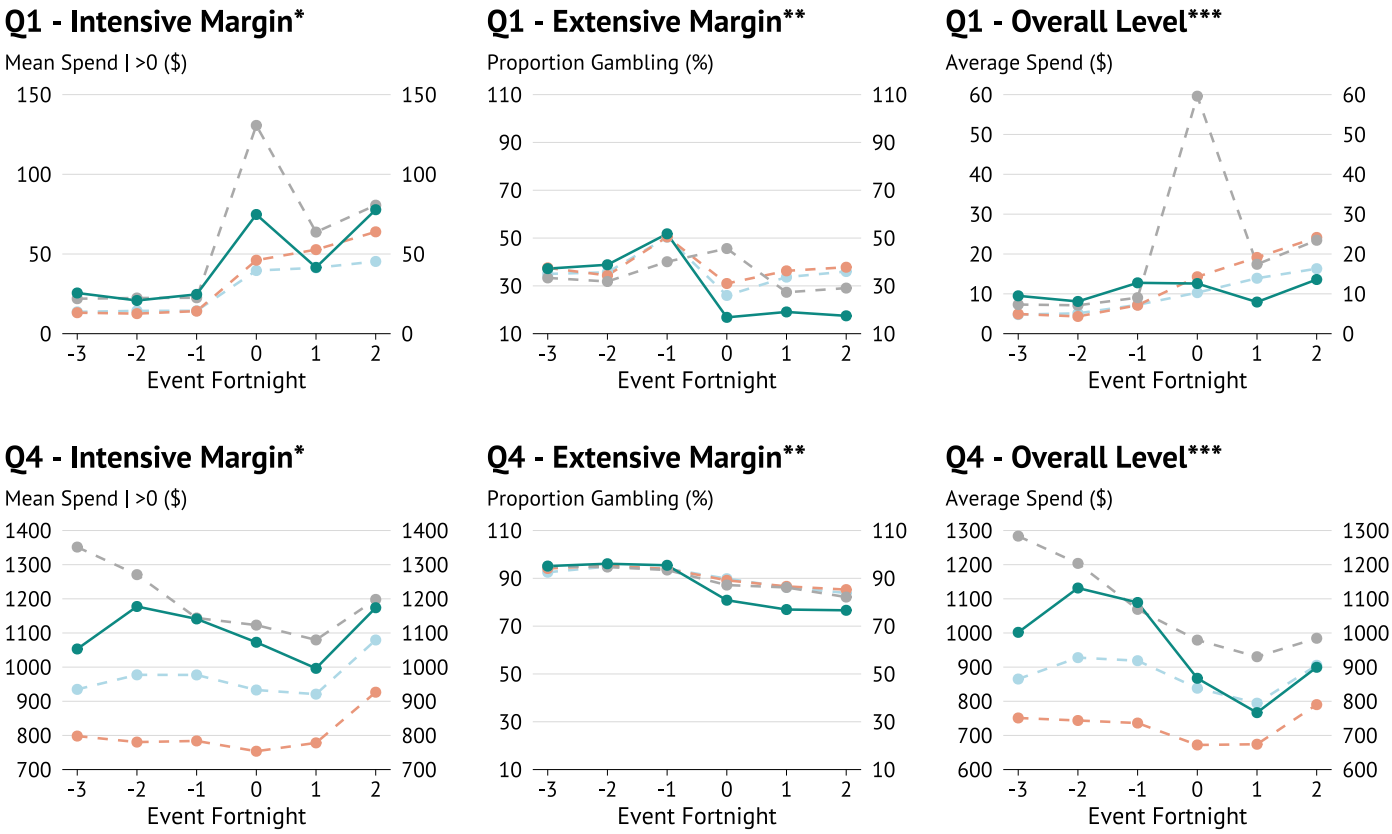
* Figure shows event-study estimates by pre-policy gambling quartiles where the outcome variable is the percentage change in the fortnights total sports gambling expenditure relative to the pre-period mean. The event-study uses the within year unmatched specification - meaning the control group are unmatched 2024 transaction gamblers. Quartile 1 consists of those gambling the least pre-policy, whilst quartile 4 consists of those gambling the most.

Figure B.10: Heterogeneity in policy effects by income



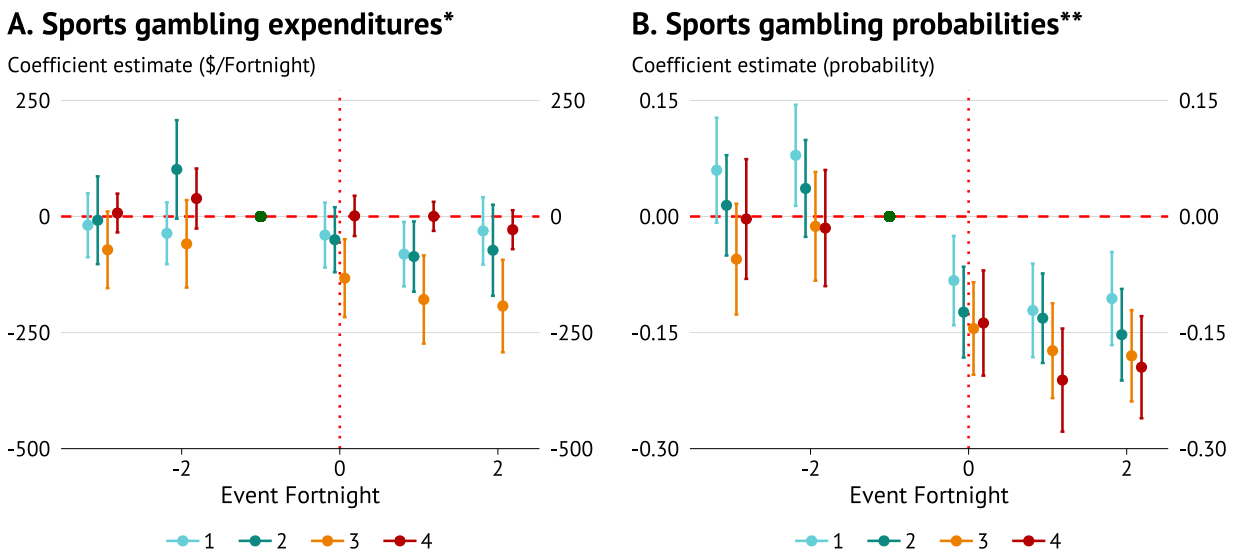
* Panel A shows event-study estimates by pre-policy income quartiles where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates by pre-policy income quartiles where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** Both panels compare the treatment group to 2024 transaction account sports gamblers (the within year unmatched specification). Quartile 1 are those with the lowest pre-period incomes whilst quartile 4 are those with the highest pre-period incomes.

Figure B.9: Extensive vs intensive margin response to policy across distribution of gamblers



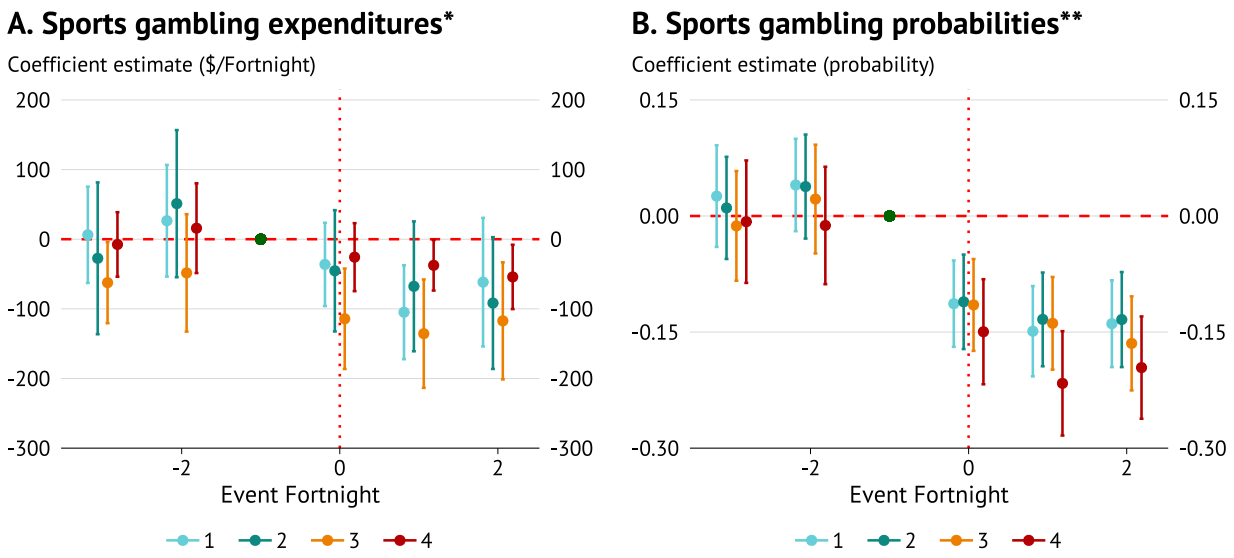
* Panel A shows average gambling expenditure per fortnight in ($), conditional on gambling in the fortnight.
** Panel B shows the proportion of group gambling in the fortnight (%)
*** Panel C shows average gambling expenditure per fortnight in ($)
**** Green shows timeseries of the treatment group. Other colours give timeseries for various specifications of the control group. Gray is 2023 credit card gamblers, orange is 2024 transaction gamblers (unmatched), and light blue is 2024 transaction gamblers (matched).

Figure B.11: Heterogeneity in policy effects by average levels of liquidity



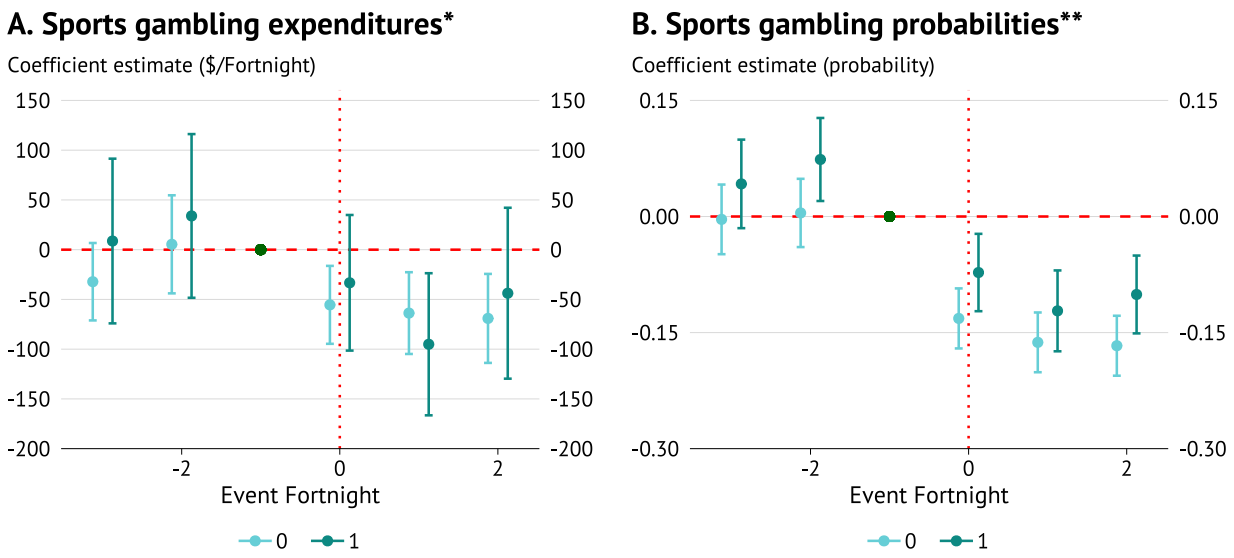
* Panel A shows event-study estimates by pre-policy liquidity quartiles where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates by pre-policy liquidity quartiles where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** Both panels compare the treatment group to 2024 transaction account sports gamblers (the within year unmatched specification). Quartile 1 are those with the lowest pre-period liquidity whilst quartile 4 are those with the highest pre-period liquidity. Here liquidity is measured as the mean balance in the transaction account across the fortnight.

Figure B.12: Heterogeneity in policy effects by measures of liquidity constraint 1



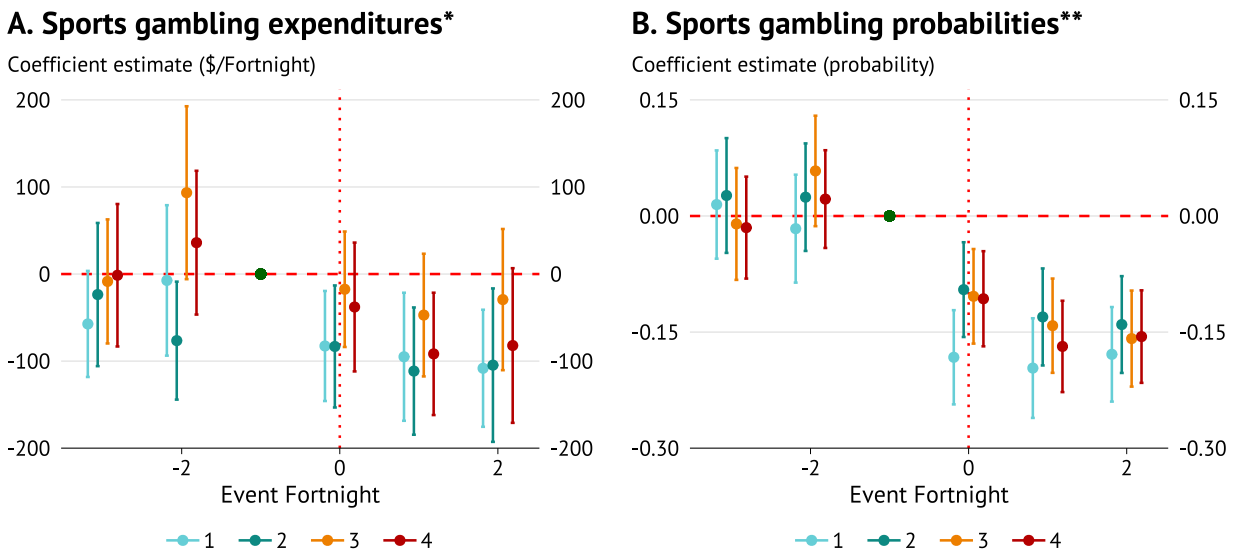
* Panel A shows event-study estimates by pre-policy liquidity quartiles where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates by pre-policy liquidity quartiles where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** Both panels compare the treatment group to 2024 transaction account sports gamblers (the within year unmatched specification). Quartile 1 are those with the lowest pre-period liquidity whilst quartile 4 are those with the highest pre-period liquidity. Here liquidity is measured as the minumum balance in the transaction account across the fortnight.

Figure B.13: Heterogeneity in policy effects by measures of liquidity constraint 2



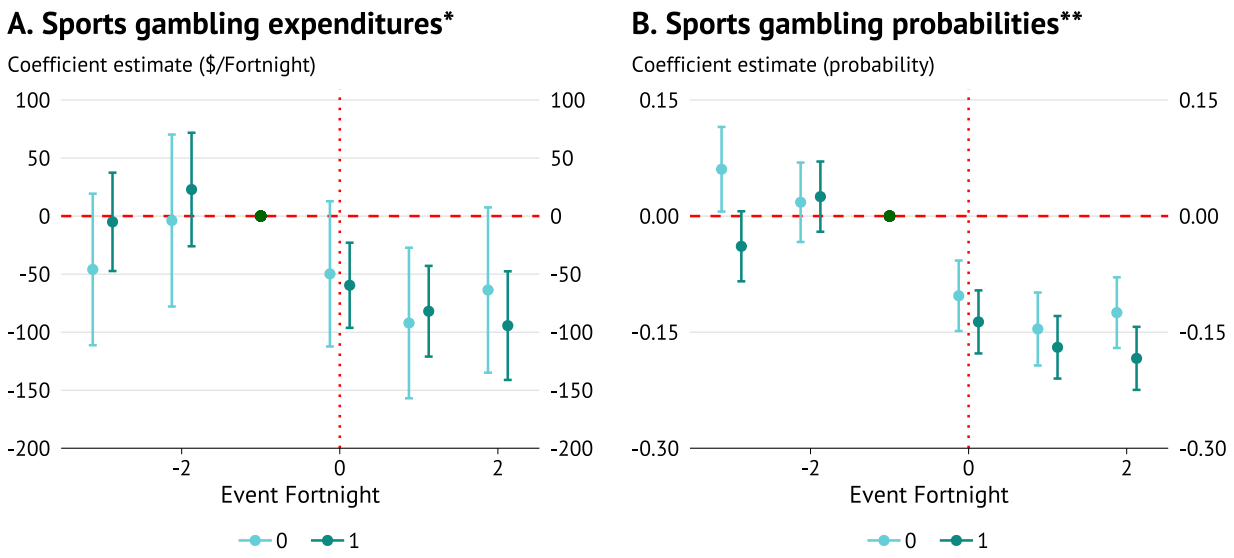
* Panel A shows event-study estimates by pre-policy liquidity constrained status where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates by pre-policy liquidity constrained status where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** Both panels compare the treatment group to 2024 transaction account sports gamblers (the within year unmatched specification). Someone is classified as liquidity constrained if during the pre-period the balance in their transaction account drops below $100 at any stage.

Figure B.14: Heterogeneity in policy effects by credit card debt holding



* Panel A shows event-study estimates by pre-policy credit card debt quartiles where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates by pre-policy credit card debt quartiles where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** Both panels compare the treatment group to 2024 transaction account sports gamblers (the within year unmatched specification). Quartile 1 are those with the lowest pre-period credit card debt whilst quartile 4 are those with the highest pre-period debt Here credit card debt is measured as the average level of outstanding debt on the card across the fortnight.

Figure B.15: Heterogeneity in policy effects by credit card debt revolving status



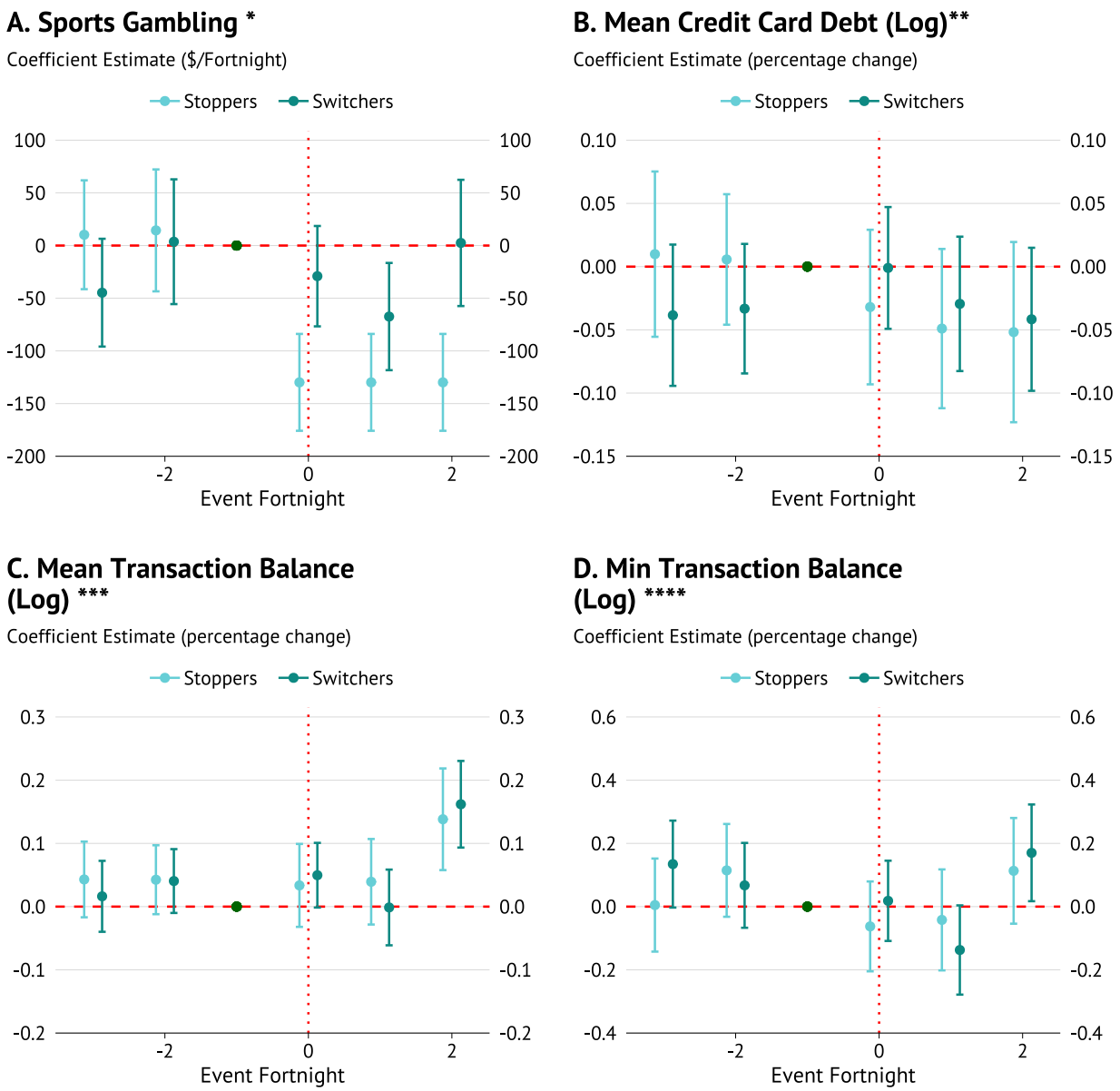
* Panel A shows event-study estimates by pre-policy debt revolver status where the outcome variable is total sports gambling expenditure per fortnight.
** Panel B shows event-study estimates by pre-policy debt revolver status where the outcome variable is a dummy for any sports gambling expenditure in the fortnight
*** Both panels compare the treatment group to 2024 transaction account sports gamblers (the within year unmatched specification). Someone is classified as a debt revolver if during the pre-period their level of outstanding credit card debt never falls below $100.

Table B.1: Predictors of stopping post policy change

	(1)	(2)	(3)	(4)	(5)
Dependent variable - Stopper (1 = stopped gambling post policy change)					
Gambling Quartile 2	−0.204***		−0.102**	−0.102**	−0.106**
	(0.036)		(0.034)	(0.034)	(0.034)
Gambling Quartile 3	−0.339***		−0.149***	−0.149***	−0.152***
	(0.037)		(0.035)	(0.035)	(0.035)
Gambling Quartile 4	−0.514***		−0.214***	−0.213***	−0.218***
	(0.037)		(0.038)	(0.038)	(0.038)
Both accounts		−0.520***	−0.443***	−0.444***	−0.437***
		(0.024)	(0.027)	(0.028)	(0.028)
Liquidity constrained				0.006	
				(0.026)	
Revolver				0.003	
				(0.024)	
Log(Transaction Balance)					0.006
					(0.007)
Log(Credit Card Debt)					0.003
					(0.010)
Constant	0.663***	0.651***	0.729***	0.726***	0.659***
	(0.026)	(0.017)	(0.024)	(0.028)	(0.094)
R^2	0.147	0.281	0.300	0.300	0.301
Adjusted R^2	0.145	0.280	0.298	0.297	0.297

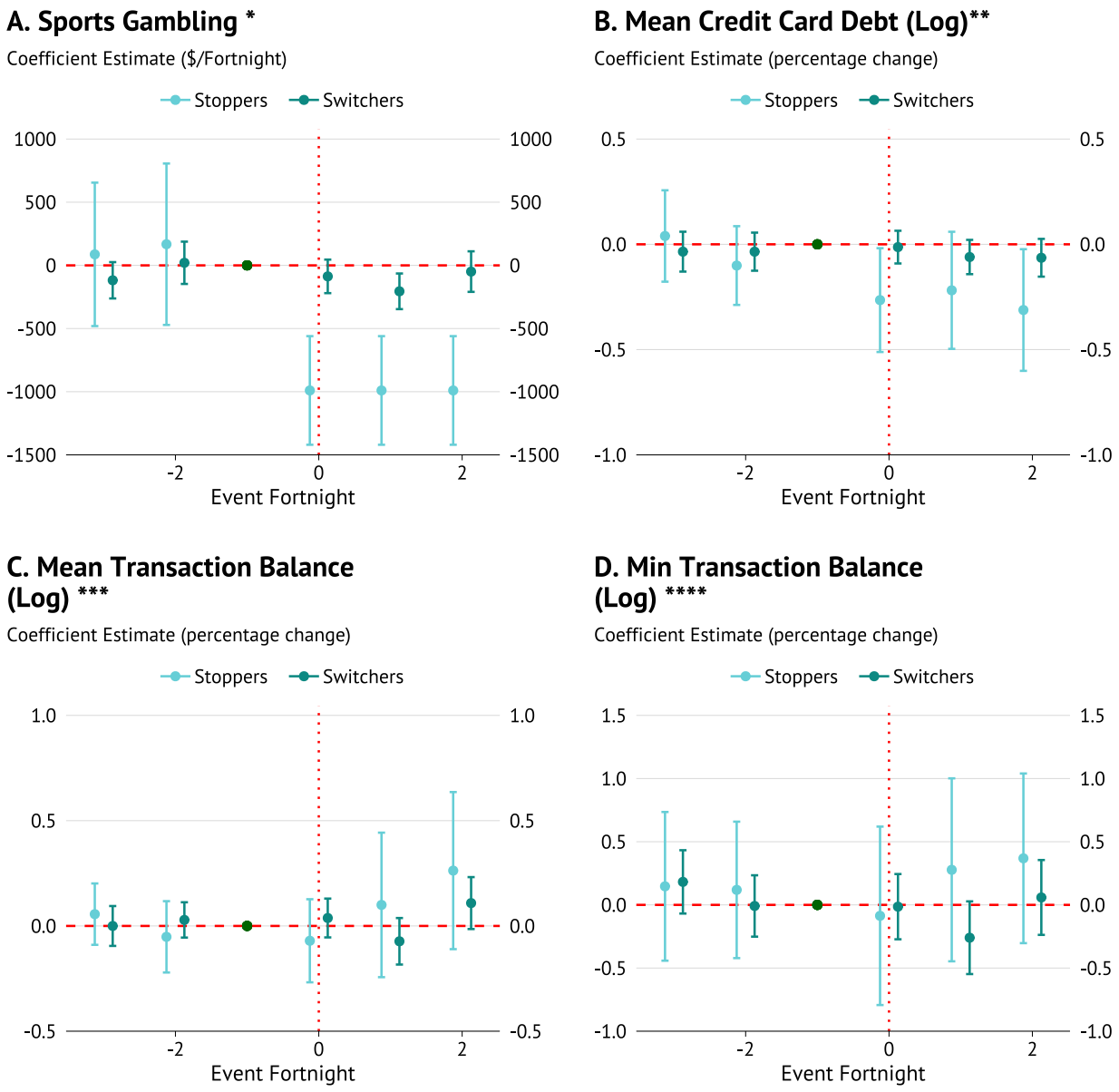
Notes: Each column reports estimates from a linear probability model where the dependent variable y_i is a dummy that takes value 1 if individual *i* has no observed sports gambling deposits in the six weeks following the policy change (i.e. stops gambling). The regressors in column (1) are indicators for pre-period gambling amount quartiles. The regressor in column (2) is a dummy taking value 1 if individual *i* uses both accounts to sports gamble pre-policy. Column (3) includes both sets of covariates. Column (4) adds controls in the form of dummies for if individual *i* is liquidity constrained or revolves their credit card debt pre-policy. Column (5) controls for the mean balance in the individuals transaction account pre-policy, as well as their mean level of outstanding credit card debt. Standard errors are reported in parentheses. Significance is given by *p<0.1; **p<0.05; ***p<0.01.

Figure B.16: First difference event-study by stopper-switcher status in treatment group



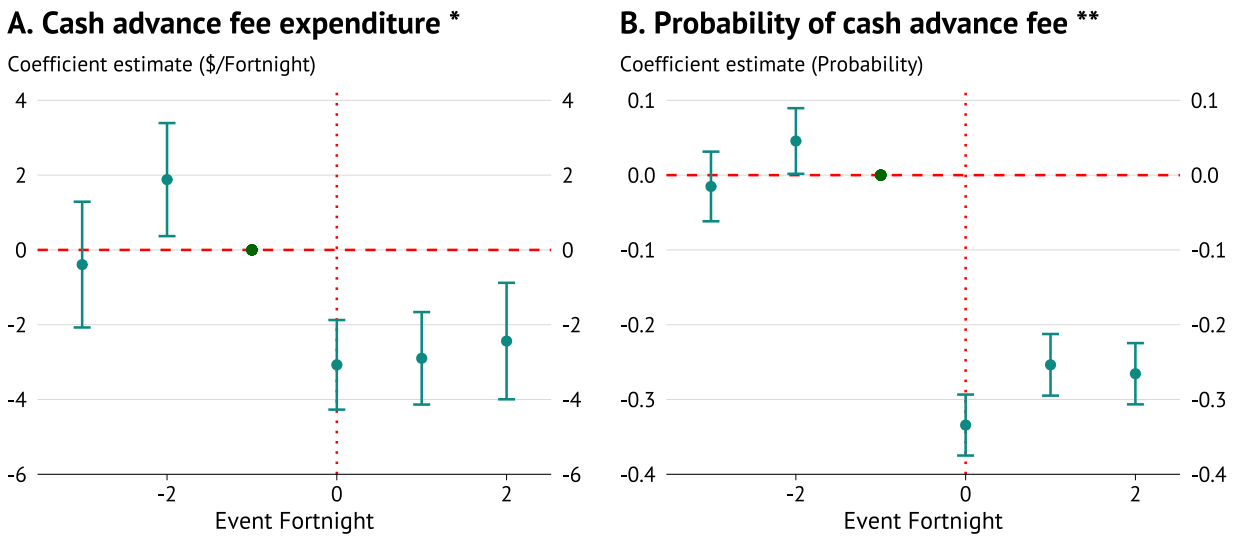
* The outcome variable in Panel A is total sports gambling in dollars.
** The outcome variable in Panel B is the mean level of outstanding debt on the credit card, in log terms.
*** The outcome variable in Panel C is the mean balance in the transaction account in logs.
**** The outcome variable in Panel D is the minimum balance in the transaction account in logs.
***** Event-study specifications are first differences - i.e. have no control group. The sample consists of all members of the treatment group.

Figure B.17: First difference event-study by stopper-switcher status in gambling quartile 4 of treatment group



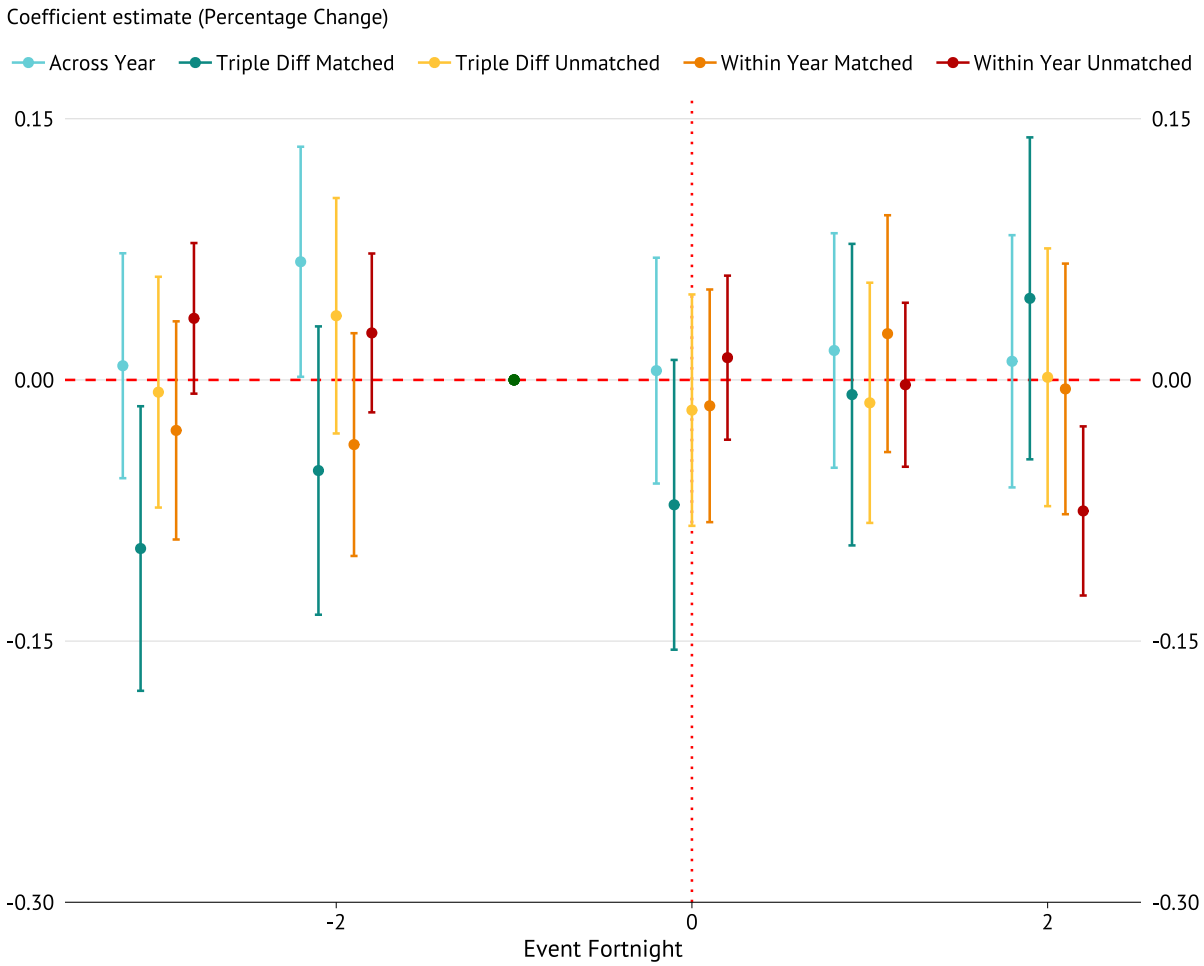
* The outcome variable in Panel A is total sports gambling in dollars.
** The outcome variable in Panel B is the mean level of outstanding debt on the credit card, in log terms.
*** The outcome variable in Panel C is the mean balance in the transaction account in logs.
**** The outcome variable in Panel D is the minimum balance in the transaction account in logs.
***** Event-study specifications are first differences - i.e. have no control group. The sample consists of memebers of the treatment group in the 4th quartile of pre-period sports gambling expenditure.

Figure B.18: Effect of the policy on cash advance fees



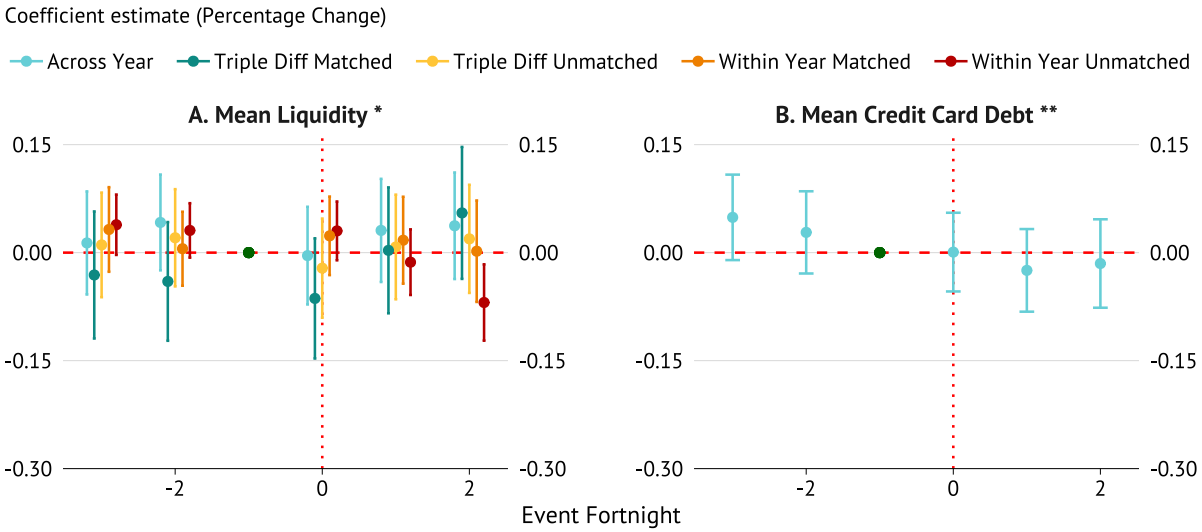
* Panel A has outcome variable of total spending on cash advance fees in dollars per fortnight.
** Panel B's outcome variable is the probability of incurring a cash advance fee in the fortnight.
*** Both panels compare the treatment group to 2023 credit card sports gamblers (the across year specification).

Figure B.19: Effect of the policy on services and non-durables expenditure



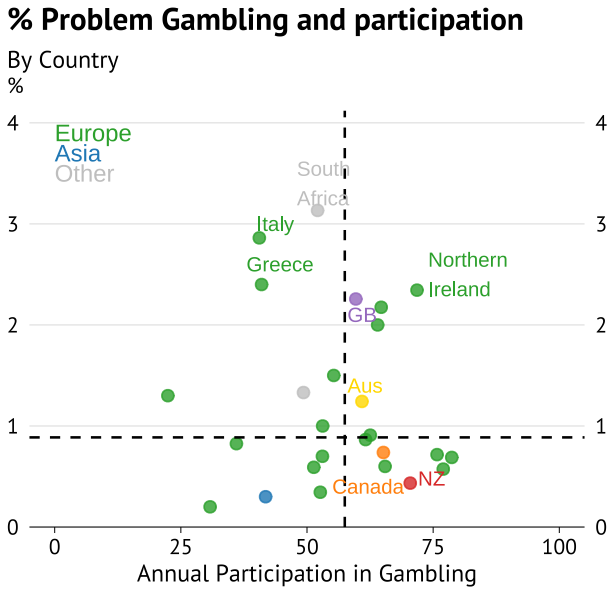
* Figure shows response of services and non-durables expenditure to the policy. Outcome variable is logged service and non-durables expenditure over the fortnight. Colours give different specifications

Figure B.20: Effect of the policy on account balances



* Panel A's outcome variable is logged mean balance on the transaction account during the fortnight
** Panel B's outcome variable is logged mean credit card debt outstanding during the fortnight.

Figure C.1: Australia ranks around average globally on problem gambling



* Dashed lines represent the median of both categories. Numbers are weighted averages for the latest ten years of data avaialable for each country, with more weight given to newer surveys.

APPENDIX D - DATA DISCLAIMER

HILDA Survey Disclaimer: This paper uses unit record data from Household, Income and Labour Dynamics in Australia [HILDA] Survey conducted by the Australian Government Department of Social Services (DSS). The findings and views reported in this paper, however, are those of the authors and should not be attributed to the Australian Government, DSS, or any of DSS' contractors or partners. DOI:10.26193/24EJST