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Encouraging and evaluating limit-setting among on-line gamblers: a naturalistic randomized controlled trial

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ABSTRACT

Aims We tested the effectiveness of three different messages designed to increase limit-setting on gambling sites and sent these via e-mail or in-account notification to compare delivery modes. As a secondary aim, we examined the effects of limit-setting on gambling behaviour. **Design** A pre-registered, naturalistic randomized control trial using a 3 × 2 plus control design. **Setting** Four on-line Australian sports and racing wagering websites. **Participants** A total of 31 989 wagering customers (reduced to 26,560 after eligibility screening) who had placed bets on at least 5 days in the past 30 [mean age = 41.4, standard deviation (SD) = 14.3; 79% male]. **Interventions and comparators** Messages were sent via e-mail or in-account notification by on-line gambling operators and were designed to either: (1) be informative, describing the availability and purpose of the tool (informative messages), (2) highlight the benefits other people receive from using the tool (social messages) or (3) promote the benefit individuals could receive from using the tool (personal messages). A control group who did not receive messages was monitored for comparison. **Measurements** Our primary outcome was the number of customers who set a deposit limit within 5 days of receiving messages and secondary outcomes included pre- and post-message betting behaviour (e.g. average daily wager). **Findings** One hundred and sixty-one (0.71%) customers sent messages set limits compared to three (0.08%) controls [adjusted odds ratio (aOR) = 8.17, 95% confidence interval (CI) = 2.99, 33.76)]. Social and personal messages were no more effective than informative messages (aOR = 0.98, 95% CI = 0.65, 1.48; aOR = 0.93, 95% CI = 0.60, 1.44) and in-account messages were no more effective than e-mails (aOR = 1.02, 95% CI = 0.71, 1.49). Customers who set limits significantly decreased their average daily wager, the SD of daily wager, net loss and betting intensity compared with non-limit-setters. **Conclusions** Messages to on-line gambling website customers are inexpensive, and may lead to small but impactful increases in setting deposit limits. Limit-setting may be an effective strategy for reducing gambling expenditure and intensity.

Keywords Consumer protection tools, gambling, message, on-line gambling, pre-commitment, responsible, sports betting, wagering.

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INTRODUCTION

Individuals can access consumer protection tools (CPTs) or ‘responsible gambling tools’ on many on-line gambling websites to help them reduce gambling harms, including the ability to set limits on how much they can deposit into their account (‘deposit limits’), access to ‘activity statements’ outlining expenditure and the ability to temporarily block accounts using ‘time-outs’ [1,2]. Surveys of players who have used CPTs in Australia [1], Sweden [2], Finland [3] and Norway [4] have found the tools to be mainly positively perceived. However, rigorous empirical evaluations of how CPT use affects actual gambling behaviour, particularly over time, are lacking. Deposit limits have been associated with reductions in betting days, total bets, the overall amount wagered [5] and, in the most ‘intense’ players, reduced gambling intensity [6] and expenditure [7]. In contrast, studies have found no effect of limit-setting on average bet size [5] or on net loss [8]. However, these studies have predominantly focused on single European operators [6–8] or are now outdated in the current on-line context [5], limiting the extent to which their findings can be extrapolated to customers across international on-line gambling sites.

Despite their positive perception and harm-reducing potential, voluntary engagement with deposit limits is low and several countries (e.g. France, Norway and Belgium) [4] have implemented mandatory limits, or make customers choose their deposit limit before they can gamble (e.g. Italy) [9]. In Australia, only 24.5% of 564 on-line wagering customers surveyed [1] had ever used deposit limits and 8.1% had used time-outs. The UK Gambling Commission found only 9% of 6425 gamblers surveyed reported using limit-setting [10].

Procter *et al*. [11] explored whether the Theory of Planned Behaviour (TPB) [12,13] could explain CPT use among Australian on-line wagerers. Consistent with the TPB, the authors found that positive attitudes and subjective norms, along with previous CPT use (but not perceived behavioural control), were positively correlated with the intention to use CPTs, which subsequently predicted actual tool use.

Two trials of messages to encourage deposit limit uptake among UK on-line gambling sites conducted by the behavioural insights team (BIT) found that differences in message content (e.g. self-reflection content versus normative feedback) had little impact [14]. The only factor associated with significant increases in limit use was reduced friction (i.e. fewer number of clicks/steps required to access the tools from the message). The pop-up messages sent in the second trial resulted in larger overall increases in limit-setting across all conditions (mean = 9.2%) compared with the e-mail messages in the first trial (mean = 7.8%). In-account pop-up messages may be more effective, as they were directly presented to participants on-site without needing to open messages in their e-mail account (i.e. reduced friction).

Using the knowledge gained from preliminary research on CPT use, the aim of the present trial was to explore the value of different message types for increasing deposit limit uptake among customers from four wagering sites in Australia and to investigate the impact of limit-setting on gambling behaviour. We focused on promoting deposit limits, given their relatively low rates of use [1,10] and potential relevance to a wide customer base when compared with more restrictive CPTs (e.g. time-outs, self-exclusion). Based on Procter and colleagues' [11] finding that CPT use was motivated by positive attitudes towards tools and subjective norms (i.e. the sense that others use and think favourably of them), we compared the effectiveness of messages that (1) highlighted the benefits to the individual of setting a deposit limit [e.g. managing spending ('personal message') and (2) highlighted the positive social perception of the tool [i.e. others use and like deposit limits ('social message')], as well as (3) a message including a description of deposit limits ('informative message') for comparison with the theoretically informed messages. In our pre-registration we outlined three hypotheses:

- [H1] On-line wagerers in the messaging conditions will be more likely than controls (i.e. those who do not receive a message) to set a deposit limit within 5 days of messaging (the same time-period used by BIT, 2018).
- [H2] On-line wagerers who receive social messages will be most likely to set a deposit limit within 5 days of messaging,[1] followed by those who receive personal messages and then those who receive informative messages.
- [H3] Messages delivered via in-account notification will be more effective at increasing deposit limit uptake than those delivered via e-mail (due to reduced friction).

We anticipated low overall rates of limit-setting in response to messages, as all on-line gambling sites in Australia were required in May 2019 (5 months before the start of the trial) to make their customers set a deposit limit or opt-out of setting one. Thus, individuals involved in this trial had already opted-out of setting a limit. Based on the existing literature, we pre-registered tentative predictions regarding the effects of limits on wagering behaviour: 'wagerers who set deposit limits will reduce the amount wagered and the duration[2] and frequency of wagering subsequent to limit-setting (compared with baseline data), but no effect on net loss will be observed'.

METHODS

Registration and transparency

A study protocol was preregistered on Open Science Framework (OSF) prior to commencing the trial: https://osf.io/6dpkw/. Unless otherwise stated, we followed the plan described in our pre-registration. All deviations are reported in a transparent changes document shared on OSF and deviations affecting confirmatory analyses are reported here.

Design

A pragmatic or naturalistic randomized control trial was used [16] wherein customers were unaware that the messages they received were part of a trial. This allowed

[1]Social messages were more positively rated overall than personal messages by gambling customers involved in pilot-testing the messages, hence our prediction that these messages will better encourage limit-setting in comparison to personal messages. Thus, H^2 is based on a combination of preliminary theory application (TRA) with descriptive work [11] and pilot-testing—all strategies recommended for hypothesis development [15].

[2]As stated in our transparent changes document (https://osf.io/6dpkw/), operators were unable to provide data relating to time spent gambling and therefore we were unable to investigate the effect of limit-setting on this variable.

us to evaluate the effectiveness of the messages in a setting with high ecological validity [17]. Customers from four Australian on-line sports and racing wagering sites were randomly allocated by their respective operator to one of seven possible conditions, including a control group of customers who did not receive messages for comparison. A factorial approach was used whereby the intervention groups were divided according two variables resulting in six conditions: delivery method (two levels: e-mail and in-account delivery) and message content (three levels: informative, personal, and social). Thus, overall, a 3 x 2 plus control design was used.

Development and pilot-testing of customer messages

We initially developed six social and six personal messages based on the findings of existing research on responsible gambling messaging (see protocol for a discussion of this literature). These were circulated to external researchers for evaluation. Next, messages were subjected to consumer evaluations via an on-line market research platform operated by a gambling site participating in the trial.[3] Customers were asked to what extent each message would encourage them to set a deposit limit, rating their agreement along a seven-point Likert scale. Tag-lines that accompanied messages and subject-lines for e-mail messages were developed and evaluated using the same methods. Full details of this pilot work are reported on OSF.

Based on the outcomes from polls, the social message selected was: 'Most people who use deposit limits find this helps them manage their spending', and the personal message was: 'Deposit limits are a great way to manage your spending'. The tag-line that accompanied both messages was: 'Keep on track with deposit limits'. The e-mail subject-lines selected were: 'Keep on track' (used for the first e-mail message sent to customers) and: 'Set and forget' (used for the second message). The informative message used was: 'Customers are able to set a personal limit on the amount of money deposited into their gambling account using the Deposit Limits tool, for a 24-hour, weekly, 2-weekly, or monthly period' (no tag-line accompanied this message). This was modelled on standard operator communications regarding CPTs.

Study sample

Participants were account holders of Australian sports and race wagering websites. We requested that customers randomly selected by operators would not currently be using the self-exclusion or time-out tools, and not already have a limit set. In order to target regular and currently active customers we requested all had placed bets on ≥5 days in the 30 preceding the trial. We asked that customers held an account for ≥90 days to allow for a sufficient period of gambling history to compare pre–post-message changes in gambling and factors predicting limit-setting.

Sample size

We used G*Power software [18] to perform power analyses for two-tailed logistic regressions to test our hypotheses.[4] Based on limit-setting rates of 3.3–4.4% in BIT's trial [14], we estimated a lower overall rate of limit-setting among the messaging groups in our study (due to already having opted-out) under H1 of 2.5% and a rate of 0.5% in the control group to account for the occurrence of unprompted, naturally occurring limit-setting. Assuming these rates, a power analysis for a logistic regression [$\alpha = 0.05$, $\beta = 0.095$, π (i.e. proportion in intervention groups) = 0.856] estimated a total required sample size of 4768. With the overall limit-setting rate predicted at 2.5%, we estimated rates of 3.5% for social messages, 2.5% for personal messages and 1.5% for informative messages. With informative messages as the reference group, a second power analysis for H2 with the same α and β values ($\pi = 0.52$ for social versus informative groups and $\pi = 0.498$ for personal versus informative) indicated that the required sample size would need to be at least 10 156. Assuming a smaller overall effect for delivery method (predicted rates of 3% for in-account messages and 2% for e-mail messages) based on BIT's [14] findings, a third power analysis for testing H3 (the same α and β, $\pi = 0.481$) estimated a total required sample size of 12 642.

While the minimum sample size required to test our hypotheses was 12 642, we aimed to maximize our sample size to most clearly understand the impact of messages. Initially, five operators were scheduled to take part in the trial (four involving 10 000 customers and one 2000), although one operator unexpectedly dropped out of the trial, which reduced the sample size from the targeted 42 000 to 31 989.

Procedures

The processes of participant recruitment, allocation and screening for eligibility criteria are presented in Fig. 1. Randomization was performed by a data scientist within each gambling company using a procedure whereby, after

[3] All those who participated in the polls used to pilot-test messages were excluded from the trial.

[4] As recommended by one of the editors at *Addiction*, we changed the statistical tests of our hypotheses from χ^2 tests to logistic regressions and re-computed our power analyses accordingly (for the full updated power analysis protocols and outcomes, see: https://osf.io/u38e9/). In our pre-registration we described an a priori power analysis [$\alpha = 0.05$, $\beta = 0.05$, degrees of freedom (d.f.) = 6] for a χ^2 analysis comparing all seven groups' limit-setting rate, with a small effect size ($\varphi = 0.1$) which indicated that the minimum sample size required was 2086. All post-peer review changes to analyses are clearly identified in our analysis scripts and documented in a transparent change document (https://osf.io/6dpkw/).

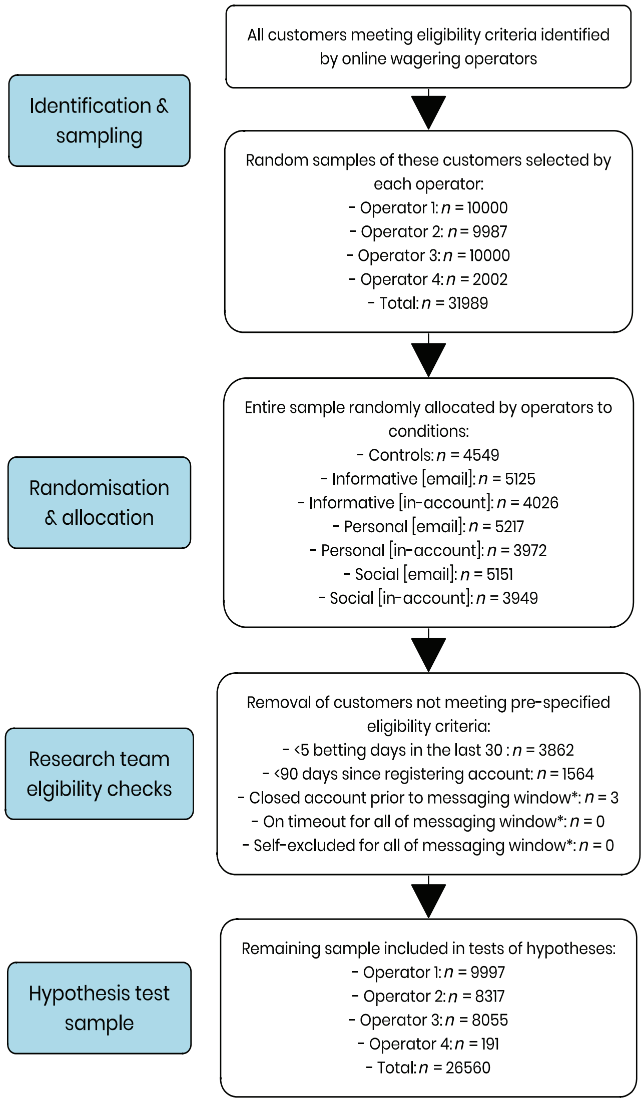

Figure 1 Flow-chart of participant recruitment and selection. This Consolidated Standards of Reporting Trials (CONSORT) flow-chart presents the process of identifying, selecting and allocating customers for involvement in the trial, as well as the subsequent removal of those not meeting study eligibility criteria. We sequentially removed ineligible participants in the order presented, but some customers did not meet multiple eligibility criteria (e.g. they held an account for < 90 days and bet on < 5 days). *Messaging window defined as the duration between the first message being delivered and 5 days after the second message was delivered (i.e. 14 October 2019–23 October 2019)

all customers meeting eligibility criteria were listed within a data set, each was a assigned a randomly generated number and re-ordered according to this number against a fixed column containing condition numbers [19]. We aimed to allocate customers to conditions with a ratio of 1 : 1 : 1 : 1 : 1 : 1 : 1, although we had to vary the specific ratio and total number of customers given to each operator as two were not able to send in-account messages and one (operator 4) was smaller and included smaller cohort of customers (see protocol for full allocation ratio for each operator: https://osf.io/gxbke). However, the operator who left the trial was one of three able to send in-account messages, so this changed the final allocation ratio to 1 : 1.11 : 0.86 : 1.11 : 0.86 : 1.11 : 0.86 (conditions ordered with respect to their position in Table 1), resulting in fewer participants in the in-account conditions.

Operators were responsible for identifying customers meeting the pre-specified eligibility criteria before randomization; however, as seen in Fig. 1, many customers randomly allocated to conditions (particularly those by operator 4) did not meet our criteria. We confirmed with the operators that customers were randomized according to our protocol and those in the messaging conditions received messages as planned. Operators 2–4 confirmed that the inclusion of ineligible customers resulted from errors in their selection process.[5]

Participants allocated to the intervention groups were sent the message corresponding to their condition on 14 October 2019, and again on 19 October 2019 if they did not set a deposit limit between these dates (both messages contained the same content but e-mail messages used a different subject-line). We requested account data for all customers for 90 days prior to the first message

[5]Namely, accidentally selecting customers with five bets in the past 30, not 5, betting days and/or failing to check sufficient registration time.

Table 1 Sample characteristics overall and by condition.

		Condition						
Variable	*Overall, N = 26,560*	*Controls, n = 3817*	*Informative (e), n = 3911*	*Informative (i–a), n = 3679*	*Social (e), n = 3987*	*Social (i–a), n = 3651*	*Personal (e), n = 3895*	*Personal (i–a), n = 3620*
Age	41.4 (14.3)	41.6 (14.3)	40.4 (14.1)	42.2 (14.5)	40.8 (14.0)	42.2 (14.7)	40.7 (13.9)	42.0 (14.5)
Gender								
Female	2123 (8.0%)	327 (8.6%)	312 (8.0%)	263 (7.1%)	378 (9.5%)	269 (7.4%)	320 (8.2%)	254 (7.0%)
Male	21 079 (79%)	3043 (80%)	3167 (81%)	2904 (79%)	3141 (79%)	2845 (78%)	3134 (80%)	2845 (79%)
Unknown	3358 (13%)	447 (12%)	432 (11%)	512 (14%)	468 (12%)	537 (15%)	441 (11%)	521 (14%)
Previous limit set	635 (2.4%)	57 (1.5%)	126 (3.2%)	72 (2.0%)	122 (3.1%)	92 (2.5%)	113 (2.9%)	53 (1.5%)
Previous 90 days gambling								
Average daily wager	164.3 (956.8)	165.1 (1123.7)	128.2 (644.0)	198.8 (904.2)	111.6 (401.7)	252.4 (1785.3)	112.5 (458.3)	192.5 (686.8)
SD of daily wager	187.9 (1 300.0)	185.2 (1 358.2)	147.6 (754.3)	236.7 (1358.2)	123.2 (442.8)	299.3 (2586.1)	127.6 (528.2)	208.8 (824.9)
Net loss	1745.2 (18 024.7)	1418.3 (14 435.4)	1156.2 (11291.0)	2420.3 (15 253.1)	816.1 (5477.2)	3554.3 (39 170.7)	675.9 (5161.1)	2389.4 (13 203.9)
Betting frequency	25.7 (16.4)	25.3 (16.2)	26.6 (16.9)	25.0 (15.8)	26.4 (17.0)	25.2 (16.1)	25.7 (16.8)	25.2 (16.1)
Betting intensity	7.6 (15.1)	7.6 (12.1)	7.4 (12.9)	8.0 (13.1)	7.3 (26.1)	8.2 (12.4)	7.2 (12.3)	7.9 (10.2)

Statistics presented: continuous variables = mean [standard deviation (SD)]; categorical variables = *n* (%); (e) = e-mail delivery; (i-a) = in-account delivery. The final allocation ratio across conditions was 1 : 1.02 : 0.96 : 1.04 : 0.96 : 1.02 : 0.95, respective to position in the table, and was thus less uneven than after the initial allocation. This re-balancing can be explained by the removal of many ineligible customers from operator 4, who only offered e-mail messages.

and 90 days following the second message (i.e. 16 July 2019–17 January 2020), including account ID, age, gender, any use of CPTs (including date and characteristics such as limit amount) and details of transactions and wagers (e.g. date, stake).

Ethical approval for the trial was obtained from the University of Sydney Human Research Ethics Committee (reference: 2018/400). Formal consent was not able to be obtained from customers due to the naturalistic trial design. Customers were users of wagering sites and, by opening accounts, they provided consent to the terms and agreements of these companies, including that they may be contacted regarding CPTs and that their anonymized data may be shared by an authorized third party.

Outcome measures

We collected customer account data from operators for all those involved in the trial and used this to compute our outcome measures. The primary outcome was whether customers set a deposit limit within 5 days of receiving messages, which is consistent with the time-frame used in the 2018 BIT trial [14] enabling comparisons between studies, and is cautious in assuming that limits set after 5 days may not have been prompted by messages. A secondary outcome outlined in our protocol was message opening rates, although only three operators were able to record these data and two of these three provided only summary figures (e.g. '35% of customers in condition 1 opened message 1'). As a result, we diverged from our protocol by including all those who received messages in analyses of our primary outcome/hypotheses, regardless of whether or not they opened them. Additional secondary measures were calculated for both 90-day windows (pre-/post-messages): previous limit use (yes/no), average daily wager (money staked/number of bets), standard deviation of wager (SD of average daily wager), net loss (total money staked minus winnings), betting frequency (sum of days in the period in which at least one wager was placed) and betting intensity (sum of bets placed divided by betting frequency).

Data analysis

Data analysis was performed using R version 4.0.2 [20]. We have provided a document presenting the annotated analysis code and outputs for the entire analysis process on OSF (https://osf.io/u38e9/). We tested the study hypotheses using a series of logistic regression analyses, including the following covariates to account for baseline variations between conditions: previous limit use, average daily wager, SD of daily wager, net loss, betting frequency and betting intensity (we report unadjusted versions of these tests in our analysis script and output document on OSF). Before testing H1 and H2, we conducted a logistic regression to test whether there were any interactions between message content and delivery mode that, if present, would have necessitated separate testing of conditions at each intersection (e.g. testing the effect of delivery mode at each level of message content). No statistically significant interactions were observed. As we removed 5426 customers post-randomization for not meeting the minimum betting days and account registration criteria, we performed an ITT-style analysis [21] for our main hypothesis (H1) including these customers to see if the overall effect of messages remained in a more diverse sample.[6] As we performed six tests of our primary outcome (including the test for interactions), we adjusted α to 0.0083 using the Bonferroni correction method.[7] All non-significant outcomes from hypothesis tests are supplemented by Bayes factors (BFs) which were calculated using Bayesian logistic regressions from the *BFpack* R package [22].[8]

For exploratory analyses of how deposit limits affected wagering behaviour (e.g. average daily wager), we compared customers gambling during the 90 days pre- and 90 days post-limit, a duration that is consistent with previous studies of limit efficacy [8]. For these analyses, we filtered the hypothesis test sample by removing all those with < 14 days wagering activity either side of messages due to account closures or the use of time-out and/or self-exclusion features. We removed all customers in the non-limit-setter group who set a limit after the messaging window, leaving 153 limit-setters and 22 207 non-limit-setters. We then randomly selected 153 non-limit-setters (matched for operator) for comparison. We aimed to use analyses of covariance (ANCOVAs) with change scores (i.e. the difference between scores pre-/post-limit) as the outcome variable and baseline scores (i.e. 90-days pre-limit) as a covariate to account for regression to the mean [23]. As data for all comparisons did not meet several statistical assumptions required for traditional ANCOVA, we undertook robust versions of the test using the 'WRS2' R package [24]. Robust ANCOVAs involve the use of trimmed means and a running interval smoother, and perform well in simulated scenarios when standard

[6]Although we have labelled this an intention-to-treat (ITT) analysis it differs from a traditional form of ITT, as all those included in our analysis received an intervention (or lack of for controls) as planned. Typically, ITTs are intended to include (in analyses) all participants randomized even if they did not receive or adhere to a prescribed intervention as planned, as opposed to including those not meeting eligibility criteria, as performed here.

[7]We set α at 0.05 for exploratory analyses unless otherwise stated.

[8]The BFs calculated represent the relative evidence for the alternative (H1) over the null (H0) hypothesis and the model requires H1 to be pre-specified in relation to which variables (or levels of variables) will be more, or equally, predictive of the outcome (e.g. 'message groups > controls' = customers in messaging groups will be more likely to set limits than controls).

Table 2 Primary outcome summary: number of limit-setters per experimental condition.

Group				*Limit-setting rate*	
	Content	*Delivery mode*	*n (group)*	*n (limit-setters)*	*Percentage*
Overall					
Control			3817	3	0.08
Intervention			22 743	161	0.71
All conditions					
Intervention	Informative	E-mail	3911	23	0.59
Intervention	Informative	In-account	3679	32	0.87
Intervention	Social	E-mail	3987	25	0.63
Intervention	Social	In-account	3651	37	1.01
Intervention	Personal	E-mail	3895	18	0.46
Intervention	Personal	In-account	3620	26	0.72
Message content aggregates					
Intervention	Informative		7590	55	0.72
Intervention	Personal		7515	44	0.59
Intervention	Social		7638	62	0.81
Delivery mode aggregates					
Intervention		E-mail	11 793	66	0.56
Intervention		In-account	10 950	95	0.87

A customer was defined as a 'limit-setter' if they set a limit within 5 days of the first message or second message being sent (i.e. between 2019-10-14 and 2019-20-23) or within the same window for controls.

Table 3 Outcomes from logistic regression tests of hypotheses

	Regression outcomes				*Unadjusted ORs*			*Covariate adjusted ORs*		
Term	*B*	*SE*	*Z*	*P-value*	*OR*	$CI^{(LB)}$	$CI^{(UB)}$	*OR*	$CI^{(LB)}$	$CI^{(UB)}$
Test of H1: overall impact of messages (n = 26 560)										
Messages	2.1003	0.5977	3.5141	0.0004	9.06	3.44	36.72	8.17	2.99	33.76
ITT analysis:* overall impact of messages (n = 31 986)										
Messages	1.9124	0.5205	3.6743	0.0002	6.75	2.86	21.94	6.77	2.77	22.45
Test of H2: impact of message content (n = 22 743)										
Personal message	−0.0737	0.2238	−0.3294	0.7418	0.81	0.54	1.20	0.93	0.60	1.44
Social message	−0.0182	0.2098	−0.0869	0.9307	1.12	0.78	1.62	0.98	0.65	1.48
Test of H3: impact of delivery mode (n = 22 743)										
In-account	0.0202	0.1882	0.1074	0.9145	1.56	1.14	2.14	1.02	0.71	1.49

All logistic regression outcomes are from covariate adjusted models. ORs = odds ratios; SE = standard error; $CI^{(LB)}$ and $CI^{(UB)}$ = lower and upper 95% confidence intervals, respectively). Hypothesis 1: reference = no message; Hypothesis 2: reference = informative message; Hypothesis 3: reference = e-mail delivery. *Intention-to-treat (ITT) analysis involving all randomized participants (excluding three who closed their accounts before messages were sent) reference = no message.

ANCOVA assumptions (e.g. homogeneity of regression slopes) are not met [25]. The test involves comparing the two groups' scores at multiple 'design points' or around values of the covariate deemed to be 'comparable' (i.e. where scores are available for ≥12 participants per group).[9] To account for Type-I error risk (five robust ANCOVAs involved 25 comparisons, five per test), we adjusted α to 0.002 (Bonferroni correction).

RESULTS

Characteristics of the sample

The characteristics of the final sample used in tests of hypotheses are described in Table 1. There was little variation between conditions in relation to age and gender, although there were some notable differences in the history of limit-setting and past 90-day gambling behaviour.

[9]In robust ANCOVA, the null hypothesis is that the trimmed mean of the outcome variable (e.g. change score for average daily wager) is equal for cohorts of the two groups who have similar covariate values (e.g. average daily wager amounts for the 90-days pre-limit of $25 or $100).

Primary outcomes

Condition-level and aggregated rates of limit-setting are presented in Table 2. The opening rate data provided by three operators suggested e-mails were opened on average by 30.6% of customers (range = 22.4–38.5%). We calculated the estimated number of participants in the sample used to test hypotheses who opened e-mails using the condition-specific rates from the full, pre-screening samples for the three operators where data were available and using the combined mean rates from these operators for the fourth operator. Including only the estimated number of customers who opened e-mails raises the limit-setting rate in the e-mail conditions from 0.56 to 2.00%, and the total rate increases from 0.71 to 1.13%.

Table 3 displays the outcomes from logistic regressions testing our hypotheses. Receiving a message was strongly and significantly predictive of limit-setting, both when using the sample meeting our eligibility criteria and all customers randomized (i.e. ITT analysis). Variations in message content had little effect on limit-setting, although the Bayesian analysis indicated that the data provided inconclusive evidence for our hypothesis (BF_{10} = 0.86; specification of H2 was set as: social messages > personal messages > informative messages). Delivery mode was not predictive of limit-setting, and again the BF indicated inconclusive evidence for our

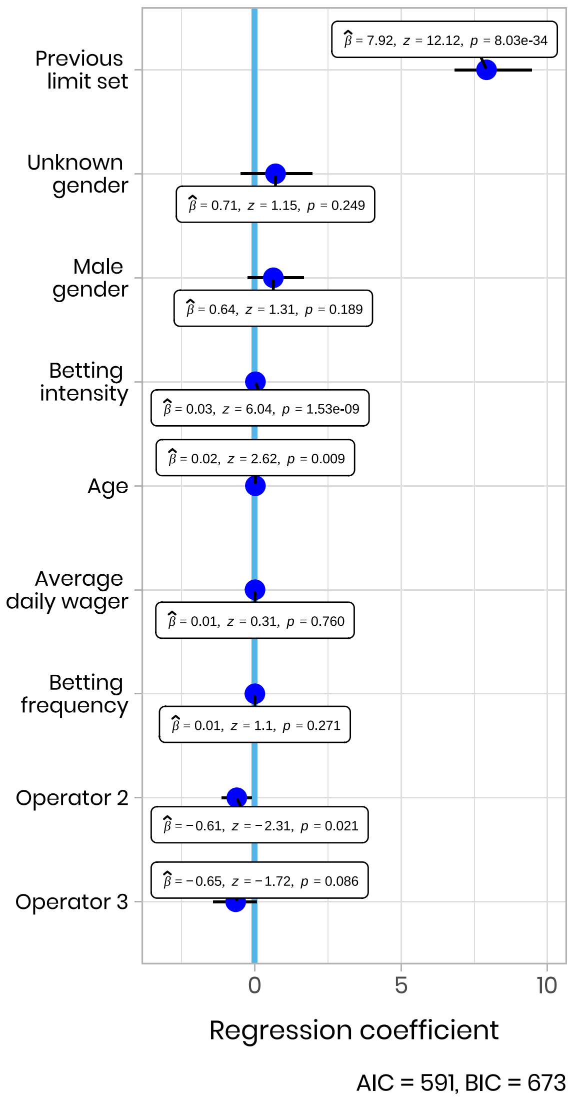

Figure 2 Predictors of limit-setting in response to messages: binomial logistic regression coefficients with 95% confidence intervals (CIs). This figure displays the regression coefficients (β) and their 95% CIs for each predictor variable in the model, as well the associated Z (i.e. β divided by its standard error) and P-values (calculated using Wald's test). The larger the regression coefficient, the more that a variable was predictive of limit-setting. For the gender-specific predictor variables, females were the reference group. For the operator-specific predictor variables, operator 1 was the reference group. AIC = Akaike information criterion; BIC = Bayesian information criterion

hypothesis (BF_{10} = 1.09; specification of H3: in-account messages > e-mail messages).

Secondary outcomes

To determine whether messages had any unintended effects, we explored whether customers who received messages subsequently used a time-out or self-excluded. No customers used a time-out in the 10-day messaging window or during the following 10 days. A total of nine customers (including two limit-setters) self-excluded in the 10-day messaging window, compared to five in the following 10 days (0 limit-setters). A logistic regression was performed to identify predictors of limit-setting in response to messages. Four variables included in the model were significant predictors (see Fig. 2. The overall model explained 61.8% of the variance in limit-setting (Nagelkerke's R^2) and its ability to predict this outcome was statistically significant compared to a null model (n = 26,319, P = 0.001).

The change in gambling expenditure and behavioural variables from pre- to post-limit (or pre-/post-message for non-limit-setters) was compared between limit-setters and non-limit-setters (see distributions of these variables in Figs 3 & 4) . Outcomes from robust ANCOVAs testing these comparisons are presented in Tables 4 and 5. The change in all three expenditure variables was statistically greater for limit-setters than non-limit-setters at low–mid design points, but not at higher design points. Thus, individuals with low–mid levels of these variables in the 90 days preceding messages (e.g. an average daily wager of ~\$6.02 to ~45.96) showed a significantly greater reduction compared to non-limit-setters with similar baseline values, whereas the change in values displayed by limit-

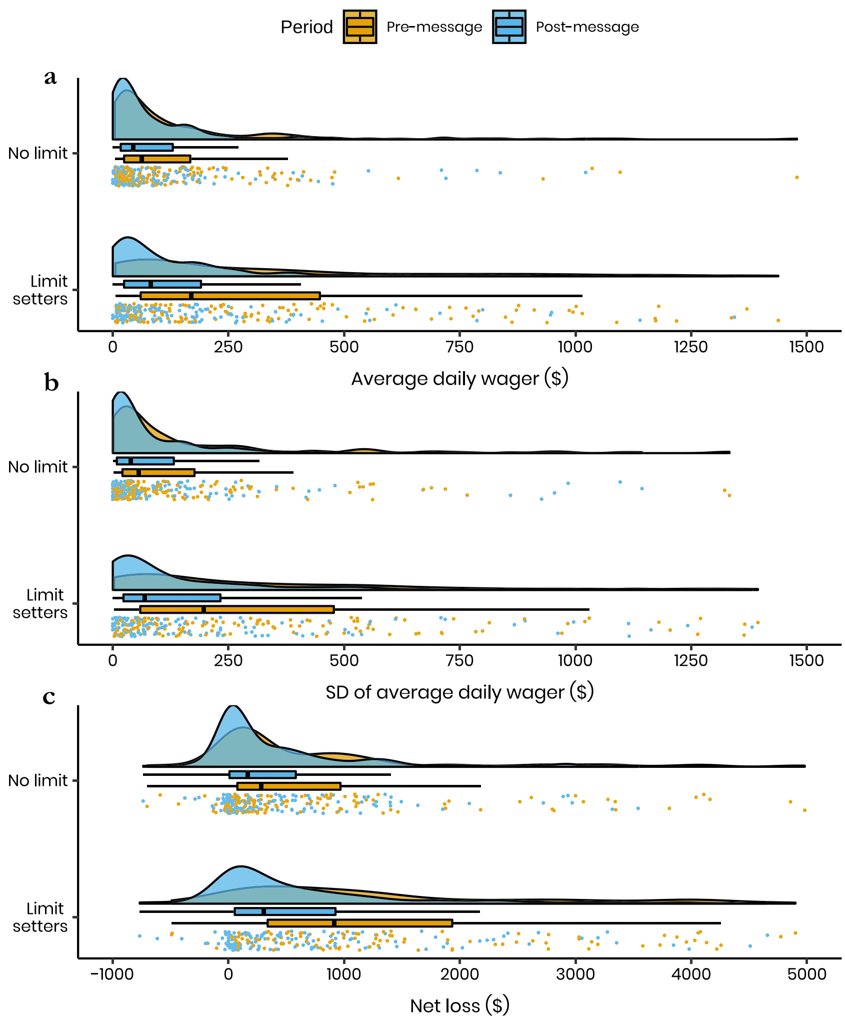

Figure 3 Effects of deposit limits on gambling expenditure (90 days before and after messages). This figure displays the distribution of customers' average daily wager amount (a), the variability [standard deviation (SD)] in average daily wager (c) and net loss (c) for the 90 days before and after receiving the first message for non-limit-setters and 90 days before and after setting their limit for the those who set a limit. The scales of Fig. 3a and 3b were capped at \$1500 and Fig. 3c was capped at \$1000 and \$5000 to enable easier visualization of outcomes. The non-truncated versions of the plots are shared on OSF

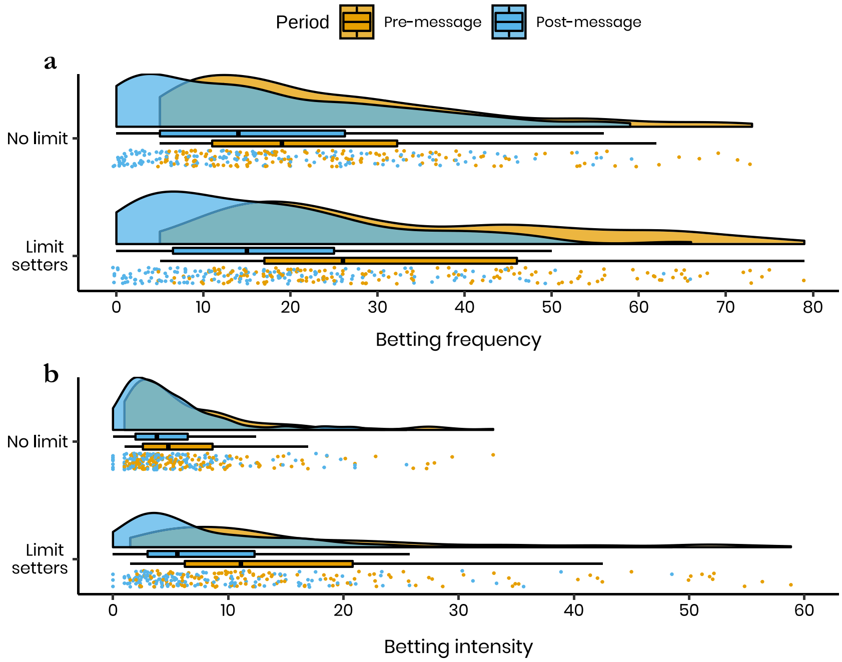

Figure 4 Effects of deposit limits on gambling behaviour (90 days before and after). This figure displays the distribution of customers' betting frequency (a) and intensity (b) for the 90 days before and after receiving the first message for non-limit-setters and 90 days before and after setting their limit for the those who set a limit. The scale of Fig. 4b

Table 4 Comparisons between limit-setters and non-limit-setters on expenditure variables: robust ANCOVA outcomes.

Point of comparison			*Difference between samples*				*Robust ANCOVA*	
Design point	*n1*	*n2*	*Mean difference*	*SE*	*CI$^{(LB)}$*	*CI$^{(UB)}$*	*Test statistic*	*P-value*
Average daily wager								
6.02	83	81	24.66	6.28	8.01	41.30	3.93	**0.0002**
45.96	89	100	28.87	7.61	8.73	49.00	3.79	**0.0003**
94.33	96	74	27.97	9.49	3.18	52.77	2.95	0.0041
220.97	110	19	2.73	23.20	−62.83	68.29	0.12	0.9075
384.95	57	13	73.47	46.82	−68.63	215.57	1.57	0.1441
SD of average daily wager								
3.28	83	84	29.36	6.92	11.02	47.70	4.25	**0.0001**
39.98	89	100	30.10	7.30	10.77	49.42	4.12	**0.0001**
78.43	96	107	30.00	7.79	9.47	50.53	3.85	**0.0003**
163.90	110	33	−10.06	17.18	−56.20	36.09	0.59	0.5615
325.00	57	14	−1.43	34.39	−103.32	100.47	0.04	0.9675
Net loss								
−491.65	83	20	477.78	128.95	108.07	847.50	3.71	**0.0017**
153.90	89	85	323.33	66.61	148.09	498.56	4.85	**0.0000**
526.80	96	88	271.87	69.68	89.60	454.13	3.90	**0.0002**
987.37	110	38	−45.81	113.55	−345.96	254.33	0.40	0.6880
1522.96	57	17	−300.74	193.34	−866.90	265.43	1.56	0.1415

ANCOVA = analysis of covariance; *n*1 = number of limit-setters; *n*2 = number of non-limit-setters; SE = standard error; CI$^{(LB)}$ and CI$^{(UB)}$ = lower and upper 95% confidence intervals, respectively; bold *P*-values are statistically significant at < 0.002.

Table 5 Comparisons between limit-setters and non-limit-setters on gambling behaviour variables: robust ANCOVA outcomes.

Point of comparison			*Difference between samples*				*Robust ANCOVA*	
Design point	*n1*	*n2*	*Mean difference*	*SE*	$CI^{(LB)}$	$CI^{(UB)}$	*Test statistic*	*P-value*
Betting frequency								
5.00	74	78	4.22	1.49	0.33	8.12	2.83	0.0057
17.00	95	113	3.63	1.30	0.27	7.00	2.80	0.0060
25.00	113	96	2.94	1.36	−0.60	6.48	2.16	0.0332
44.00	74	39	7.21	3.12	−1.13	15.54	2.31	0.0253
68.00	37	12	5.26	4.96	−10.24	20.76	1.06	0.3147
Betting intensity								
1.50	77	87	2.14	0.52	0.77	3.51	4.12	**0.0001**
4.98	89	114	2.71	0.53	1.31	4.10	5.12	**0.0000**
8.66	99	60	1.44	0.72	−0.44	3.31	2.00	0.0483
11.15	103	37	0.71	0.86	−1.58	3.00	0.83	0.4129
16.64	85	14	−0.05	1.47	−4.44	4.34	0.04	0.9721

ANCOVA = analysis of covariance; *n*1 = number of limit-setters; *n*2 = number of non-limit-setters; SE = standard error; $CI^{(LB)}$ and $CI^{(UB)}$ = lower and upper 95% confidence intervals, respectively; bold *P*-values are statistically significant at < 0.002.

setters with higher baseline values (e.g. average wager of ≥~$94.33) was not significantly different from non-limit-setters with similar baseline values. The change in betting frequency was not significantly different between groups at any design point. The change in betting intensity was found to be significantly greater for limit-setters at low–mid design points.

DISCUSSION

The primary aim of this naturalistic randomized controlled trial (RCT) was to explore the value of different customer messages for increasing the uptake of deposit limits. Consistent with H1, customers who were sent messages were significantly more likely to set a deposit limit during the ensuing 5 days compared to controls. Analyses indicated that our data provided inconclusive evidence relating to the impact of both message content and delivery method, inconsistent with our predictions in H2 and H3 and warranting further research attention. Our manipulations of message content were designed to promote positive attitudes (personal messages) and social norms around limit-setting (social messages), and the inconclusive impact of these manipulations questions research that has found that these factors affect engagement with CPTs [11,26]. However, it may be that brief messages are insufficient for engendering these states and that alternative manipulations in content (e.g. positive versus negative framing) [27] may be more effective in this context.

Customer messages resulted in a small but notable increase (0.71%) in the use of the deposit limit feature and limit-setting was considerably higher when accounting for e-mail opening rates (1.13%). Given the minimal costs and resources required to send brief messages to on-line gambling customers, additional research should be directed towards better understanding how to increase customer engagement with both e-mail and in-account messages in order to maximize the impact of this low-cost intervention. Our limit-setting rates were lower than reported by BIT [14], who observed uptake rates for deposit limits of between 3.3 and 4.4% in response to their messages. Possible reasons for this discrepancy include that BIT sent messages in response to the operators' risk-identification system alerting them to a customer displaying risk behaviours (e.g. use of multiple payment methods), suggesting that these individuals may have perceived assistive tools as more relevant at the time of messaging than the randomly selected cohorts in our trial. Further, all individuals involved in our trial had already opted-out of setting a limit, indicating past decisions not to set a limit, although many of the limit-setters in our trial had previously set and removed a limit. Having previously used limits was the strongest predictor of setting one in response to our messages, suggesting that customer messages may be a useful strategy for encouraging re-uptake of CPTs.

In relation to our second aim, the study provides important results to support the value of deposit limits in shifting customers to sustainable gambling behaviours. Monitoring gambling expenditure and involvement revealed that limit-setters, compared to non-limit-setters, showed a significantly greater reduction in average daily wager, variation in average daily wager, net loss and betting intensity. These significant reductions were only found for customers with low–mid (not high) baseline gambling levels in the 90 days preceding messages. However, it is possible that limit-setters with high baseline levels of these variables significantly reduced their gambling, but our tests at these design points were underpowered to detect these effects. There were far fewer non-limit-setters with high baseline values with which to compare limit-setters (see '*n*2' column in Tables 4 and 5 for sample sizes used in all comparisons), and therefore the effects of limits on gamblers with high pre-limit levels of gambling involvement requires further study.

It is unknown whether deposit limit users who significantly reduced their gambling migrated to other sites and therefore did not reduce their overall gambling. Nonetheless, these findings are consistent with extant studies which have observed reductions in betting involvement and expenditure following limit-setting [5–7] but diverge from Ivanova *et al*. [8], who found no effect of limit-setting on net loss. This discrepancy may be because these authors compared net loss in the 90 days after registration between limit-setters and non-limit-setters, as opposed to the change in net loss observed before and after limit-setting, as was performed here.

This is the first trial, to our knowledge, to have studied both the effects of consumer messages and the effects of limit-setting on on-line gambling sites in Australia. The naturalistic RCT design used and the involvement of multiple operators adds credence to the external validity of the findings observed. However, there are limitations to the methods used. Our choice of a 90-day window pre- and post-message, while enabling some of the analyses reported here (e.g. predictors of limit-setting), precluded our ability to study individuals in the early stages of holding an account. We could not reliably detect individuals who may have held accounts with multiple operators, so some individuals may have disproportionately influenced outcomes. The study involved Australian on-line wagerers—whose gambling activity is restricted to sports and race wagering—and customers in other jurisdictions and/or using alternative forms of on-line gambling may respond differently to the messages used. Further, we targeted cohorts of individuals who engaged in regular gambling, but without consideration of behavioural risk indicators such as the BIT study [14]. Instead, we aimed to encourage deposit limit use as a harm reduction strategy for all regular on-line wagering customers, as this may be a method of preventing the development of problems. Future research in live gambling environments is required to determine the effectiveness of targeted messages sent in response to certain risk behaviours [14], as well as messages tailored to individuals and based on cohort preferences, both of which have shown preliminary value as customization strategies [27,28].

Overall, the findings from this trial suggest that customer messaging may be an inexpensive and easily implemented strategy that on-line gambling sites can use to increase the use of deposit limits and other CPTs. Given the reductions in gambling expenditure and involvement we observed among limit-setters, the use of deposit limits should be encouraged on on-line sites as a possible harm-reducing strategy.

Declaration of interests

R.M.H. has no declarations of interest. During the last 3 years (2017–2020), S.M.G. has worked on projects that have been received funding and in-kind support through her institution from Australian Research Council, NSW Liquor and Gaming, Svenska Spel Research Council, Responsible Wagering Australia, Australian Communication and Media Authority, Commonwealth Bank of Australia, National Association for Gambling Studies, GameCo, ClubsNSW, Crown Resorts and Wymac Gaming. S.G. is currently a member (2019–20) of the National Council on Problem Gambling International Advisory Board (Singapore) and receives an honorarium for this role as well as travel expenses to attend an annual meeting. She is a member of the Steering Committee for Remote Gambling Research and the Independent Research Oversight Panel both run by GambleAware, which provide an honorarium. S.G. has received honoraria directly and indirectly for research, presentations and advisory services from Credit Suisse, Oxford University, ClubsNSW, Clubs4Fun, Centrecare WA, Gambling Research Exchange Ontario, Crown, Department of Social Services, Community Clubs Victoria, Financial and Consumer Rights Council, Australian Communications and Media Authority, Manitoba Gambling Research Program, VGW Holdings, Nova Scotia Provincial Lotteries and Casino Corporation, Ministry of Health, Clayton Utz, Greenslade and Generation Next. S.G. has received travel expenses to attend meetings from Franklin Women, GambleAware, Community Clubs Victoria, Centrecare WA, Financial and Consumer Rights Council, Stiftelsen Nordiska Sällskapet för Upplysning om Spelberoende, Generation Next, Alberta Gambling Research Institute, QLD Treasury and Responsible Gambling Council

Trial registration

The trial pre-registration, full study protocol, pilot study reports, materials, data analysis code and all analysis outputs are all shared on this project's Open Science Framework page: https://osf.io/6dpkw/.

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Author contributions

Robert Heirene: Conceptualization; data curation; formal analysis; methodology; project administration; software; visualization. **Sally Gainsbury:** Conceptualization; funding acquisition; methodology; supervision.

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