



# Does relaying 'house edge' information influence gambler's perceived chances of winning and their factual understanding of the statistical outcomes?

A recommendation by [Charlotte Pennington](#)  based on peer reviews by [Zhang Chen](#)  and [Graeme Knibb](#) of the STAGE 2 REPORT:

Philip Newall, Richard James, Olivia Maynard (2023) How does the phrasing of house edge information affect gamblers' perceptions and level of understanding? A Registered Report. PsyArXiv, ver. 2, peer-reviewed and recommended by Peer Community in Registered Reports. <https://doi.org/10.31234/osf.io/pfnzd>

Submitted: 10 January 2023, Recommended: 19 March 2023

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Many products that can impact upon health and wellbeing (e.g., alcohol, food) relay information to consumers about the potential risks. However, such information is commonly provided in suboptimal format for gambling-related products. To encourage safer gambling, research has therefore recommended that information about the average loss from a gambling product ("house edge") or percentage payout ("return-to-player") should be communicated, with the former translating to better perceived understanding by gamblers. This Registered Report aimed to experimentally compare two phrasings of the house edge against a control return-to-player to arrive at the most effective phrasing to aid gambler's perceived chances of winning and their factual understanding of the statistical outcomes of their bet. Using a hypothetical gambling scenario, a sample of 3,333 UK-based online gamblers received one of three phrasings: an original house-edge ("his game keeps 10% of all money bet on average"), an alternative house-edge ("on average this game is programmed to cost you 10% of your stake on each bet") or return-to-player ("this game has an average percentage payout of 90%"). Two outcome measures were employed to judge the effectiveness of this information: gamblers' perceived changes of winning and factual understanding. The findings indicate that the two-house edge formats were more effective in communicating gambling-related harms than the return-to-player format, but the original house edge phrasing appeared to be the most optimal as it decreased gambler's perceived chances of winning and increased their factual understanding compared to return-to-player. These results can therefore inform public

health policies to reduce gambling-related harm by presenting the most effective communication of gambling risk. After two in-depth reviews, the recommender judged that the manuscript met the Stage 2 criteria and awarded a positive recommendation. **URL to the preregistered Stage 1 protocol:** <https://osf.io/5npy9>  
**Level of bias control achieved:** Level 6. *No part of the data or evidence that was used to answer the research question was generated until after IPA.* **List of eligible PCI RR-friendly journals:**

- [Addiction Research & Theory](#)
- [F1000Research](#)
- [Peer Community Journal](#)
- [PeerJ](#)
- [Royal Society Open Science](#)
- [Swiss Psychology Open](#)

## References

1. Newall, P. W. S., James, R. J. E. & Maynard, O. M. (2023). How does the phrasing of house edge information affect gamblers' perceptions and level of understanding? A Registered Report. Acceptance of Version 2 by Peer Community in Registered Reports. <https://doi.org/10.31234/osf.io/pfnzd>

## Reviews

### Evaluation round #2

Reviewed by [Zhang Chen](#) , 08 March 2023

All my comments have been addressed satisfactorily. I look forward to seeing this nice piece of work being published.

### Evaluation round #1

DOI or URL of the preprint: <https://psyarxiv.com/pfnzd>

Version of the preprint: 1

**Authors' reply, 06 March 2023**

[Download author's reply](#)

**Decision by [Charlotte Pennington](#) , posted 15 February 2023, validated 16 February 2023**

**Decision on your Stage 2 Registered Report**

Dear Philip Newall and collaborators,

Thank you for submitting your Stage 2 Registered Report. I have now received reviews from two experts in the field of behavioural addictions who also reviewed your Stage 1 RR. You will see that one is very positive with only minor suggested changes and the other is more detailed with more substantial recommendations. I agree with the evaluation of Reviewer 2 (Chen) and would like you to carefully consider these, outlining your responses along with your revision. I have also undertaken a full review of your manuscript and detail my recommendations below – the main one being the reporting of effect sizes and associated confidence intervals for the t-tests and equivalent tests. I therefore offered a decision of: Invitation to revise.

Some additional comments from me regarding my own review and information about formatting:

1. Thank you for including the Stage 1 accepted protocol in the Abstract section. You may want to adjust its positioning, however, to ensure it reads exactly how you'd like to see it when it's in print. For example, I personally think this would read better in the Methods section of the Abstract. Please use the date format DD/MM/YYYY. This same information should also be outlined in your Methods section, in the first paragraph (also see Point 2).

2. The first paragraph of the Methods section is still written in present tense and from my review, the link to the OSF project page (<https://osf.io/6hbyp/>) does not include the data and materials. Please also carefully consider the layout of your OSF project page – it is clearer for users if you use sub-folders such as 'Data', 'Materials' etc with accompanying read.me files or data descriptions. PCI RR is a signatory of the Transparency and Openness Promotion (TOP) guidelines, which describe a series of modular standards for transparency and reproducibility in published research. In general, authors are required to make all study data, digital materials, and computer code publicly available (at Stage 2 submission) to the maximum extent permissible by relevant legal or ethical restrictions.

3. The Abstract states: "The optimal communication of risk information can act as an input to a public health approach to reducing gambling-related harm" but it is not clear exactly what this optimal communication of risk information might be from your findings.

4. Please check your manuscript for grammar. E.g., there are full stops missing in the Participants section of the Methods.

5. The Method section seems to read confusingly without the information about the final sample size. Can the following sentence be taken from the Results and added to the Participants section in the Methods: "Overall, 3,453 participants completed the experiment as planned. Of these, 62 (1.8%) participants completed the study in under one minute, and a further 58 participants (1.7%) indicated that we should not use their data, resulting in a final sample size of 3,333 participants". You also need to provide a breakdown of how many participants were in each condition.

6. The study has now been completed so I ask you to pay close attention to the phrasing from your Stage 1 Report – it is fine to change such phrasing/tense (only) in the Stage 2 report. For example, the following sentence reads strangely: "We now consider some power analyses to support our plan to collect 1,000 usable responses per-condition. For H1 and H2, it is impossible to know at this stage what magnitude of change on the dependent measures would lead to meaningful differences in actual gambling environments." This could be "Within our preregistered protocol (see LINK), we considered some power analyses..."

7. In your Methods section you state that "We aimed for an average sample size of 1,000 participants passing data quality checks per-condition, as this is was the closest round number which exceeded the required sample size in each of the below power analyses", but this is inconsistent with a later sentence that states "Therefore, with these two data quality checks in mind, we planned to collect data from 1,151 participants per-condition in order to reach our planned sample size." Can you explain this discrepancy and fix it?

8. The section on Statistical Analysis is still written in the present tense – please ensure consistent tense is used in the final Stage 2 report.

9. For the equivalence tests, I don't necessarily agree with Reviewer 2 that the t-tests don't add anything

different. The t-tests show you whether the effect was statistically significant to zero whereas the equivalence tests show you whether or not you have a meaningful, equivalent, or inconclusive effect. There is missing information here, however, which is required to the evaluation of equivalence tests – the 90%/95% confidence intervals (whichever one used) of the effect size. As per Zhang Chen's comments, please ensure that your evaluation of these is correct – if the equivalence tests are significant but include effect sizes at either end of the CIs which overlap with both a practically meaningful and practically equivalent effect, then the data is best described as inconclusive (the ES could be meaningful, or it could be not meaningful... there is not enough data to conclude). Please add the effect sizes and associated CIs to the reporting of the equivalence tests and check your interpretation of these with regards to meaningful, equivalent or inconclusive.

10. The discussion states: "Overall, this supports the use of the original phrasing instead of return-to-player information (Newall, Walasek, & Ludvig, 2020a)" – is this finding in line with your 2020a research? The placement of the reference made me question this so you might want to say "in line with Newall et al. (2020a)" (if this indeed was the case).

11. On Page 18, the first and second paragraphs should be combined together – they don't read well separately.

I wish you the best of luck with your revision and look forward to receiving it for further consideration.

Dr Charlotte Pennington

PCI Recommender

### Reviewed by **Zhang Chen** , 17 January 2023

Thank you for the opportunity to review this Stage 2 manuscript. I have received the Stage 1 proposal of this paper, and thus mostly focused on the results and the discussion section. The authors adhered precisely to the registered study procedures, and the data are able to address the proposed hypotheses. The conclusions are also justified given the evidence. Below I list a few comments/suggestions, mostly quite minor, to hopefully make the results more informative and easier to follow. To be clear, the page numbers refer to the version with tracked changes. Hope my comments are useful to the authors.

1. Page 9: I think it would be useful to have more information on the participants in terms of their (self-reported) gambling behavior. For instance, from Prolific, it should be possible to download the participants' responses to the prescreen question "What types of online gambling / casino games have you played?". Providing some descriptive information on e.g. the number of gambling games played by each participant, and the most popular gambling games in this sample would be informative.

2. Page 9: It would be useful to mention the scores on the PGSI used to create the four groups of gamblers.

3. Page 12: It would help me as a reader if the authors could specify how the seven options in the Likert scale are coded. For instance, when first encountering the scale on page 11 "My chances of winning are... Very high / High / Somewhat high / Neither high nor low / Somewhat low / Low / Very low chance of coming out ahead", I assumed that "Very high" = 1, "High" = 2 etc. However, after reading the results and the conclusions, I think "Very high" = 7, "High" = 6 ... Thus, a higher score means a higher perceived chance of winning. The authors may explicitly state this in the methods and results section.

4. Page 12: I find the part on equivalence tests a bit difficult to follow. First, I think the first two t-tests are redundant because they essentially test the same hypotheses as the two regression analyses reported above? Or are they actually inferiority tests (see below)? The authors may need to explain the rationale behind equivalence tests a bit more to help readers better interpret and understand the statistical results. E.g., one of the two one-sided tests tests when the effect is larger than the lower bound of the region of practical

equivalence (ROPE). However, since the effect size in this case is smaller than the lower bound of the ROPE (thus in the opposite direction as tested by the one-sided test), the one-sided t test yields a p value of 1. From Figures 1 and 2, it is clear that the effect sizes are outside of the ROPE, and the authors conclude that the differences are “practically different”. However, they may strengthen this conclusion by formally testing it, by e.g. using an inferiority test. Lakens et al. explained these different tests in their tutorial paper Lakens, D., Scheel, A. M., & Isager, P. M. (2018). Equivalence testing for psychological research: A tutorial. *Advances in Methods and Practices in Psychological Science*, 1(2), 259-269.

5. Table 3: This may be a matter of personal taste. Personally I find the table easier to read and understand if the coefficient and the 95% CI would be presented together in one column (since the 95% CI reflects uncertainty in the estimation of the coefficient), and t value and p value would be in one column (since they often have a one-to-one mapping).

6. Table 3 shows that the coefficient for the interaction effect on perceived chances of winning is positive, with the confidence interval just including 0. Seeing this effect, I think the conclusion that “PGSI levels did not influence either outcome measure ( $p$ 's  $\geq .078$ )” (page 15) may be premature. Apart from the usual problem that one cannot support the null hypothesis based on p values larger than .05, this interaction effect shows that the effect on perceived chance of winning between alternative versus original phrasing of the house-edge information might be reduced among problem gamblers. While acknowledging that this is exploratory in nature, I would not immediately dismiss this potential effect.

7. This is a minor point - sometimes the text and the statistical result are not consistent with each other. E.g., on page 15, “which according to an ordinary least squares regression was higher than both the original ( $B = -0.95$ ,  $t = -14.1$ ,  $p < .001$ , 95% CI [-1.08, -0.82]), and alternative ( $B = -1.66$ ,  $t = -24.6$ ,  $p < .001$ , 95% CI [-1.80, -1.53]) house edge conditions”, the text says “higher” but the coefficients are all negative. While I am aware that this has to do with which group is used as the reference category in the analysis, presenting the results consistently may nevertheless facilitate understanding.

8. Similarly, for Figures 1 and 2, to facilitate understanding, the authors may consider adding an arrow pointing from 0 to the left, and a label such as “the alternative group had lower perceived chances of winning/had a lower accuracy”, and an arrow pointing to the right and saying “the original group had lower perceived chances of winning/had a lower accuracy”.

9. Page 17: The reduced correct understanding in the alternative group might be explained by one of the two added incorrect answers. While the authors argued that this made the measurement more sensitive, it also made me wonder whether there is research on what kind of interpretations gamblers spontaneously generate when they are presented with different messages. If they do not spontaneously generate interpretations as the added incorrect option, adding this option may make the measurement less valid? It may also be interesting to test understanding in such a way, with open-ended questions - after all, in real-life gambling situations, gamblers are also not presented with options to choose from.

10. The discussion mostly focused on the issue of correct understanding. I wonder how the authors may explain the lower perceived chance of winning in the alternative phrasing of house-edge condition.

### **Reviewed by Graeme Knibb, 15 February 2023**

This study investigated the effect of two different ‘house-edge’ messages on gamblers understanding of loss. In addition these two messages were compared to return-to-player information.

Since the stage1 review the manuscript has only minimal changes (related to tense). There is also a clear link to the approved protocol. The analysis does not deviate from those which were initially planned.

The overall conclusions, as presented within the discussion, are clear and based on the evidence.

As stated in my previous review I think this is a strong and well considered RR and I am very happy to see a stage 2 manuscript. I have no issues with the study or the conclusions that have been drawn and I am very happy to recommend this stage 2 manuscript for publication.

One minor wording that may need changing does appear on page 8 in which the open materials are discussed. The language here is still in future tense and should be altered accordingly. I.e. 'will be placed' could become 'can be found'.