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6th May, 2025

Dear Dr Hamilton and Dr Traccani,

Please consider our revised manuscript “Variable and sub-optimal responses to a choice problem are a persistent default mode” for publication Q*JEP* (QJE-STD-23-065). We were offered the option to resubmit this paper if we could address your, and the reviewers’, comments. As the delay in resubmission implies, we have carefully and thoroughly considered all the comments and made extensive revisions to the paper to address them. These changes include a major overhaul of the analysis approach (replacing the frequentist analysis with a bayesian one), incorporating the experiments from the supplementary materials and appendix into the main text, improving the clarity and focus, and integrating additional literature into the writeup. We are grateful for all the suggestions and comments, and for the opportunity to improve and clarify the paper.

Below we provide responses (in green) to each point raised by you and the reviewers (in italics). Changes in the paper are highlighted in red. Thank you for considering our revised manuscript. We look forward to hearing your opinion and thank you in advance for your consideration of this article for publication in QJEP.

Kind regards,

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***Editor’s comments***

*1) Right from the beginning of the Introduction, the reader encounters difficulties understanding your arguments. For example, in the first few lines, you state that “Given two relatively easy tasks, it makes sense to try and accomplish both” and that in the typical “focus-or-divide” dilemma "[participants are] given two targets, and they ha[ve] to decide whether to try and accomplish both or to focus on one or the other." There seem to be 2 TASKS that people might choose to perform. However, immediately after, it is stated that there is only one task but with an uncertain target: "For example, in one version of the task, participants' goal is to throw a beanbag into one of two hoops, but they are not told which of the two hoops is the target until after they choose where to stand."*

Thanks for this helpful point - we see that our wording here was unclear. We now describe the dilemma more carefully and clearly, and use the beanbag-throwing task as a specific example of the more general structure. We are more careful in our language and stick with the more generic term “goals” and the resource requirements of achieving them. We use “task” to refer to the experimental task we ask participants to perform. We also added a paragraph to the discussion (page 26-27) that addresses the ways in which our particular implementation of the focus-divide decision may or may not extend beyond this context.

*The "focus-or-divide" logic (i.e., choosing to either focus on a target or attend to all possible targets) is not well explained at the beginning of the paper and is only understood later on. In fact, the explicit description of what "focus-or-divide" means when applied to a task with two possible targets is not provided (the examples given earlier, such as turning off the radio while driving, do not provide much clarity).*

We now give a real-life example of the dilemma (home renovation) in the first paragraph that more straightforwardly maps onto the conditions of the experiments we report. We walk through the constraints and consequences of these kinds of choices for this real world situation, as well as describing it in the abstract, and in terms of specific experimental predictions. We hope this has made it clearer from the start exactly what the decision and its logical solution is.

*It is stated that participants make suboptimal choices, but it is not explained what they actually do when attempting to solve, e.g., the beanbag-hoop problem (i.e., that they usually show a variable pattern of responding). A few lines later, you state "most of them do not modify their focus or divide choices with manipulations of task difficulty at all." Until that point, the difficulty of the task had not been mentioned (only later does the reader realize that you were referring to the distance between the targets), so it is unclear which modification is being referred to and why it should have occurred.*

We have clarified the description here, and together with the changes to describe the dilemma more completely and clearly, we think has eliminated the potential for confusion.

*Finally, while discussing Clarke and Hunt’s (2016) experiment, mention is made of an “APPENDIX A” (which is actually the only appendix, so the specification "A" creates confusion) where readers expect to find data related to Clarke and Hunt’s study, but instead find the description of an experiment with the authors as participants. The experiment, which is intended to demonstrate that people are capable of implementing optimal strategies once they have made the decision to do so, can only be understood by the reader later in the text (for example, at that point in the article, the reader is still unaware that you used the same task as Clarke and Hunt) and is not adequately introduced (the reader should be provided with an explanation of why this experiment is necessary – is there any reason to question people's ability to implement the optimal strategy?). Similar issues are present in other parts of the manuscript (as noted in the reviewers' comments). The descriptions of the two experiments, their results, and their rationales (including the replications of the experiments) should also be better integrated with each other. Another possibility could be to focus solely on one experiment (the preregistered one). However, I am unsure if you have enough evidence to substantiate your arguments in that scenario.*

We agree that the issue we are addressing with Appendix A (now Experiment 3) is really only clear once the overall results are known – had people been able to use the training to make better decisions, as we had predicted, this sanity check would not have been necessary. To improve the overall flow of the paper, we now foreshadow the results of the whole series of experiments at the end of the introduction (page 6) rather than leaving readers “in suspense”.

To the second point (i.e. why would we even question whether people can implement the optimal strategy?): We agree it seems intuitively unlikely that people would fail at this, as it seems quite a simple task. But over the course of running these experiments we had repeatedly expected people to start using the optimal strategy based on what we considered to be very firm nudges in that direction in the form of the reaching and instruction interventions. The persistent failure caused us to wonder if implementing the strategy consistently really was as easy as it seemed, or if this was a failure on our part to recognise some complication or obstacle. So we tried the experiment on ourselves and some lab members as a sanity-check and confirmed that yes, it really is very easy to implement effectively. We have added an introduction to what is now Experiment 3 that lays this thought process out more clearly.

We also had to add a new appendix (see next point) which is simply called “Appendix” (we agree the “A” was superfluous, and a vestige of having had two appendices in an earlier version of the paper).

*2) As underlined by the reviewers, both the experiments and the analyses should be better described. The statistical choices should be more thoroughly justified to prevent future readers from raising the same doubts as the reviewers did. The way in which Experiments 1 and 2’s analyses are presented should be consistent. Analyses that are based on the same logic and/or the same statistical tests should be presented in the same manner and with the same terminology/symbolism to help the reader understand them. If similar analyses have been conducted in previous studies that are based on the same experimental procedure, this should be explicitly stated.*

We have substantially rewritten these sections to make them easier to understand. Following Reviewer 3’s advice, we have switched to using Bayesian rather than frequentist methods as these are more appropriate for our data and predictions. There is no change in the conclusions. The analysis plan we had pre-registered for Experiment 2 was frequentist though, so in the interest of open science we reported the pre-registered analysis in the Appendix .

Where possible, we have used the same terminology, order, and presentation conventions across all analyses. The change in the nature of the decision - from the continuous standing position choices to discrete fixation at center or side - does mean some adjustments to the modeling have to be made, but we have tried to align these as closely as possible. In this particular paradigm we find participants either all solve the problem optimally (as seen in the reaching task, and in the non-naive participants in the Appendix) and have near-0 variance, or they are highly variable (as seen in the throwing and detection tasks in E1 and E2). The distinction is stark and the statistics we have used have been focused not on detecting the difference (which is massive) but on describing the variability as precisely as we can.

*It could be helpful to include an introductory paragraph (before both experiments or before describing the results of each experiment) that describes the planned analyses and their rationale. Moreover, it should be made clear why only Experiment 2 was preregistered. What was the reasoning that motivated you to plan and conduct another experiment? One can certainly understand the reason, but the thought process behind it should be presented to the reader. Why did you decide to use different tasks in Experiments 1 and 2? What is the underlying rationale? The most obvious choice would have been to use the same paradigm (e.g., the one used in Experiment 2) in both experiments.*

We have followed your recommendation and expanded the introductory paragraphs for each experiment. Using converging methods helps demonstrate that our results generalise and don’t just apply to the specific conditions of particular experiments, and one of the strengths of the paradigm is that it is so portable across different situations. As to why we pre-registered Experiment 2 and not Experiment 1: to be perfectly honest, we only thought of this when we got to Experiment 2, at which point we recognized that we had a clearly defined plan and set of predictions that were well-suited to pre-registration.

*Some parts of the Results sections are particularly difficult to understand, for example, the section regarding the performance analysis in Experiment 1 (incorrectly labeled as Experiment 2 in Figure 3). Figure 3 itself (the boxplot) is not described clearly. Although this figure employs a standard way of representing data, providing a description of the meaning of the different aspects of the graph would help the reader understand your conclusions and the reasons why, in that case, a specific comparison test between the simulated performance and the actual performance of the participants was not necessary. Similar problems also pertain to other figures and analyses (as indicated by the reviewers' comments).*

All figures have been considerably revamped alongside the switch from frequentist to bayesian analysis. We have taken this comment on board as we did so, and expanded our figure captions to ensure we described them in detail without assuming prior knowledge of standard plotting conventions.

*3) All the experiments included in appendices or supplementary material should be reported in the main text and adequately introduced. In this regard, if the authors choose to resubmit the article to QJEP, I recommend seeking clarification from the editorial office regarding the QJEP policy on authorship when a portion of the experimental work is conducted by individuals who are not listed as authors of the article.*

As requested, the four additional experiments that had previously appeared in the Appendix and supplementary materials are now included in the main manuscript as experiments 1B, 1C, 3A and 3B. They have been substantially revised to integrate them into the main text. We have added to the acknowledgements sections to thank the students who contributed data collection to these experiments.

*The results of the original experiments and their replications might be analysed together. That would strengthen your conclusions. Let's take, for example, Experiment 1. The critical effect was indeed observed in the original experiment (even if it was not observed in the replications). Your conclusions (i.e., that the significant effect in the original experiment was due to chance) do not appear completely justified.*

As suggested, these three experiments are now grouped together as Experiment 1a, b & c. The critical effect was *not* clearly observed in Experiment 1: it might appear that way at first glance, but we conclude that the small group difference we observed between the reaching group and the control group was largely due to the fact that our control group happened to include more participants who preferred to stand near the middle in the throwing task. This interpretation is reinforced by the bayesian hurdle-log-normal model we apply to the data in the revised paper, and from the results from what is now reported as Experiments 1b and 1c (as suggested in this comment), as they also do not show any clear effect of the reaching task on decisions in the throwing task.

4) Two additional considerations: (a) participants were never asked whether they had chosen and followed any particular strategy, and (b) there is no control condition in Experiment 1 (i.e., a condition in which participants, before performing the beanbag-hoop task, engage in an unrelated task). It might be worth considering conducting a replication of the experiments taking these aspects into account.

For a: We have posed this question in other versions of the focus-divide experiment, and not found the responses very enlightening. They often just describe verbally what they did in the experiment (e.g., a person who stands in the middle a lot will say “I mostly just stood in the middle.”). For b: there is a control group in all three versions of Experiment 1. We hope this is conveyed more clearly by the addition of Figure 1, which presents control manipulations in blue, and the priming in red, for all four experiments. The results figures maintain this same colour scheme.

Reviewer: 1

Comments to the Author

The paper is well written and present a wide literature review. The authors present two experiments in order to investigate how participants made the optimal choice. Although some insightful results are presented, some recommendations are listed below to add more values to the paper.

Major Comments:

*1) The aim of the paper should be reinforced in abstract. Before presenting the experiments, the authors should discuss why the experiments have been done.*

As noted in response to the editor’s comments, we have added a more detailed motivation immediately before each experiment, and expanded the narrative to include more centrally the follow-ups to corroborate Experiment 1 (now E1B and E1C) and the “sanity check” experiment that appeared in the Appendix (now E3A and E3B). We also added a sentence to the abstract explicitly stating the “aim”.

*2) In introduction the authors declare “There are many examples of these “focus-or-divide” kinds of dilemmas in daily life, from deciding which home improvement projects to embark on with a limited budget, to deciding to switch off the car radio when navigating through an unfamiliar neighborhood.” This involves Multi-Attribute Decision Making problems; thus, this issue should be cited in the paper.*

*See:*

*Belton, V., Stewart, T., 2002. Multiple Criteria Decision Analysis: An Integrated Approach. Springer Science & Business Media, Boston, MA*

*Keeney, R.L., Raiffa, H., 1976. Decision Analysis with Multiple Conflicting Objectives. Wiley & Sons, New York.*

This part of the introduction changed in response to editor suggestions and these examples no longer appear there.

*3) In introduction the authors declare “Which of these strategies will lead to a higher probability of success depends on the distance between the hoops”. However, the authors do not discuss how the probability of success is obtained.*

We have substantially edited this paragraph to make the paradigm clearer. Hopefully this should resolve any confusion.

*4) In introduction the authors present several studies, but the knowledge gap can be better presented and justified. Moreover, highlighted which is the impact of the study in this area of knowledge.*

The introduction has been revised in line with editor and Reviewer 2 suggestions. We hope the motivation is clearer now.

*5) In Experiment 1, the authors declare that “If so, the decisions of participants in the throwing task should be close to optimal if they just carried out optimal decisions in the table version of the task.” What is the table version of the task? This is similar to consequence matrix in Multi-Attribute approach? The decision table should be included in the paper.*

I think some confusion has arisen due to our inconsistent use of terminology. Both “table” and “reaching” tasks refer to the same version of the focus-divide experiment in which the participant chooses where to sit at a table and reaches for beanbags on it. We have edited our manuscript to consistently use “reaching task”. This task is described in detail in the preamble to Experiment 1A.

*6) In Results section, what standard deviations about the probability of success are not discussed? Which is the statistical distribution used to compute the probability of success? This topic requires a major statistical rigor. A study using the probability of success and standard deviation in a similar decision task has been investigated in a previous paper (“The use of the success-based decision rule to support the holistic evaluation process in FITradeoff”).*

We agree with the general sentiment that statistical rigour is important. We have changed the analysis approach considerably since the previous version, which we think better captures uncertainty around our estimates. Variability in free-choice behaviour is very large in this task, which is an interesting feature of the behaviour but it makes the average a poor model. We have instead presented either data from individuals or 95% HPDIs for parameter estimates. We hope this addresses the concern about standard deviation.

*7) It is not clear if any equipment has been used in Experiment 1. The author should clarify this issue.*

We have improved the section headers and it should now be clear that there is a “Materials and Procedure” section in which the equipment is listed as 6 PVC beanbags and 6 hoops.

*8) The author should divide the General discussion in two sections in order to include a conclusion section with remarks about the limitations of the study and future studies.*

While we agree that section headers can help structure a paper, the general discussion section includes comments about limitations and future directions throughout the text, and extracting these into a single section would either add repetition or make them difficult to understand, or both. That said, we have added a penultimate paragraph to the discussion that addresses the constraints to generality of our conclusions. The final paragraph makes broad conclusions and begins with the phase “in conclusion’.

*9) In addition, in my view, the main remarks of the paper has been presented in the last paragraph of the paper. This should be reinforced in the Introduction section and in the General discussion. The authors discuss a lot the study using rats but do not present the main impact of the paper for the community.*

We have tried to reinforce/foreshadow the broader conclusions and implications more clearly in the introduction and GD.

*Minor Comments.*

*The presentation is clear, but Figure 1 should be improved. In my view, Figure 1 could present only the table and color information and Figure 2 should present the building.*

We have improved Figure 1 to include an overview of the three versions of the experiment we devised to test for transfer effects from the table task to the throwing task. It is our view that having all the methodological elements presented in the same figure makes it easier on the reader to understand the logic of the manipulation and relationship between the two key tasks.

*Reviewer: 2*

*Comments to the Author*

*This manuscript describes two studies of action planning, aimed at investigating when optimal allocation strategies are adopted. The action planning tasks are to allocate one's position optimally in where to stand for a beanbag-tossing task (Experiment 1), and where to fixate in a dot-detection paradigm (Experiment 2).*

*The tasks examined here, and the strategies that could be applied, are interesting material for exploration. And in general, I think that the question of when responses become habits, and habits become strategies, are big and interesting questions. But I don't feel this paper, as written, makes much progress towards providing answers to these questions.*

*To elaborate, I found the paper hard to assimilate, because the investigation does not seem to be firmly grounded in any body of theory and the corresponding literature. Initially, the authors position this as a study of rational decision making, although passing reference is also made to insight problem solving, and to reinforcement learning. The lack of a clear focus is perhaps connected to a failure to summarize relevant literatures (of which there are several, more on that below). The references seem to focus on prior works by the author(s) on the same task or closely related tasks, with some references to related work, e.g. Morvan & Maloney, 2012; Grant & Spivey, 2003.*

It has indeed been challenging to integrate this series of experiments into existing theoretical frameworks. In our view, that’s what makes the results important to publish – the fact that the behaviour we’re observing in these experiments does not appear to be well explained by existing theories. It does make it difficult to write an introduction, as the number of potentially (but not quite) relevant concepts and theories is very large, and the number of directly relevant ones may in fact be 0. We are grateful for all suggestions when it comes to additional literature we should include and have broadened the introduction to bring in the suggestion below.

*As a central theoretical framework, the authors reach back to a work by Maier (1940). Maier is said to have described three basic processes (or "categories") that are used in problem-solving: variability of responses, "equivalence reactions", and reasoning. Variability of responding is said to enable reinforcement learning. This is a reasonable idea, and in fact fundamental (if sometimes implicit) in the reinforcement learning literatures (animal learning, human implicit learning, machine learning) and also in the decision literature on the exploration-exploitation tradeoff. Citing these literatures seems to me not only adviseable from a scholarly perspective, but these literatures offer potential mechanisms that could account for performance and learning in tasks like those studied here.*

*The second proposed process, "equivalence reactions", seems to* ***involve transfer of learning*** *(what I believe is a more useful term). Again, a huge literature, not cited here in any depth or breadth. In this manuscript, transfer (if I may) is said to stem from "shared features" (p. 4) or similarity. But I would argue that the transfer to be expected in the present studies is analogical transfer -- the table task, the beanbag task, and the eye-fixation tasks are not at all similar from perception and action points of view. They may only seem "similar" to a cognitive science researcher. Thus, implicit association-learning systems (one important context where reinforcement learning can operate) would not be expected to be triggered, in my opinion. Rather, these tasks are only similar in a very abstract way, so if anything, I would characterize the hoped-for phenomena here as analogical transfer. And analogical transfer is very difficult to achieve, see Gick and Holyoak, 1983; Holyoak & Koh, 1987; Novick, 1998; Bassok, 2003; etc. etc.) In general, there is a large, relevant literature on this sort of analogical learning and transfer, under the term "****pragmatic schemas****"; a seminal paper is Cheng & Holyoak, 1985; but there were a slew of related papers on this in this very journal (QJEP) during the 1990's (e.g. Markovits & Savary, 1992, to name just one).*

The literature on learning and transfer is indeed very broad and covers many subfields – animal learning, problem solving, skilled motor control, perceptual learning (to name a few). In positioning the focus-divide experiments in this wider literature, we drew a distinction between insight problem solving and our task, which we argued had – as a simple, repeated decision – more in common with the kinds of decisions made in non-human animal learning experiments (see second page of the introduction). The papers this reviewer recommends we reference involve logic problems and inferential reasoning. For example, in Gick and Holyoak (1983) and in Markovits & Savary (1992), participants solve hypothetical problems, framed as a story, under different conditions of “hints”. The terms and theories in this area of research don’t transfer directly (so to speak) to our experiments, in which participants make a series of standing position and fixation choices over dozens/hundreds of trials, in the service of a larger goal (throwing/detection accuracy). We do agree however that in drawing this distinction we did miss an opportunity to acknowledge that there is a similar conclusion from many of these subfields as we draw in ours, which is that the failure to evoke better decisions in our task echoes the failure to evoke analogous transfer by Gick & Holyoak and others. To widen our literature review to include this, we felt it also important to incorporate perceptual learning, which is more similar to our approach. Perceptual learning is also highly specific, often showing a failure to transfer training benefits even from one location on the retina to another (e.g. Karni & Sagi, 1991). To address the reviewer’s comment, we have cautiously expanded the literature review in the introduction and discussion to include some elements from other subfields that may overlap conceptually with ours. However, we note in the paper (and here) that the mechanisms may be distinct, so as not to over-generalise our conclusions.

*The final process mentioned is reasoning, which may or may not be important in how some participants approach this task. It is certainly important in rational theories of decision-making, and insight problem solving. But not much is said about this process or "category". And again, the decision-making literature is not really covered here. I think the authors would do well to look at, and bring into the discussion, the decision-making work on probability matching. That is a phenomenon where people consistently perform suboptimally, emitting a mix of responses even though a simpler strategy, consistent choice of the more probable outcome, is optimal.**This literature could not be more relevant, in my view. In particular, Gao and Corter (2015) posited that subjects probability match because they overestimate their own abilities, operating under an implicit goal or expectation to perform perfectly. Could something like that be happening in the present tasks?*

We are well aware of the probability-matching phenomenon, and have addressed its relationship to the focus-divide dilemma in another paper in which we manipulate which of the tasks or goals is more probable, difficult, or rewarding (James et al., 2023). But we have added a reference to it here as well (see page 30).

*Another issue, besides this dearth of theoretical depth and engagement with recent literatures in decision making and problem solving, is that the manuscript could be organized better. Passing reference is made at certain points to material in the Appendix, but that status and relevance of that material is not explained very well. Is it pilot work, follow-ups? Why is it not incorporated into the main body of the text?*

We have reorganised the paper according to the suggestions of the editor and reviewers. The previous contents of the Supplementary Material document now appear as Experiment 1b and 1c, and the follow-up in the Appendix is now Experiment 3. Our original goal was to try and keep the paper focused by eliminating some redundancies, but this appears to have gone too far outside of convention so we have added it back in. All the data and analysis scripts supporting the paper are available in a public repository.

*The exposition problems above were somewhat remediated in the first few paragraphs of the General Discussion, which presents a brief and clear summary of the problem being studied, and what we do and do not learn from the results. A clear statement like this would be a good way to end the Introduction. And the remainder of the Discussion engages fairly well with one relevant literature, on habits and strategy learning (although there is more on the latter in the decision making literature). Actually, I am completely serious in proposing that the paper would be improved by essentially switching the General Discussion and the Introduction (with appropriate adjustments, of course).*

We have reworked the introduction to “front-load’ the conclusions better. To be transparent here, we were repeatedly surprised that our attempts to nudge people to solve the problem did not work. The original order of the paper was a reflection of our own change in thinking before, into after, once we had convinced ourselves that the results were robust and generalisable through repeated replication of the same outcome. We realize now that this made it confusing to read and have revised accordingly.

*Still, I wonder how well we can generalize the conclusion that "Based on the results, we conclude a variable trial-and-error approach to making choices is a stable default." Is it a stable default in this laboratory task? Perhaps. But how and how well does that conclusion generalize to other tasks and contexts? And is it important, or does it just mean that when participants don't know what is going on, they muck around and explore? Perhaps some post-task interviews with the participants would reveal what they understood about the task, and whether they even realized that their choices about initial position might affect their performance. Again, are we as researchers on the lookout for implicit or explicit learning? That seems critical to directly address and discuss.*

There are several comments to address here.

“How well does that conclusion generalize?”: we have conducted many experiments on this dilemma, retaining the same mathematical structure but across a wide range of contexts - throwing, detection, memory (Clarke & Hunt, 2016), a gamified version of the fixation task (James et al., 2019), and a speeded reaching task (Hesse et al., 2019). In as-yet-unpublished work we have also documented the same failure in decisions about bidding in an auction and studying for a test. We are confident this is not a context-specific problem.

“Do people even realise their choices… affect their performance?” In other work, we have addressed (and ruled out) the concern that people’s understanding of this relationship might be noisy or incomplete (it is not, James et al., 2017).

“And is it important, or does it just mean that when participants don’t know what’s going on, they muck about and explore.”? Yes to both. This is our conclusion, in a nutshell – and also that getting them to shift them out of this “mucking about” mode is surprisingly hard to do. We think this is an important observation that does not fit clearly into any theoretical framework we have been able to identify, except the “means-end readiness of the problem space” idea presented by Krechevsky (1937). If the reviewer can point us to a more up-to-date (and ideally human-centered) theoretical framework we would happily adopt it.

“Are we … on the lookout for implicit or explicit learning?” We were on the lookout for any learning, and didn’t find it.

*Specific (and some general) comments:*

*p. 5. The authors say that "our question in this study is about whether and how we can transition people from variability to reasoning." My question is: How can we detect that people are engaging in reasoning? Optimal responding could result from reasoning, or from fast and efficient reinforcement learning with feedback. What mechanisms do the authors think are involved? What data will be gathered to distinguish between these possibilities?*

Had we found evidence that people made a transition, this would have been an interesting question to ask next. People did not transition out of variability.

*p. 7-8. Regarding the table task: "For the largest distance between the beanbags.." -- does this mean when they were asked to pick up the two BLUE beanbags?*

Rephased to avoid confusion. It now says: “When they were told they would need to pick up one of the two blue beanbags (too far away to reach from the central chair)...” .

*Figure 2 is not explained well. The caption should say what the Y and X axes show - I initially was confused about which was the DV, since both axes represent physical distance. "Delta" is not explicitly defined anywhere that I can see, although I eventually figured out what it was. Perhaps symbols added to the graph showing optimal performance would be helpful to the reader. BTW, what is optimal performance for delta=4? I honestly have no idea, since it seems to be dependent on people's skill at throwing beanbags. Also, what does it mean that some people have a "Normalised standing position"=2? Where exactly were they standing? What does this mean about their strategy?*

We have rewriitten the caption for Figure 2 to try and address these points of confusion. On a handful (7) of the 1440 trials conducted, participants did not choose a standing position that was between the two hoops that were their targets. To be fully transparent about the range of variability we observed in this experiment, we have presented every data point we collected in Figure 2, including these ones. We have added a note stating this to the description of the results on page 11. We have also relabeled delta. The optimal choice for the closest distance is to stand equidistant between the hoops, but it makes a negligible difference to performance if participants don’t stand precisely at center. In previous versions of this task (Clarke and Hunt, 2016; James et al., 2019; James et al., 2023) we calibrated the hoop distances to each individual’s throwing ability, but it adds an extra stage of collecting throwing performance data and fitting a function before setting out the hoop positions that didn’t fit with the protocol for E1. We have enough data from these previous tasks to be confident that participants will all be well over 50% likely to hit the target in the near locations, and well under 50% in the far condition, which is the threshold for switching between a divide (central) or focus (side) strategy.

*The DV that is tested to compare groups is not a very precise one -- it is the difference between the NSP for the largest and smallest between-hoops distance. But if NSP=2 and 1 for these task types, the difference would equal to 1, and the authors (or at least the data analysis) would conclude that responding was optimal.*

In shifting from using an NHST framework to a Bayesian one, we no longer use this difference score as a benchmark (but just for the record, for that comparison the 7 trials where NSP>1 were removed (and even if they hadn’t been, they would not be very influential).

*p. 9. Also, why use a Welch's test? When researchers perform a less-obvious test without presenting any justification, and obtain a p value of .048, it can give the impression that p-hacking might have occurred.*

We were predicting unequal variance between groups (one optimal with low variance, and one sub-optimal with high-variance), so this test was the most appropriate. In any case, the point is moot because, following advice from another reviewer, we have moved to using Bayesian analysis.

*Performance. There is no need to simulate optimal performance for a simulated group of 32 participants -- by assumption optimal performance is 50%. The simulation merely provides an inaccurate estimate of 50% - it adds no useful information. Also, the text does not give information on what statistical tests were used to support the conclusions in this paragraph. Regarding Figure 3, here the figure caption may be doing too much work, and the text too little. Also, here the three distance conditions are called "close", "medium" and "far" - better to use consistent terminology in all figures.*

We ran this simulation not to estimate the mean (yes, it’s 50%), but the variance. Nonetheless we have modified the figure to remove it, to avoid confusion. We have, as noted above, extensively revised the analysis to better suit the measures we’re using, fitting a hurdle-lognormal model instead of a mean/normal distribution.

About Figure 3 (now 4): We used the physical hoop separation in the figures showing empirical data (2 and 4). For the figure showing the fixed effects from the model (what is now Figure 3), we have used “near” and “far” to reflect that these are now estimates of effects, and also because can use the same labels for fixed effects from Experiments 1B and 1C, even though the physical distances were slightly different (but not different enough to change what strategy should be used). We have shortened the caption and given more information in the text.

*p. 10. Experiment 2. The authors state: "People appear to have solved the table task choice problem using reason, but did not generalize this reasoning to new circumstances." I do not see how this conclusion follows. We do not plan out our motor movements in navigating through the world and executing simple physical actions by reason, we have learned these through "implicit learning" -- I would argue through reinforcement learning and ontogenetic unfolding.*

We apologise for our imprecise use of terminology here and have rephrased this sentence to avoid any reference to “reasoning.” Our experiments are not setup to distinguish between reasoning, problem solving, reinforcement learning, etc. And in any case, as our results are null, such distinctions are beside the point. Clearly none of the above concepts are taking place, as there is little difference between groups.

This sentence now simply says: “People appear to have solved the reaching task choice problem but did not generalize this behaviour to new circumstances”

*In the same paragraph, the role of context is invoked to help explain the lack of transfer. This seems reasonable, but no theory and no mechanism is offered to explain why context matters here (and there are no references to seminal experimental psychology work, e.g. by Gordon Bower and coauthors, on the role of context in memory retrieval).*

Thanks for this suggestion. We have gone even further back to Carr (1925). Context specificity is a very general tendency that has been known for some time.

*p. 12. The purpose and use of the "participants' individual switch point (E0)" is not explained clearly.*

Good point - we should have unpacked the logic of this in the text instead of referring to a paper that explains it. We have added a couple of sentences to the acuity mapping phase to explain this better.

*p. 13. "The main research question in Experiment 2 was whether optimal eye movement choices can be trained. To address this question, we calculated a difference score between each participant’s expected target discrimination accuracy (under an optimal model)". Please describe this optimal model. Otherwise, the meaning of Figure 5 is not clear. Also, the caption of Figure 5 refers to "Experiment 3", that should be Experiment 2.*

We have shifted this analysis to the Appendix and replaced it with the Bayesian analysis, and explained it more.

*p. 14. "The hypothesis was that participants who initially were guided to make eye movements that maximize their chance of correct target discrimination would continue to perform optimally when freely choosing where to fixate in Session 2, and should therefore differ from optimal less than the control group. However, a t-test comparing how much the two groups differed from optimal in Session 2 was not significant.."*

*Also, "There is no clear difference between the groups in terms of the distribution of choices between the center and side boxes". So the cuing manipulation on Experiment 3 was not successful. I think this may be because the cuing might affect implicit learning more than reasoning processes. Perhaps an explicit dual-process model is needed to put these results, and the studies themselves, in perspective.*

This is correct (if what the reviewer means by “not successful” is that giving participants explicit instructions about where to fixate, despite leading to higher accuracy, did not facilitate better fixation decisions in the second block). Throughout the paper we have been explicitly grounded in the idea of multiple routes to a response (habits vs actions, to use the Dickinson terms) and trying to get people to transition into “action” (i.e. teleological control). By “explicit dual-process model” does the reviewer mean a formal model? If so, that would be quite a different paper than the one we have written, but we hope our results might contribute (along with many others) to such a model in the future.

*Reviewer: 3*

*Comments to the Author*

The authors demonstrate that participants exhibit suboptimal decision-making in a location and detection task. Additionally, they find that simple interventions, such as generalizing the optimal decision strategy from pre-training to the target task, do not lead to improved decisions. Overall, I am favorable towards this research program. My impression is that insight problems are understudied in the field of decision making, and these studies contribute to the existing literature. However, there are several concerns that the authors might be able to address in a revision.

*Firstly, the sample sizes are small, although I acknowledge that there are repeated observations per participant. The paper lacks a rationale for the chosen sample sizes and a priori power analyses. I would expect the authors to include a sensitivity analysis that demonstrates the power in relation to the effect size. This is my main criticism of the studies. I am uncertain about how convincing the results are based on the small sample sizes. Both studies rather appear to be interesting pilot studies.*

We have included a *Justification of Sample size* section for Experiment 1 (page 8). Experiment 2 was pre-registered. As we note in the paper, a meaningful effect in these experiments would not be subtle or small. We are confident that adding more participants would be of little value as it would change none of our conclusions.

*Secondly, interpreting null results within a null-hypothesis framework is always challenging, especially when sample sizes are small. I recommend that the authors incorporate the Bayesian analysis for the target effect in the text.*

As recommended, we now use a Bayesian rather than frequentist framework for all of our analysis. In hindsight, the use or null-hypothesis significance testing in the original submission (and pre-registration) was inappropriate - comparisons to the null are not particularly interesting for our hypothesis.

*Thirdly, Experiment 2 is difficult to understand, and I had to look up the old reference on which the experiment was based. It would be helpful if the authors provided more information to assist readers in comprehending the experiment.*

The additional explanation of the paradigm in the introduction has, we hope, helped to make the overall framework more clear; the logic of this experiment and solution to the decision problem is the same as Experiment 1, despite the large change in context.

*Fourthly, I am having difficulty understanding figures 5 and 6. How is the expected accuracy calculated in figure 5? Additionally, how does each line represent a participant in figure 6?*

These two figures have been revised and merged into Figure 5, which no longer uses expected accuracy but instead shows their actual accuracy on the task. However, it is still the case that each line in this figure represents a participant. We are unsure where the confusion lies in this presentation, but the lines are now overlaid against a model of the fixed effect, which may provide a presentation of the data that is more conventional.

*Finally, I would like to inquire whether the authors preregistered their studies and if the data and analysis scripts have been made publicly available. Nowadays, there is an increasing expectation for transparency in research, and I believe these details should be provided.*

Yes, we pre-registered Experiment 2 and also uploaded all the data from both experiments to this repository:

<https://osf.io/deh4j>