Cashless gambling and the pain of paying: effects of monetary format on slot machine gambling

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Abstract

Advances in cashless technologies create a dilemma for gambling regulators. Research indicates that cash purchases entail a 'pain of paying' that is attenuated with more abstract forms of payment, yet limited research has directly tested the impact of mode of payment on gambling behaviour. Across two experiments, community-recruited gamblers were randomized to use an authentic slot machine in the laboratory, under different conditions of monetary endowment. In Experiment 1 (n = 61), participants were endowed with funds to play the slot machine, in either a cash or voucher format. In Experiment 2 (n = 48), participants acquired the cash endowment as a windfall or from an earning task. In session-level analyses, bet size and bet volume did not vary as a function of monetary condition. In more sensitive trial-level analyses, no interactions involving the monetary manipulations were consistent across the two experiments. Data from both experiments indicated faster spin initiation latencies as a function of losing streak length, and slower spin initiation latencies and larger bet size as a function of the prior win magnitude. These trial-level analyses show systematic influences on gambling behaviour in the laboratory environment, supporting the basic sensitivity of our design. Overall, our data provide weak evidence for the hypothesis that monetary factors influence gambling tendencies. Acknowledging the possibility of the null hypothesis, these data also highlight the methodological challenges with manipulating monetary value in gambling research, including the use of endowed funds, and controlling for sources of variability when using authentic slot machines.

Introduction

Money is a central feature of gambling (Binde 2013). Modern commercial gambling is an activity that necessarily costs money, with a chance of winning a larger prize than the amount bet. Regulatory issues surrounding money and gambling are becoming more important as payment technologies evolve (Gainsbury and Blaszczynski 2020). In the North American casino landscape, bill acceptors and Ticket-In Ticket-Out (TITO) interfaces began to replace coin operation on electronic gaming machines (EGMs) in the early 2000s. A contemporary slot machine will accept either a banknote or TITO voucher, but wins or remaining funds on that machine can only be cashed out as a voucher, which the gambler must take to a cashier desk to convert back into actual cash. Recent technological advances could readily enable card-based payments (either debit cards, credit cards or venue loyalty-card programs) or contactless payments (e.g. via mobile phone) (Parke et al. 2008) in gambling venues, subject to regulatory approval. While most jurisdictions are yet to embrace these developments, regulators may anticipate industry pressure, given the added convenience as our societies become 'cashless'. These developments may be amplified in the wake of the COVID-19 pandemic, which restricted the use of physical cash in many countries (e.g. Wilson, 2020), and precipitated the temporary closure of land-based gambling venues, supporting a migration to online gambling (Price 2020; Håkansson 2020). Relatively little is known about how gambling payment format affects gambling behaviour, and whether these developments could exacerbate gambling-related harm (Swanton and Gainsbury 2020).

Economic theory stresses that money is fungible: one $20 bill is worth the same as any other $20 bill. At the same time, not all $20 transactions are equal. For example, consumer behaviour changes as a function of which 'mental account' a payment comes from (Thaler 1985) (see

Muehlbacher and Kirchler 2019 for review). Each purchase is associated with a psychological cost termed the ‘pain of paying’ (Prelec and Loewenstein 1998; Prelec and Simester 2001), which is reconciled against the value of the good that is obtained. Several factors are thought to modulate the psychological pain experienced. Here we consider two specific factors; the method of payment, and how the money was obtained. Payments made with physical cash (i.e. bills or coins) are hypothesized to be more ‘painful’ than cashless payments, and research has found that people spend more when using more abstract forms of payment, such as credit cards (Soman 2003; Thomas et al. 2011; Meyll and Walter 2019), vouchers (Raghubir and Srivastava 2008), or mobile payment technology (Meyll and Walter 2019). By some accounts, cash payments may differentially recruit actual pain-related circuitry in the brain e.g. the insula (c.f. Banker et al. 2021). Various boundary conditions appear to exist for pain-of-paying effects (See-To and Ngai 2019) and it is conceivable that these effects may be changing over time as the use of real-world cash declines, and cashless payments become the norm.

These influences have received limited attention in the specific context of gambling behaviour and harmful gambling. A number of studies have tested a coarse comparison of gambling for money, versus non-incentivized predictions or gambling for points (e.g. Meyer et al. 2000; Ladouceur et al. 2003; Weatherly and Brandt 2004; Wulfert et al. 2005). These studies consistently indicate increased arousal and altered gambling behaviour when money is at stake, but these designs do not speak to the contemporary discussions around cashless technologies, in which the money is real but takes a less tangible form. Other studies have examined how the balance information is displayed in electronic gaming machines (EGMs), in either a cash (e.g. $9.90) or credit (990) format. In an observational study in regular gamblers, 86% reported using the cash display setting and 58% of these endorsed the view that this feature helped to control their gambling (Ladouceur and Sévigny 2009). In a laboratory study manipulating the availability of a cash counter, pathological gamblers gave lower ratings for 'difficulty of stopping play' in the cash counter-on compared to the -off condition (Loba et al. 2001). Other work has considered the removal of high denomination bill acceptors from EGMs (Blaszczynski et al. 2005; Sharpe et al. 2005). Under this configuration, a gambler could enter 5 × $20 bills but would not be permitted to insert a single $100 bill. People with gambling problems were more likely than the recreational gamblers to use high denomination bills for gambling, but restricting this feature had no discernible impact on gambling behaviour. The clear differences between these manipulations highlight the limited nature of the current evidence base for monetary influences on gambling (Palmer et al, 2021). In these examples, the use of cash displays and restrictions on high denomination bills may be considered subtle manipulations that might 'nudge' gamblers towards healthy behaviour, but these experiments do not directly address the possible impacts of cashless modes of payment on gambling behaviour.

A further factor that modulates the pain of paying is the source of the money. According to the 'house money effect' (Thaler and Johnson 1990), participants are more willing to spend money that has been won than earned money. In 'real-effort' procedures in behavioural economics, participants engage in an initial task in which funds are earned through an effortful, monotonous procedure, to create a sense of ownership (Erkal et al. 2011). Earned funds were associated with less spending compared to windfalls (Reinstein and Riener 2011; Corgnet et al. 2015), and higher levels of earned income were associated with lower donations on a subsequent charitable giving task (Erkal et al. 2011). Earning manipulations have not been directly examined in a gambling context. In a field study of 'windfalls', casino patrons who received a free-credit voucher upon entry actually gambled less, in contrast to the house money effect (Rüdisser et al.

2017). As laboratory experiments on gambling typically rely on endowed funds (akin to a windfall), some studies have sought to encourage participants to treat the endowment as their own money. When playing a slot machine simulator, participants who initially saw and held their cash endowment gambled less and left with more money than those who were not given this opportunity (Weatherly et al. 2006). Another study found no difference in behaviour between participants who were shown a picture of the money, versus no picture (Brandt and Martin 2015).

In the present study, we manipulated monetary format in two experiments using authentic multi-line slot machines housed in a laboratory environment. Across both experiments, we hypothesize that endowment conditions that increase the pain of paying would decrease risky gambling behaviour, and vice versa (see Figure 1). In Experiment 1, we compared a standard cash endowment with a voucher condition, based on a realistic TITO voucher. We predicted that the voucher would be associated with reduced pain of paying and thus increased gambling intensity. In Experiment 2, we compared a 'windfall' endowment with an earned condition based on a real-effort procedure, predicting that the earned condition would experience increased pain of paying and thus decreased gambling intensity. In each experiment, the primary analyses of gambling intensity relied on the total number of bets and the average bet size, aggregated over the session. Notably, our cash condition in Experiment 1 and the windfall condition in Experiment 2, although named differently, had highly comparable endowment procedures (see Figure 1).

A further 'trial-level' analysis was undertaken to examine the amount bet, and the pace of play, as a function of a number of in-game factors that could not be controlled in the context of an authentic slot machine game. Inspired by behavioural research on the 'micro analysis' of alcohol consumption and smoking (Gust et al. 1983; Davidson et al. 1999; Lee et al. 2003), this was expected to be a more sensitive analysis, taking into account the number of successive losses, the size of any previous win, and the current in-game balance. For example, the post-reinforcement pause (PRP) refers to a slowing in the time taken to initiate the spin, following a winning outcome compared to a loss (Delfabbro and Winefield 1999; Dixon et al. 2013; Chu et al. 2018). (Note this effect has both an appetitive/hedonic component and an aversive/frustrative component, Eben et al. 2020). Both the PRP effect and the average bet size also scale with the *size* of a prior win (Tremblay et al. 2011; Dixon et al. 2013). The number of successive losses can also modulate the bet size (Studer et al. 2014; Tobias-Webb et al. 2016); and putatively, the machine's current balance may serve as a reference point to elicit either loss chasing (when losing) or a house money effect (when in profit) (c.f. Chapman et al. 2019). Our trial level analyses tested for these systematic influences, in order to examine the sensitivity of our basic approach (i.e. studying authentic slot machines in a laboratory environment) and the consistency of any effects across the two experiments.

Methods

This study was approved by the Behavioural Research Ethics Board at the University of British Columbia (H16-01168). Participants provided written informed consent prior to participation.

Participants

For both experiments, participants were recruited through advertisements online (Craigslist, Kijiji, and departmental websites) and in local newspapers. Participants were eligible for inclusion if they had gambled on slot machines (land-based or online) in the past three months, were 19 years or older, and had normal or corrected-to-normal vision. Prior to participation, individuals were screened for eligibility by telephone. Individuals were excluded if they scored greater than seven on the Problem Gambling Severity Index (PGSI) (Ferris and Wynne 2001), or had ever sought treatment for gambling problems or enrolled in voluntary self-exclusion. Further exclusion criteria were a history of neurological illness, head injury, or psychiatric hospitalisation.

Experiment 1: Cash vs voucher. Data were collected from 69 participants and complete data is reported from n = 61 (cash = 30, voucher n = 31). Eight participants could not be included due to early problems with our video capture procedures from the slot machine session.

Experiment 2: Windfall vs Earned. Data were collected from 53 participants and complete data is reported from n = 48 (windfall n = 28, earned n = 20). Data from one participant was excluded as they did not meet the inclusion criteria, one participant had incomplete video data, and three participants in the Earned condition did not engage with the earnings task.

Procedures

Experiment 1: Cash vs voucher. Participants attended a single test session lasting approximately two hours. Upon arrival, participants were randomly assigned to the 'voucher' or 'credit' group. In a standard testing room, participants completed the consent procedure and PGSI administration, followed by some further questionnaire measures and a computerized decision-making task (to be reported elsewhere) on which they could win a small amount of money. Participants were given written instructions for the slot machine session and were informed that the EGM video feed would be recorded. The slot machine used was Great Wall II (Williams Interactive, WMS), which was provided to our laboratory by the British Columbia Lottery Corporation (see Supplementary S1). Participants were instructed that they would have up to 30 minutes to play the slot machine. This included a fixed period, followed by a further period when they were free to stop at any time. The end of the fixed period was indicated by flashing the ambient lighting. If the participant chose to stop playing before the 30 minutes ended, or ran out of machine credits, they were asked to remain in the lab, and were given neutral reading materials to pass the time. Any credits remaining at session end would be payable as a cash bonus (bonus = final balance divided by two, up to a maximum of $50). For a study in community gamblers, we considered it important to use an incentive structure that was directly related to their gambling outcomes, while balancing the ethical consideration that with an authentic slot machine, some participants could win large jackpots.

Following the instructions, participants in the cash group were given $40 (CAD) in $5 bills, and were asked to count this money. Participants in the voucher group were given a $40 paper slip modeled on the TITO vouchers used in local casinos. All participants were asked to write down the value of the funds received, on a participant payment sheet that also displayed the formula for the cash bonus. Participants were then taken to an adjacent room housing four slot machines, with comfortable casino stools and dim lighting.

Participants in the cash group were asked to load the $40 into the machine. The voucher group saw and held the voucher, but the slot machine was pre-loaded with the $40 credit before the participant entered the lab. Nevertheless, the participant was instructed to post the voucher into a black box attached to the machine next to the bill acceptor. As part of the manipulation, the slot machine display was set to the cash format in the cash group, and the credit format in the voucher group. As experienced slot machine gamblers, the participants were instructed that they could vary their betting style during the session across both the number of lines and the credits per line. Upon initiating the first bet, the experimenter started a timer and exited the room, in order to ensure a naturalistic environment and reduce observer effects (e.g. Rockloff and Dyer

2007). After ten minutes, the lights in the room were flashed on and off several times by the experimenter outside the room. After 30 minutes, the experimenter re-entered the room and noted the machine balance. The participant returned to the original testing room, recorded their final balance and corresponding bonus payment on the payment form, and then completed some further questionnaires. Debriefing included both verbal and pamphlet information about myths associated with slot machines and local resources for problem gambling.

Experiment 2: Windfall vs Earned. Upon arrival participants were randomly assigned to the windfall or earned group. The first stage of the procedure was identical to Experiment 1, with the key difference that participants in the earned group completed an initial task to earn the funds for their subsequent slot machine session. The Navon task (Navon 1977) was chosen as a cognitively demanding but monotonous task in which the participant views compound letters (e.g. the letter H constructed from small Ss), and must identify the local letter (S or H) on each trial. Participants were instructed that they would earn 20 cents for each correct answer and they needed to earn $40 for the slot machine session. When the participant had earned $40, they were given the cash in $5 bills, asked to count it and fill in the payment record, and placed the cash in their wallet, purse or pocket. In the windfall condition, participants were given a magazine to read instead of completing the Navon task, and after 20 minutes they were given the $40 in $5 bills. For the slot machine session, there were two adjustments from Experiment 1: i) we used a different slot machine, Buffalo Spirit (Williams Interactive, WMS) (see Supplementary S1), ii) the fixed period of required play was reduced from 10 to 5 minutes (see Supplementary S2).

Data Extraction

Behavioural data capture from authentic slot machines is not straightforward. In these experiments, the gambling session was recorded by splitting the video output from the slot machine's internal computer, and events were extracted from this feed using custom python scripts (see Supplementary S2).

Analysis

All analyses were carried out in R (R core team, Vienna) and R scripts are available online (https://github.com/CGR-UBC/cashless_casinos_2021/). We used identical analysis pipelines for both experiments. The analysis for Experiment 2 was pre-registered (https://aspredicted.org/blind.php?x=nr6zy3) based on preliminary analyses from Experiment 1. Ultimately, we made some deviations to our pre-registered plan for Experiment 2 (see Supplementary S4), due to unanticipated characteristics of the data that were revealed in further analysis of the Experiment 1 dataset.

For each experiment, group characteristics (age, PGSI, self-reported monthly slots expenditure) were compared between groups using Wilcoxon rank sum tests, due to these data not meeting the assumption of normality. Gender was compared between groups using Chi-square tests.

Our analyses comprise a 'session-level' comparison of the experimental conditions, i.e. the per participant summary variables from the slot machine session, and a further 'trial-level' analysis using multiple regression models on the entire trial-by-trial dataset (i.e. a single datasheet comprising all spins, from all participants). For the session-level analysis, we identified summary variables with the aim of distinguishing risk-taking and persistence as different expressions of gambling intensity (see Supplemental S2 for further explanation): 1) mean bet size, 2) total bet amount across the whole session, 3) machine balance at the end of the session, 4) total bet amount in the initial five minutes. Each of these scores were compared between conditions with Wilcoxon rank sum tests, due to deviations from normality in these data. Four participants were excluded from the session-level analyses: one participant in each experiment chose to stop playing before the end of the fixed period, and two participants in Experiment 2 accidentally cashed out (a button that renders the machine unplayable while an attendant is called). Available data for these participants were included in the trial-level analysis.

In the trial-level analysis, participant number was entered as a fixed effect. Fixed effects regression allows each participant to act as their own control, and this is well-suited for handling missing and unbalanced data (Allison 2005; Studer et al. 2014; Murch et al. 2017; Chu et al. 2018) (see also Supplementary S3). Separate models were run on trials following a win (i.e. any non-zero outcome), and trials following a loss, in order to include win size, and losing streak length, as linear predictors that were specific to these respective conditions. Due to the distribution of outcomes on a slot machine, the loss models inherently contained more trials than the win models. As well as distinguishing these two sets of models, two dependent variables were considered. The spin initiation latencies were analyzed with linear regression. A spin initiation latency was defined as the time from the end of a trial (when the button panel is released to allow the next bet) to the participant starting the next trial by pressing the 'spin' button. Trials with latencies over 10 seconds were removed (see Supplementary Table S1 for the number of trials removed in each model, and Supplementary S4 for the outlier approach), and the latency data were log transformed. Bet size was analyzed using logistic regression, as a binary variable indicating whether any given bet was below (or at) the participant's median (= 0), or above the participant's median (= 1), as a function of the prior outcomes. In summary, four models were specified for each experiment: a Win model, including the size of the prior win as a predictor, on the spin initiation latencies and the bet sizes; and a Loss model, including the losing streak length, on the spin initiation latencies and the bet sizes.

For the Loss models, the following regressors of interest were entered: loss streak length (number of trials since a win, log transformed), the current Machine Balance (in dollars), and the interaction of these regressors with group (Experiment 1: cash (0) vs voucher (1); Experiment 2: windfall (0) vs earned (1)). Coding the reference categories in this way facilitates the comparison of the cash and windfall conditions, which have similar endowment procedures. For the Win models, the win size (in cents, log transformed) and the interaction between log win size and group were the predictors of interest. Machine Balance was tested in the Loss models due to the greater number of available trials, and was entered as a regressor of no interest in the Win models. For all models, trial number (square root transformed) was entered as a regressor of no interest. For the spin initiation latency models, a binary variable indicating whether the bet amount was changed was entered as a regressor of no interest, as any change in the betting configuration is likely to delay the initiation latency. For any models where significant ($p < .05$) interactions with group were observed, the model was re-run with the groups reversed, to test for the effect in the alternative reference category.

Regression models were tested using robust regression, to reduce the impact of outliers and deviations from normality. All models were visually assessed to check residuals were normally distributed, and the weights applied during the robust regression were inspected to ensure that there was no systematic bias in the de-weighting of data points that may reduce the interpretability of the models. To produce a visual representation of the raw data, data from all participants were combined. Linear predictors were binned, and a boxplot was produced using these bins as categories. For the model predictions, predictions were made for every participant, and the mean of these predictions was plotted. All variables in the model (other than the variable plotted and group) were fixed at the median, with the exception of the binary bet change variable which was set at zero (no change). Therefore, the predicted plots show the effect of the variable of interest, controlling for the other variables in the model. In contrast, the raw data boxplots do not separate the effects of different variables, or account for the unbalanced nature of the data between participants.

Results

Across both experiments, the groups did not differ significantly in age, gender, PGSI score, and self-reported past-month slot machine expenditure (Table 1). For the session-level analysis, we did not observe any group differences between the four summary variables in either experiment. Thus, neither monetary manipulation had an overall effect on gambling intensity at the session level (Table 1).

For the trial-level analysis, we observed several effects on betting behaviour and spin initiation latency, as a function of the current state of the machine. The regression models are reported in full in Supplemental Tables S3-S10.

Models with Spin Initiation Latency as the Dependent Variable

Loss Streak Length. In Experiment 1, we observed a significant negative effect of loss streak length in the cash group. As loss streak length increased, the spin initiation latencies became faster (Table 2, Figure 3A). This effect was significantly modulated by group, and was not significant in the voucher group. In Experiment 2, we observed a significant effect in the windfall group, again finding that as loss streak length increased, the spin initiation latencies became faster (Table 2, Figure 3B). This effect was not significantly different in the earned group.

Machine Balance. In Experiment 1, we did not observe any effects of Machine Balance on the spin initiation latencies (Table 2, Figure 3C). In Experiment 2, we observed a significant negative effect of Machine Balance in the windfall group. As Machine Balance increased, the spin initiation latencies became faster. This effect was significantly modulated by group, and in the earned group, as Machine Balance increased, spin initiation latencies became slower (Table 2, Figure 3D).

Win Size. In Experiment 1, we observed a significant effect of win size on spin initiation latency. In the cash group, as the size of a previous win increased, the spin initiation latencies became slower (Table 2, Figure 3E), in line with a post-reinforcement pause effect. This effect did not differ across groups. In Experiment 2, we observed a significant effect of win size in the windfall group, again observing slower spin initiation latencies as the size of the win increased (Table 2, Figure 3F). This effect was attenuated (indicated by a significant win size by group interaction), but was still significant, in the earned group.

Models with Bet Size as the Dependent Variable

Loss Streak Length. In Experiment 1, we observed a significant effect of loss streak length on the bet size (Table 3, Figure 4A). In the cash group, as a losing streak increased, the probability of placing a high bet decreased. This effect did not differ significantly in the voucher group. In Experiment 2, the predictor for loss streak length was not significant (Table 3, Figure 4B).

Machine Balance. In Experiment 1, we observed a significant effect of Machine Balance on the bet size (Table 3, Figure 4C). In the cash group, as Machine Balance increased, the probability of placing a high bet increased. This effect did not differ significantly in the voucher group. In Experiment 2, the predictor for Machine Balance was not significant (Table 3, Figure 4D).

Win Size. In Experiment 1, we observed a significant effect of the amount won on the size of the next bet (Table 3, Figure 4E). In the cash group, as win size increased, the probability of placing a high bet increased. This effect was not modulated by group. In Experiment 2, we observed the same effect in the voucher group: as win size increased, the probability of placing a high bet increased (Table 3, Figure 4F) and again, this effect was not modulated by group.

Discussion

Across two experiments, we examined the impact of monetary manipulations in participants who were experienced slot machine gamblers, using an authentic slot machine housed in a laboratory environment. In Experiment 1, we manipulated the mode of payment, by comparing cash and voucher conditions. In Experiment 2, we manipulated how the money was acquired, by comparing earned and windfall conditions. We did not find evidence to support our predictions, inspired by the 'pain of paying' hypothesis, that monetary factors would influence session-level gambling intensity. Neither measures of average bet size nor overall bet volume differed significantly by mode of payment (Experiment 1) or how the money was acquired (Experiment 2).

Due to the variability that is inherent to using real EGMs, our trial-level analysis tested for effects of monetary condition in the context of several game-level factors. This was, effectively, a more sensitive 'manipulation check' of gambling in our laboratory environment. These analyses indicated systematic effects on bet amount and speed of play, as a function of losing streak length and the size of a previous win. In discussing these analyses, we emphasize effects that were consistent across the cash condition (Experiment 1) and the windfall condition (Experiment 2), as largely comparable conditions. Machine balance, a third game-level predictor, did not exert consistent effects from this perspective. On speed of play, we observed a significant effect of losing streak length on spin initiation latencies: participants initiated their next bet more quickly as the number of sequential losses increased. This loss-induced impulsivity was previously observed on the trial immediately following a loss (Verbruggen et al. 2016; Eben et al. 2020) and our data extend this effect, showing that this speeding accumulates over a sequence of losses. This effect may constitute an over-looked expression of loss chasing, whereby gamblers respond in a faster and more uncontrolled way on losing streaks (Zhang and Clark 2020).

In the win models, the magnitude of wins also exerted a reliable effect on both the initiation speed and the size of the next bet. As win magnitude increased, the spin initiation latencies slowed. Prior work has shown that this 'post-reinforcement pause' scales with win magnitude in gamblers playing a simulated slot machine game (Dixon et al. 2013; Dixon et al. 2014; Dixon et al. 2019). Our data extend these findings, showing the high sensitivity of this variable to reward value during authentic slot machine use. The corresponding effect on the size of the next bet could be interpreted as a house money effect (Thaler and Johnson 1990) or in terms of an availability heuristic (Croson and Sundali 2005), that the prospect of further wins is easily brought to mind, encouraging a high wager. This effect also accumulates with winning streak length in a recent analysis of baccarat gambling (Abe et al. 2021). The collective results of the trial-level analyses demonstrate the sensitivity of our dependent variables and modelling approach for investigating slot machine behaviour in the laboratory environment. Although participants were not playing with their own money in a real casino, the trial-level predictors are psychologically plausible, and reproducible across the cash and windfall groups in the two experiments.

The trial-level analyses identified some statistically significant interactions between the game-level predictors and our monetary conditions. In Experiment 1, the effect of losing streak length on spin initiation latency in the cash group was abolished in the voucher group. This is to say, the voucher group did not show the accumulative speeding effect on a sequence of losses. In Experiment 2, the effect of win magnitude to lengthen the spin initiation latency (i.e. the post-reinforcement pause effect) was attenuated in the earned group. In both cases, these interactions were not robust across the two experiments. Without *a priori* hypotheses linking the game-level predictors to the pain of paying framework, we are cautious about the interpretation of these effects. We also acknowledge that by analyzing Experiments 1 and 2 separately, we have not statistically compared these terms. Future research may consider looking to replicate these preliminary effects using pre-registered designs.

In Experiment 1, we observed two further effects on bet size in the cash group that were not replicated in the windfall group of Experiment 2. In the cash group, bet size decreased as a function of losing streak length. Losing streak length also represents an increasing distance from the gambler's last win; this could elicit either pessimism or optimism (via a gambler's fallacy effect) about one's chances of winning. The reduced bet size implies the former, in line with a ‘cold-hand’ effect (Croson and Sundali 2005). Bet size also increased as a function of Machine Balance in Experiment 1: gamblers tended to bet higher when they were more ‘in the black’, and this supports the ‘house-money’ effect that was also seen for the win magnitude predictor across both experiments. For the analyses of machine balance, the negative expectancy of the slot machine dictated that most participants spent much of their sessions below their starting balance (‘in the red’). This range restriction, alongside the smaller sample size in Experiment 2, may have compromised our ability to test (and confirm) the Machine Balance effect in Experiment 2.

Methodological Considerations

One interpretation of the lack of evidence for monetary effects in our session-level analyses is clearly that changes in monetary format are not associated with changes in risky or uncontrolled gambling. This account may appeal to stakeholder groups keen to promote the adoption of digital payment methods. The traditional forms of evidence for ‘pain of paying’ observed in consumer research ten years ago may also have attenuated, as the population adapts to cashless alternatives. Our own view is that our findings also highlight the methodological challenges with manipulating monetary factors in the laboratory, especially in the context of endowed funds (Gainsbury and Blaszczynski 2011). Although our participants were experienced gamblers, they were not playing with their own money. Our procedure included a number of elements intended to reinforce our monetary manipulations (e.g. a realistic in-house ‘voucher’, and asking participants to count and hold the bills), but it is possible that these features were unsuccessful. If participants continued to construe the endowment as a windfall across all conditions, any “pain of paying” effects may be negligible. Similarly, our earning manipulation in Expt 2 was contrived in so far as it was an unavoidable component of our procedure; participants could not decide to ‘not work’ (other than by withdrawing from the study), nor can we be sure our earning task successfully fostered a sense of ownership. Clearly, reimbursement procedures carry ethical considerations that are especially important in gambling research (Cantinotti et al. 2016), but we suggest there is nonetheless scope for methodological refinement here, such as borrowing procedures from behavioural economics (Erkal et al. 2011; Rüdisser et al. 2017) or examining windfalls during the gambling game itself (Rockloff et al. 2020).

In our experiments, the sensitivity of our designs was also affected by the variability associated with using authentic slot machines. While the games afford ecological validity, the outcome sequence cannot be controlled, and we see substantial within-condition variability in profit/loss (machine balance) and the ensuing subjective experience of our participants (e.g. elation, frustration). This variability was further amplified by our decision to allow participants to vary their bets, which we took in order to derive more direct measures of risk-taking (see Supplementary S2). In future studies, the use of realistic simulators to present a controlled sequence could reduce this variability, although it is impossible to fully eliminate some outcome variability if participants are allowed to vary their betting strategies.

Our findings should be considered in light of a number of further strengths and weaknesses. First, although we pre-registered the hypotheses for Experiment 2, behavioural data from authentic slot machines are complex, and some deviations were necessary from the pre-registered plan (see Supplementary S4). With the richness of the data, precise operationalization of behavioural variables is key: alternative session-level variables may have shown greater sensitivity to monetary factors. In our trial-level analyses, bet size was a binary variable centered on each participant's average bet, but this variable did not distinguish changes in line style and bet multiplier strategy, which exert somewhat distinct effects on the reinforcement profile (Barr and Durbach 2008). Second, our decision to recruit experienced gamblers traded off against reasonably small group sizes. Although many of our participants scored in the 'at risk' range on the PGSI, from our decision to exclude participants scoring 8 or higher, it is possible that our monetary manipulations may exert stronger effects in those with gambling problems. We did not test for moderating effects of PGSI or age, which would be worthwhile in larger samples. We did not collect data on income or socioeconomic status, which could moderate the impact of financial factors and 'wealth shocks'. Lastly, some minor procedural differences existed between Experiments 1 and 2; for example, the slot machine cash/credit display in Experiment 1 was congruent with the cash/voucher condition, but was not systematically controlled in Experiment 2, which could have contributed to some inconsistent findings between the two studies.

Collectively, these findings highlight the challenges that face policy-oriented research on the impact of monetary formats on gambling behaviour. Despite our design gaining external validity from the use of both authentic gambling products and experienced slot machine gamblers (the "real gamblers, real games" requirement for evaluations of responsible gambling tools by Ladouceur et al. 2017), there are methodological barriers to examining the psychological impacts of financial factors in the laboratory. Given jurisdictional differences in EGM specifications and the logistical challenges with community-based recruitment, future research could benefit from pooling data collection across multiple labs. Improved access to field data (e.g.) gambling operators will also aid policy-related decisions around cashless gambling. Although constraints also apply in the field -- for example, there is no 'cash' option on a gambling website -- better understanding of financial influences on gambling will likely require convergent data including both controlled laboratory designs and ecologically-valid field research.

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Expt 1a:	Cash	Voucher	
Demographic variables			
N	30	31	
Age	48 (21 - 79)	44 (20 - 71)	W = 384.5, r = 0.07, p = .58
Gender	12 male, 18 female	18 male, 13 female	χ_2(1) = 1.33, p = .25
PGSI	1 (0 - 6)	1 (0 - 4)	W = 424.5, r = 0.09, p = .55
Slot spend per month ($)	45 (0.5 - 500)	30 (1.6 - 400)	W = 490.5, r = 0.05, p = .72
Session-level variables			
Mean bet size (cents)	30.53 (1.41 - 102.23)	30.46 (1.83 - 102.23)	W = 502, r = .10, p = .45
Total bet (session) ($)	52.75 (1.00 - 270.60)	54.37 (0.73 - 151.50)	W 436, r = .026, p = .84
Final balance ($)	26.24 (0 - 156.78)	14.50 (0 - 51.38)	W = 569.5, r = .23, p = .077
Total bet by 5 minutes ($)	17.00 (0.61 - 44.70)	14.56 (0.45 - 63.80)	W = 461, r = 0.020, p = .88
Expt 1b:	**Windfall**	**Earned**	
Demographic variables			
N	28	20	
Age	42 (19 -81)	53.5 (19 - 54)	W = 249, r = .066, p = .66
Gender	11 male, 16 female, 1 other	8 male, 12 female	χ_2(2) = .732, p = .69
PGSI	2 (0 - 6)	1.5 (0 -6)	W = 331.5, r = .16, p = .28
Slot spend per month ($)*	50 (0 - 1000)	100 (2 - 500)	W = 257.5, r = .07, p = .64
Session-level variables			
Mean bet size (cents)	40.00 (4.89 - 117.66)	40.00 (3.52 - 188)	W = 258.5, r = .037, p = .80
Total bet (session) ($)	49.13 (4.39 - 208.69)	47.76(9.79 - 166.17)	W = 242, r = .016, p = .92
Final balance ($)	27.37 (0 - 100.35)	30.00 (0 - 104.83)	W = 236, r = .035, p = .82
Total bet by 5 minutes ($)	18.40 (1.45 - 47.27)	12.56 (1.27 - 47.00)	W = 285, r = .12, p = .42

Table 1: Demographic and session-level variables. Continuous data violated the assumption of normality, so summary statistics are median and range, and Wilcoxon rank-sum tests were used to test for group differences. Three participants in experiment 1a and one participant in experiment 1b did not provide their age, and so are excluded from the age analysis. For the

session-level variables, we excluded participants who had accidentally cashed out (two participants in experiment 1b) and participants who chose to stop gambling prior to the light flashing (one participant from each experiment). PGSI = problem gambling severity index, $ = Canadian dollar.

	Beta	95% CI	p value
After a loss			
Exp1a: Cash vs credit			
Log loss streak $_{(CASH)}$	-0.056	-0.072, -0.039	**< .001**
Log loss streak * group	0.043	0.019, 0.068	**< .001**
Log loss streak $_{(CREDIT)}$	-0.012	-0.031, 0.0062	.19
Machine balance ($)$_{\mathbf{(CASH)}}$	0.00013	-0.00021, 0.0018	.9
Machine balance ($)* group	0.0018	-0.00075, 0.0044	.165
Exp1b: Windfall vs earned			
Log loss streak $_{(WINDFALL)}$	-0.020	-0.037, -0.0020	**< .05**
Log loss streak * group	-0.0055	-0.032, 0.021	.676
Machine balance ($)$_{(WINDFALL)}$	-0.0031	-0.0046, -0.0015	**< .001**
Machine balance ($)* group	0.0072	0.0049, 0.0096	**< .001**
Machine balance ($)$_{(EARNED)}$	0.0041	0.0023, 0.0059	**< .001**
After a win			
Exp1a: Cash vs credit			
Log win size	0.10	0.074, 0.13	**< .001**
Log win size * group	0.0072	-0.030, 0.044	.699
Exp1b: Windfall vs earned			
Log win size$_{(WINDFALL)}$	0.16	0.13, 0.18	**< .001**
Log win size * group	-0.094	-0.13, -0.058	**< .001**
Log win size$_{(EARNED)}$	0.062	0.036, 0.089	**< .001**

Table 2: Predictors of interest in the models of spin initiation latency. Subscript text indicates in which group the effect is measured in (group 0). For predictors that are significantly modulated by group (p <.05), the model was repeated with the group order reversed, to measure the effect in group 1. Bold text indicates significant predictors. CI = confidence interval. See supplemental materials for full models, including regressors of no-interest.

	OR	95% CI	p value
After a loss			
Exp1a: Cash vs credit			
Log loss streak(CASH)	**0.92**	**0.90, 0.94**	**< .001**
Log loss streak* group	1.06	0.94, 1.19	.326
Machine balance ($)(CASH)	**1.04**	**1.03, 1.05**	**< .001**
Machine balance ($) * group	1.00	0.98, 1.01	.504
Exp1b: Windfall vs earned			
Log loss streak (WINDFALL)	0.95	0.86, 1.05	.319
Log loss streak* group	1.07	0.92, 1.25	.373
Machine balance ($) (WINDFALL)	1.00	0.99, 1.01	.836
Machine balance ($) * group	1.00	0.98, 1.00	.788
After a win			
Exp1a: Cash vs credit			
Log win size(CASH)	**1.70**	**1.36, 2.12**	**< .001**
Log win size * group	0.88	0.64, 1.21	.430
Exp1b: Windfall vs earned			
Log win size(WINDFALL)	**1.26**	**1.03, 1.54**	**< .05**
Log win size * group	1.01	0.74 1.38	.928

Table 3: Predictors of interest in the next bet models. Subscript text indicates in which group the effect is measured in (group 0). CI = confidence interval, OR = odds ratio. See supplemental materials for full models, including regressors of no-interest.

Figure 1: The pain of paying hypothesis. As the pain of paying increases, risky behaviour should decrease. A) Hypothesis 1 predicts increased gambling when participants receive the money to gamble as a voucher, compared to cash. B) Hypothesis 2 predicts decreased gambling when participants earn money to gamble, compared to a cash windfall. Image source for $5 bills: Bank of Canada.

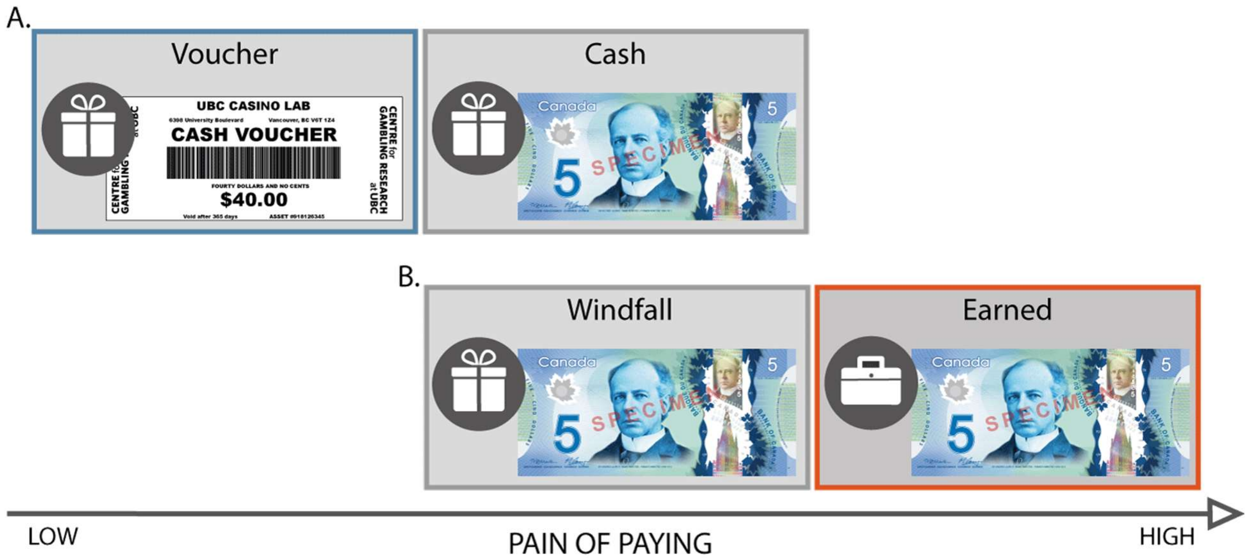


Figure 2: Trial structure for the trial-level analysis. Spin initiation latency and next bet size (in red) were analysed as a function of the current state of the machine at *, after the outcome.

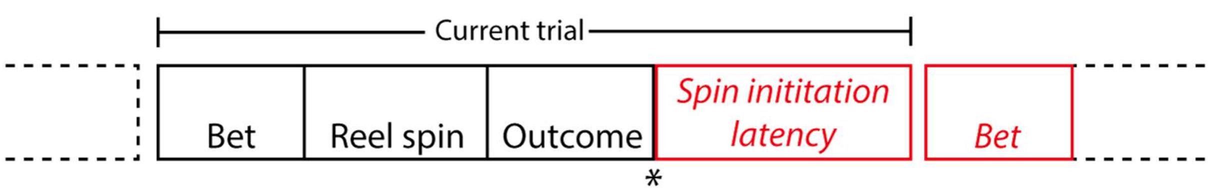


Figure 3: Observed and predicted data for the spin initiation latency models. Observed data shown using Tukey boxplots. Spin initiation latency as a function of machine balance in experiment 1a (A) and experiment 1b (B). Spin initiation latency as a function of loss streak length in experiment 1a (C) and experiment 1b (D). Spin initiation latency as a function of the size of a win in experiment 1a (E) and experiment 1b (F).

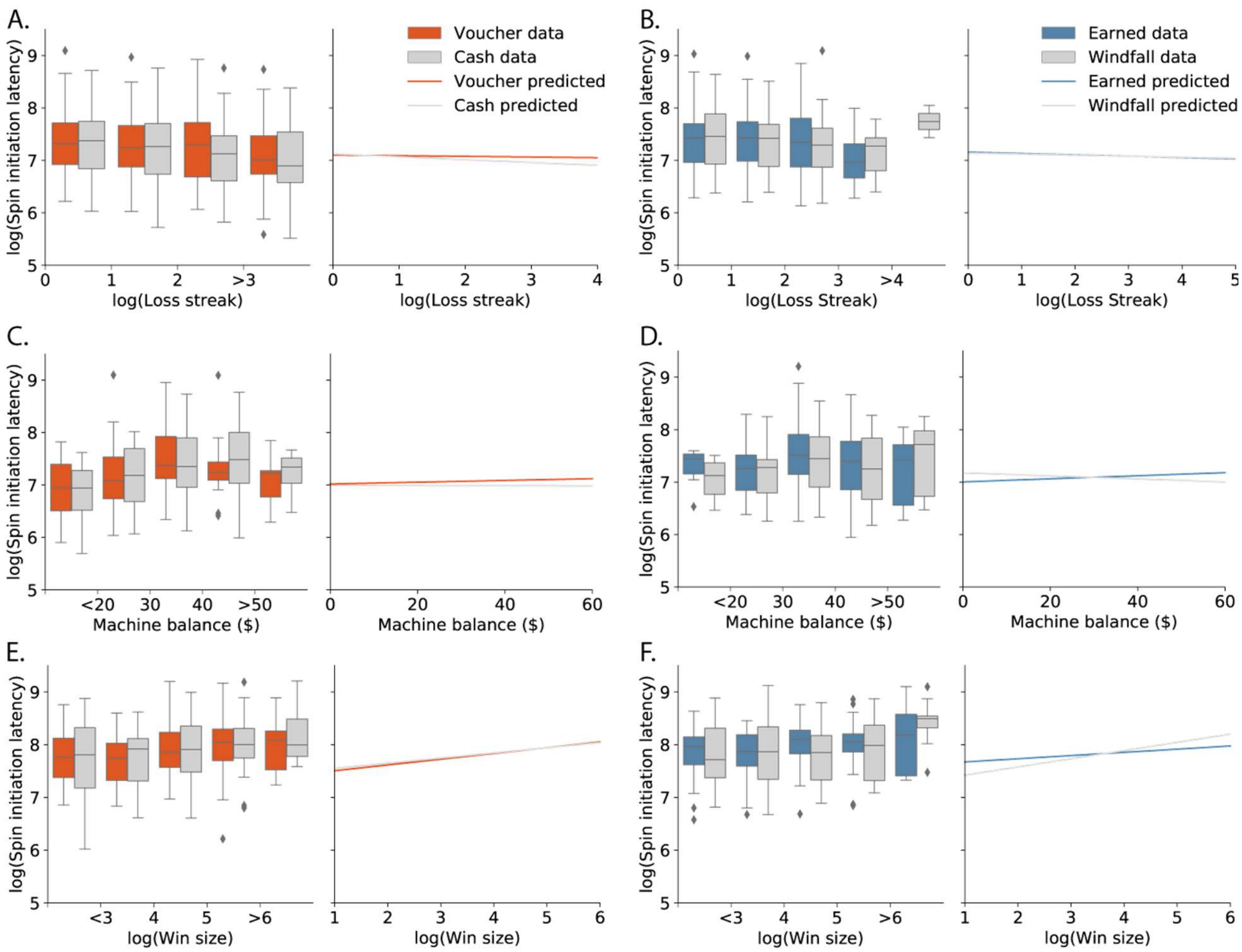
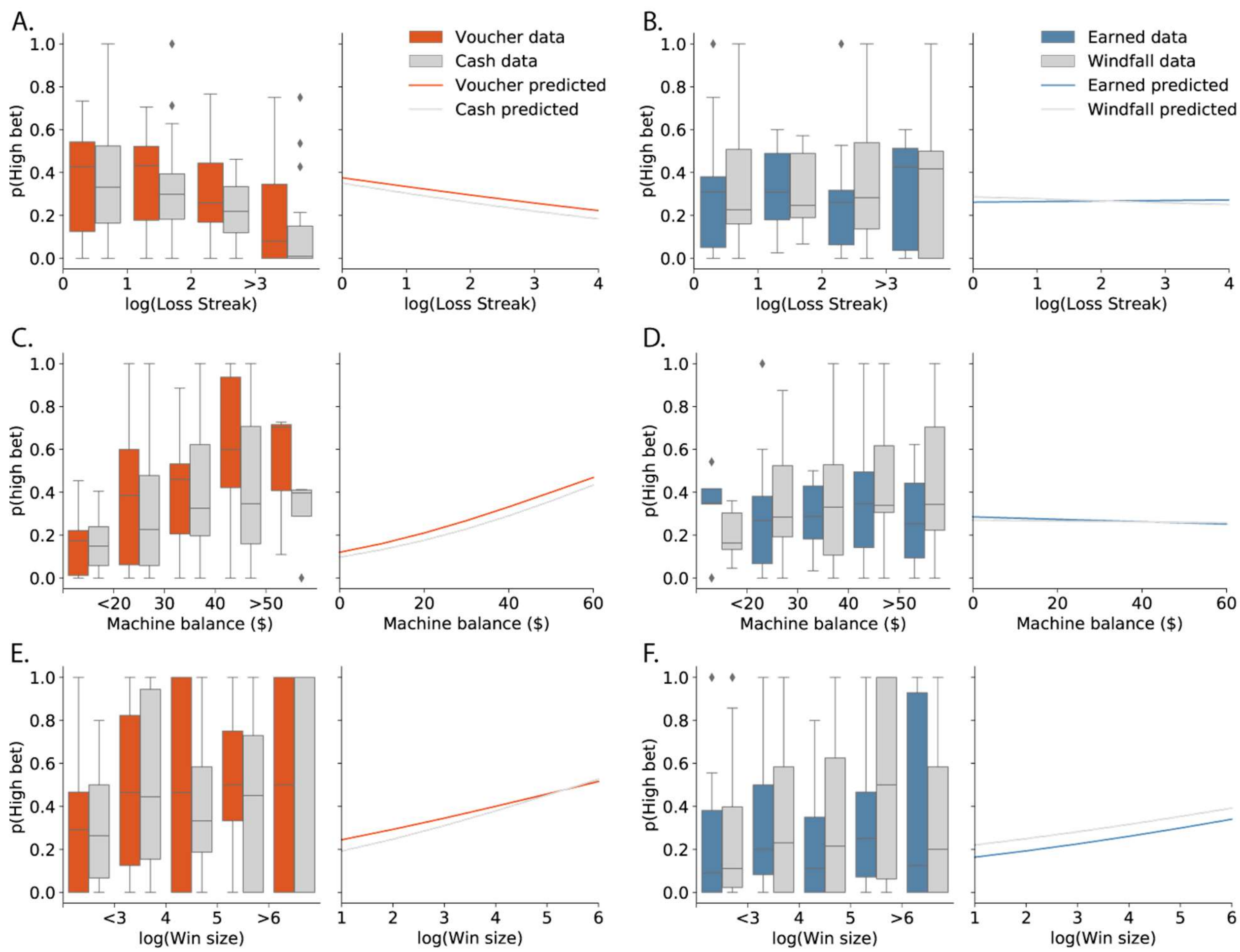


Figure 4: Observed and predicted data for the next bet size models. Observed data shown using Tukey boxplots. Probability of the next bet being higher than the participants median bet as a function of machine balance in experiment 1a (A) and experiment 1b (B). Probability of the next bet being higher than the participants median bet as a function of loss streak length in experiment 1a (C) and experiment 1b (D). Probability of the next bet being higher than the participants median bet as a function of the size of a win in experiment 1a (E) and experiment 1b (F).



Supplementary Material for Limbrick-Oldfield et al “Cashless casinos and the pain of paying: testing the effects of monetary format on slot machine gambling”

S1: Modern slot machine design and the EGM settings in Experiments 1 and 2

A traditional ‘single line’ slot machine game would typically contain three ‘reels’ (rotating drums) displaying a number of symbols (e.g. different fruit). The gambler would initiate their bet by pressing a button, or pulling a lever on the side of the machine (a ‘one-armed bandit’) and this spins the reels. If three identical symbols aligned along the central horizontal ‘payline’, the gambler would win. Different symbols may be worth different payouts (e.g. 3 oranges = 25c, 3 cherries = $10). Modern multi-line slot machines, of which Great Wall II (Experiment 1) and Buffalo Spirit (Experiment 2) are both examples, differ from this description in a number of important ways. Modern games are fully computerized: the spinning reels are digitally animated, although the symbols still rely on a random number generator. This allows wins to be accompanied by more intense forms of both visual and auditory sensory feedback (Bramley and Gainsbury 2015). Following a loss, the machine is relatively quiet until the next bet is placed (some machines play background music whilst others fall silent). Lastly, the multi-line setting allows the gambler to bet simultaneously on several paylines, in addition to the central payline. Great Wall II and Buffalo Spirit both use five animated reels, and besides the horizontal lines above and below the central payline, the gambler can bet on matching symbols along other diagonal and zigzag ‘lines’. For Buffalo Spirit there were three symbol spaces per column, a;though this varies between machines. For Great Wall II, the reel arrangement ws atypical in that not all the reels contained the same number of symbol spaces. The first two reels contained 2 symbols each, and the remaining three reels had four symbol spaces. Betting on multiple lines necessarily increases the bet size, e.g. betting 1 cent per line on 40 lines on Buffalo Spirit equates to a 40 cent bet on each spin. This creates a type of outcome termed a 'loss disguised as win' (Dixon et al. 2010) where the gambler may receive a win on one or more lines, but these wins still reflect a net loss given the elevated wager. For the Win models in our trial-level analyses, we look at behaviour following all non-zero outcomes, i.e. both 'losses disguised as wins' and 'true wins', entering the size of that win ('Win Size') as a predictor.

Specifically, Great Wall II is a multi-line slot machine set to a 1 cent denomination (i.e. the minimum bet), a Return-to-Player of 89%, and up to 60 paylines. Great Wall II is somewhat atypical in that each credit wagered was worth two lines. Our participants could vary their betting style across the five line options (2, 10, 40, 50, 60) and five bet multiplier options (1 to 5). Therefore, the maximum bet was $1.50 (5 credits per line on 60 lines). In Experiment 2, Buffalo Spirit was also set at a 1 cent denomination and 89% return-to-player, with up to 40 paylines. Buffalo Spirit uses the more conventional 1:1 ratio between credits wagered and number of lines, and participants could vary their betting style across the five line options (1, 5, 9, 20, 40) and five bet multiplier options (1 to 5), such that the maximum bet was $2.00.

S2: Slot machine data extraction and session-level analysis

For both experiments, the gambling session was recorded by splitting the video output from the slot machine's internal computer. This allowed the video to be displayed (unchanged) on the slot machine screen, and captured on a separate computer for subsequent analysis of game events, at high temporal resolution. The duplicate video signal passed via AV.IO Video Grabber (Epiphan, Palo Alto, CA) to a video capture computer running Debut 3.01 recording software (NCH, Greenwood Village, CO) at 60Hz. Events from this video stream were extracted using custom python scripts (PySlotsTracker, available https://github.com/CGR-UBC/PySlotsTracker). Using openCV, each frame was scanned for information that indicated the current machine balance, whether the reels were spinning, and whether the machine was ready to accept the next bet. From this time series, we could derive the onset and amount of each bet, the duration of the reel spin, and the outcome size and outcome duration. Both Great Wall II and Buffalo Spirit included 'free spin' bonus rounds (Chapman et al. 2019), which were coded by our extraction scripts. Free spins within a bonus occurred automatically i.e. without the gambler pressing the spin button. These bonus rounds were rare overall, and some participants did not encounter any such events, and so we excluded bonus rounds from all analyses.

Both experiments entailed a 'fixed period' of gambling (10 minutes in Experiment 1, 5 minutes in Experiment 2) followed by a window where the participant could stop gambling at any point (20 minutes and 25 minutes respectively). For the session-level analysis, the summary variables were 1) mean bet size, 2) total bet amount across the whole session, 3) machine balance at the end of the session, 4) total bet amount in the initial five minutes. The logic behind the fixed and optional periods, and our aim with defining these variables, was to capture behavioural measures of risk-taking and persistence, which may be regarded as distinct components of 'gambling intensity'. We reasoned that monetary format and the 'pain of paying' could influence a gambler's willingness to continue playing (e.g. until funds are exhausted) *or* their willingness to place high bets. At the same time, these constructs are somewhat antagonistic, because a gambler who increases their bet size is likely to deplete their available funds more rapidly, leading to less persistence (Browne et al. 2015). Ultimately, our ability to separate risk-taking from persistence was overshadowed by the substantial variability in session outcomes that arises from using authentic slot machines (see Discussion). For example, some participants encountered sustained losses early in their session, which restricted both their bet volume and bet size. We selected the total bet amount in the initial five minutes of the session in order to minimize these cumulative influences of game outcomes, which varied between participants. This same issue motivated our decision to shorten the fixed period from 10 minutes to 5 minutes in Experiment 2, as some participants in Experiment 1 were close to exhausting their funds after 10 minutes.

S3: Trial-level analysis methods

A key issue for the trial-level analysis is that naturalistic slot machine data are inherently unbalanced: participants experience varying numbers of each event type (e.g. wins), and not every participant experiences every condition (e.g. large wins). For example, if a participant consistently loses across the session, and never exceeds their initial balance, they would not contribute to estimates of the effect of balance when it was greater than the initial endowment. In rare cases where a participant's profit on the slot machine exceeded $100, we remove these trials from the analysis given the cap on the bonus payment. For the bet models, participants who did not vary their bet amount within the subset of trials included in the model were removed, and therefore, the bet models contained fewer trials than the spin initiation models (see Tables S1 and S2).

In considering win magnitude and losing streak length, these continuous variables were skewed such that high magnitude wins and long losing streaks were inherently rare events. We used two methods to address the positive skew of the data. First, we transformed the linear predictors to reduce the influence of the rare events in the model. Second, we employed robust regression (robust package for R, https://cran.r-project.org/web/packages/robust/robust.pdf) to reduce the impact of any deviations from normality. Robust regression applies a weight to each data point, dependent upon how much influence it has in a standard regression model. The regression is then re-run using the generated weights to specify how much importance the data point should be given in the model. Therefore, a few extreme data points, which may otherwise exert undue influence and contribute spurious effects, are de-weighted or even removed. In the case where violations from normality have not affected the model, robust regression and standard regression give similar results.

S4: Differences between the pre-registered and reported analysis plan in Experiment 2.

The analysis plan for Experiment 2 was pre-registered based on preliminary analysis of Experiment 1. The pre-registration document is available here: https://aspredicted.org/blind.php?x=nr6zy3. *[Link to be replaced with public version prior to publication]*. Further analysis of Experiment 1 necessitated a number of changes to the final analysis plan:

As-predicted section 6 (outlier removal)

Pre-registered plan: for the spin initiation latencies, we will remove outliers using the method described in (Van Selst and Jolicoeur 1994).

Reported: The van Selst & Jolicoeur technique requires that trials are separated into conditions (bins): outliers are removed from each bin independently, with the removal criteria dependent on the number of trials within each bin. Separating into bins by condition ensures that effects of interest are not accidentally removed. However, this variable was ultimately quite different in our data, compared to the type of experimentally-controlled reaction time data that the van Selst

& Jolicoeur method was devised for. The number of datapoints per bin varied according to both participant behaviour and the random outcomes of the slot machines, so that some bins were too small for an outlier to be detected reliably - there would be systematic variability in the rate of false positives and false negatives, dependent on participant behaviour. We resorted to a simpler, unbiased method of removing any trials with a spin initiation latency over 10s. This was a balance between removing genuine outliers, and trying not to remove any effects of interest, such as a long pause following a large payout. Numbers of excluded trials are displayed in Tables S1 and S2. We also ran all models using the van Selst & Jolicoeur exclusion procedure in the pre-registered plan, and these results were not qualitatively different, although we do not report these models as with hindsight that approach is ill-suited to quasi-experimental data.

As-predicted section 5 (pre-registered statistical analysis):

Pre-registered plan: We proposed to include scores on the Problem Gambling Severity Index (PGSI), a post-game immersive rating (the GEQ-flow scale), and a loss aversion score derived from a separate behavioural economics task, in our models.

Reported: We chose not to include PGSI score or immersion ratings in our models. Following inspection of the data and observation of both skew and range-restriction, we were concerned that these predictors would inflate the risk of false positives driven by relatively few data points. Preliminary analyses of the loss aversion task highlighted a number of possible summary variables from this task, as well as issues with model assumptions (see Ke Zhang, unpublished MA thesis from University of British Columbia). Due to the already complex nature of our analysis and results in the current study, we elected to not test further interactions with loss aversion.

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Supplemental Tables

Table S1: Number of participants and trials included in each reported model for Experiment 1.

Dependent Variable	Model	Cash N	Voucher N	N trials included (removed)	Trials per P (median and range)
Spin initiation latency	Loss	31	30	9098 (184)	147 (5 – 465)
	Win	31	29	1358 (47)	19 (1 – 81)
Bet size	Loss	25	24	7814	148 (22 – 466)
	Win	21	20	927	19 (2 – 60)

Table S2: Number of participants and trials included in each reported model for Experiment 2.

Dependent Variable	Model	Windfall N	Earned N	N trials included (removed)	Trial per p (median and range)
Spin initiation latency	Loss	28	20	6093 (85)	90.5 (10 – 364)
	Win	28	18	1136 (63)	18 (3-70)
Bet size	Loss	20	13	4424	91 (22 – 367)
	Win	19	11	702	18 (4 – 71)

In Tables S1 and S2, we report the number of participants included, the number of trials included and excluded, and the median (and range) of trials per participant in each on the trial-level models. Spin initiation latencies were excluded from those models if the latency was over 10 seconds.

Table S3: Experiment 1, Loss model, d.v. = Spin Initiation Latencies.

	Beta	95% CI (lower)	95% CI (upper)	p value
Bet Switch	0.50	0.46	0.54	< .001
Trial Number (sqrt)	-0.057	-0.061	-0.053	< .001
Machine Balance	-0.00013	0.0021	0.0018	.90
Loss Streak Length (log)	-0.056	-0.072	-0.039	< .001
Machine Balance * Group	0.0018	-0.00075	0.0044	.16
Loss Streak (log) * Group	0.043	0.019	0.068	< .01
Loss $\text{Streak}_{\text{VOUCHER}}$	-0.012	-0.031	0.0062	.19

Machine Balance was coded in $.

Table S4: Experiment 1, Win model, d.v. = Spin Initiation Latencies.

	Beta	95% CI (lower)	95% CI (upper)	p value
Trial Number (sqrt)	-0.025	-0.033	-0.017	< .05
Bet Switch	0.18	0.1	0.26	< .05
Win Size (log)	0.1	0.074	0.13	< .05
Machine Balance	-0.0019	-0.0046	0.00083	0.175

Win Size (log) * Group	0.0072	-0.03	0.044	0.699

Win size was coded in cents for all non-zero outcomes. Machine Balance was coded in $.

Table S5: Experiment 1, Loss model, d.v. = bet size (logistic)

	Odds ratio	95% CI (lower)	95% CI (upper)	p value
Trial Number (sqrt)	0.92	0.90	0.94	< .001
Machine Balance	1.04	1.03	1.05	< .001
Loss Streak Length (log)	0.74	0.68	0.80	< .001
Machine Balance * Group	1.00	0.98	1.01	0.504
Loss Streak (log) * Group	1.06	0.94	1.19	0.326

Table S6: Experiment 1, Win model, d.v. = bet size (logistic)

	Odds ratio	95% CI (lower)	95% CI (upper)	p value
Trial Number (sqrt)	0.90	0.85	0.96	< .01
Win Size (log)	1.70	1.36	2.12	< .001
Machine Balance	1.05	1.03	1.08	0.000
Win Size (log) * Group	0.88	0.64	1.21	0.430

Win size was coded in cents for all non-zero outcomes. Machine Balance was coded in $.

Table S7: Experiment 2, Loss model, d.v. = Spin Initiation Latencies.

	Beta	95% CI (lower)	95% CI (upper)	p value
Trial Number (sqrt)	-0.035	-0.039	-0.032	< .001
Bet Switch	0.54	0.51	0.58	< .001
Machine Balance	-0.0031	-0.0046	-0.0015	< .001
Loss Streak Length (log)	-0.020	-0.037	-0.0020	< .05
Machine Balance * Group	0.0072	0.0049	0.0096	< .001
Loss Streak (log) * Group	-0.0055	-0.032	0.021	.676
Machine Balance$_{WINDFALL}$	0.0041	0.0023	0.0059	<.001

Table S8: Experiment 2, Win model, d.v. = Spin Initiation Latencies.

	Beta	95% CI (lower)	95% CI (upper)	p value
Trial Number (sqrt)	-0.021	-0.028	-0.014	< .001
Bet Switch	0.41	0.32	0.50	< .001
Win Size (log)	0.16	0.13	0.18	< .001
Machine Balance	0.00099	-0.0010	0.0030	0.34
Win Size (log) * Group	-0.094	-0.13	-0.058	< .001
Win Size$_{WINDFALL}$	0.062	0.036	0.089	< .001

Table S9: Experiment 2, Loss model, d.v. = bet size (logistic)

	Odds ratio	95% CI (lower)	95% CI (upper)	p value
Trial Number (sqrt)	0.92	0.90	0.94	<.001
Machine Balance	1.00	0.99	1.01	.83
Loss Streak (log)	0.95	0.86	1.05	.32

Machine Balance * Group	1.00	0.98	1.00	.79
Loss Streak (log) * Group	1.07	0.92	1.25	.37

Table S10: Experiment 2, Win model, d.v. = bet size (logistic)

	Odds ratio	95% CI (lower)	95% CI (upper)	p value
Trial Number (sqrt)	0.82	0.76	0.89	< .001
Win Size (log)	1.26	1.03	1.54	< .05
Machine Balance	0.98	0.96	1.00	< .05
Win Size (log) * Group	1.01	0.74	1.38	.93