XGE-2020-2502  
The nature of decision noise in random exploration.  
Journal of Experimental Psychology: General  
  
Dear Mr. Wang,

I am writing concerning the manuscript entitled “The nature of decision noise in random exploration” (XGE-2020-2502) that you submitted to Journal of Experimental Psychology: General (JEP:G). I received three reviews from colleagues who are very knowledgeable and highly respected experts in the topical area you are investigating. I read the manuscript prior to receiving these reviews in order to gain an independent perspective on the paper, and then again with the reviews in hand. As you will see when you read their critiques, the reviewers found that your paper is well written and addresses an interesting topic. Reviewer 3 was especially interested in the statistical model, having not seen anything like before. However, and despite these merits, they also raise major concerns that prevented them to recommend publication. I share many of these concerns, and I decided to decline your paper for publication in JEP(G). Reviewers wrote detailed and self-explanatory reviews, so I will not reiterate their criticisms in this letter, but just evoke in the following the main reasons that led me to this decision.

The first point concerns the adequacy of your article to a journal like JEP:G. I had a doubt about the interest of the question addressed for the large readership that our journal is targeting. However, as I didn’t want to make such a decision alone, I preferred to get some advices from specialists of the domain. Both Reviewers 1 and 3 express doubts about the fit for the journal. However, though important, this problem is not the main reason for my decision.

The most important point is raised by Reviewer 2 and concerns the design of your study, that this Reviewer considers as not sufficient to conclude that the variation is truly random. The rationale of your design is based on the idea that individual games, and especially the repeated games, are independent (that they stand alone as Reviewer 3 says). However, Reviewer 2 lists a series of deterministic factors that are not taken into account in your analyses and that could account for variations, especially the memory that participants can have of the recent history and outcomes of the choices already made. As stressed by the Reviewer, explicit memory is not even necessary. Reviewer 3 evokes the same problem when noting that not only memory of the first occurrence in repeated games, but also the results from previous non-repeated trials can influence behavior. This suggests that the situation in which participants are involved is far more complex and difficult to control than a series of unrelated and independent trials. This makes that, as Reviewer 2 notes, the empirical evidence you gathered is not sufficient to support your conclusion. As the Reviewer says, strong claims require strong evidence, and the Reviewers do not think that we have strong evidence here.

The Reviewers have also raised several aspects of your article that make it difficult to read, and especially the way you define the two types of noises and you explain your model, something that seems difficult to understand even for specialists of the domain.

Given these concerns, I regret to report that I am declining this paper for publication in Journal of Experimental Psychology: General. As you probably know, we can accept only small fraction of the papers that are submitted each year. Accordingly, we must make decisions based not only on the scientific merit of the work but also with an eye to the potential level of impact for the findings for our broad and diverse readership. If you decide to pursue publication in another journal at some point (which I hope you will consider), I hope that the suggestions and comments offered in these reviews will be helpful.

Thank you for submitting your work to the Journal. I wish you the best in your continued research, and please try us again in the future if you think you have a manuscript that is a good fit for Journal of Experimental Psychology: General.

Sincerely,  
Pierre Barrouillet  
Associate Editor  
Journal of Experimental Psychology: General  
  
Reviewers' comments:  
  
Reviewer #1: In this submission, Wang and Wilson endeavor to examine the sources of 'decision noise' in a well-characterized exploration-exploitation choice task—asking whether the apparent randomness in exploratory choices is attributable to 'random' versus 'deterministic' noise in the Horizon Task. By looking at repeated choices taken under the same circumstances, the authors conclude that exploratory choices bear a marked influence of 'random' noise as inconsistency between choices increases as general exploration levels increase.  
  
Aside from the severe presentational issues that I believe this work suffers from (see below), I don't believe that the rather nuanced and specialized question addressed here is sufficiently interesting to the broad readership of this journal. Beyond research specifically examining exploratory choice in specific task settings, how does the answer to this question inform any other line of psychological inquiry? I think this might be of interest to a very specialized crowd. I now turn to my issues with the way this work is presented:  
  
- In my view the most severe theoretical shortcoming of this paper is the authors' lack of clear definition, conceptually, of what these noise sources are. As an example, I struggled to understand this likely important sentence on page 2: "what we have called 'noise' in previous researches could actually just be some missing deterministic components from the model. Decision noise as defined in previous researchers are more or less a quantification of what's not predictable by the model." What model are the authors discussing here? Following this the authors mention that " this 'deterministic noise' is very much in the stimulus" (p 3) which I understand even less. I had even more difficulty following the coin metaphor illustrating 'random' noise. Without a clear conceptual-level definition of the psychological phenomena at play here, I don't see how this manuscript could even make a modest contribution to the literature.  
  
- While the analysis of choice consistency struck me as reasonable, I could not ascertain the additional the contribution of the model-based analysis taken here was. Part of this stems from the lack of information in conceptual-level model description (e.g. adequately defining two the terms in Eq 1), but also, it appeared to me that the experiment itself might be well-suited enough to disambiguate noise sources. What is the added explanatory value of the posterior density plots for parameter estimates or the model comparison exercises depicted in Figure 5--with 24 plots (!)-- which readers are not given nearly enough intuition, in the text, to interpret?  
  
- There is prior work, omitted by the authors, discussing the role of randomness in exploration , which any treatment of this question should discuss (Blanco, Love, Ramscar, Otto, Smayda, and Maddox, 2016; Otto, Knox, Markman, & Love, 2014; Blanco, Love, Cooper, McGreary, Knopic, & Maddox, 2015).  
  
Minor:  
  
- Why doesn't this submission adhere to APA standards (i.e. Methods preceding the Results sections). Was this manuscript submitted elsewhere prior to this journal with different formatting standards?  
  
- The number of mechanical/grammatical issues that I encountered while reading this leads me to believe this manuscript did not receive sufficient editing before submission.  
  
  
  
  
Reviewer #2: This paper tackles the interesting issue of trying to disentangle random variability from deterministic sources of variability in exploration during a reward-based decision-making task. The question is whether there are truly random or stochastic processes in the brain influencing these choices, or whether noise in models of choice come from deterministic components that are unaccounted for. To show that human decision-making does indeed have a truly random component would be interesting and important. As the author's note, this is very difficult to do, and I am not convinced that this paper succeeds in doing it. The authors have participants complete a modified version of the Horizon task previously used in Wilson et al. 2014. The main modification was that participants now will see each individual game twice during the experiment. The reasoning for this is that any inconsistency in how participants respond across identical games represents variability due to random processes (rather than deterministic processes which should produce consistent responses). While this is clever, I don't think that the design and analyses are sufficient to conclude that this variation is truly random. As the author's themselves note, their estimate on random noise is an upper bound (with no estimate of lower bound, meaning the actual contribution of random noise could be 0). I can think of a number of deterministic factors that would lead to variability between identical games that are not accounted for by the author's analysis--some of which should even be expected based on existing literature. Here are a few:  
  
1) game number: participants may be more likely to explore earlier in the experiment than later in order to understand the basic rules and parameters of the game better. I guess this could be instantiated in a 'random' way, but maybe not. They may also become more motivated by reward values later in the experiment as they become bored or impatient and want to do well to finish the experiment sooner.  
2) the outcome of the previous game(s): participants may be more or less likely to explore based on how well they believe they did on the previous game.  
3) the recent history of explore/exploit choices on previous games: participants are probably more likely to explore if they were exploiting frequently recently (the horizon of the previous game is also therefore a candidate factor)  
4) whether they remember the previous identical game. This one is critical since participants may simply remember the previous identical game and make a deterministic choice to do the opposite of what they did the first time. Or they may simply have learned from the outcomes of the previous identical game and now know that the other option is better. Explicit memory may not even be necessary in that case.  
  
While individual games are meant to be independent, it seems very likely to me that recent outcomes and the recent history of choices across previous games may be deterministic factors contributing to whether a person will explore or exploit. While it may not be optimal or logical in the context of this particular experiment, I'd be more surprised if exploratory behavior wasn't significantly affected these things than if it was. So things like example 2 and 3 above are real candidate factors, and there may be a number of others that each contribute small amounts. It would be hard to account for them all, but I do not think they are trivial. The possibility for learning or memory (example 4) is an even more serious problem.  
  
I think it's possible that there is a real stochastic element to human decision-making, but it is a pretty strong claim that requires strong evidence, and I don't think this paper provides it. I am not convinced. It's a clever attempt to address the problem, but it is an extremely difficult problem. I encourage the authors to continue working on it. I think it's an interesting topic, and I do think that the paper was generally very well written with good analyses, so I think there is promising potential in this line of research despite the current issues.  
  
  
Reviewer #3: The nature of decision noise in random exploration  
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The authors explore the contribution of two kinds of noise in a one-armed bandit task. On each trial, participants are asked to select one of two slot machines to play. Each machine has a different expected value (EV), which the participant needs to learn through experience. Some trials are repeated, so there are two responses from the participant on that trial. The main question is to determine the contribution of 1) random noise - noise that is truly stochastic and 2) deterministic noise - noise that is explainable, but not included in the statistical or theoretical model. On repeated trials, random noise should tend to produce different responses, but deterministic noise should tend to produce the same response. The empirical findings and a statistical model both supported this idea, and suggested a stronger contribution of random noise.  
  
The topic is interesting. The work is well done. The manuscript isn't ready for publication.  
  
I'm not 100% sure this manuscript is a good fit for the journal. Although the basic idea is of wide interest, this work reads to me as methodological (introducing a way to measure these types of noise) rather than empirical or theoretical. That is, it reads as if the empirical work is more of a test-bed for the statistical model, rather than teaching us something about exploration. Indeed, the current link to exploration is relatively weak. Maybe some re-writing would help (see below).  
  
What I liked most about this work was the statistical model. I haven't seen anything like it before and the question it answers seems of potential interest to a wide range of researchers. Thus, I would like to see the model's testing and discussion expanded. In terms of writing, it took me a while to really understand what the model was doing and how it worked. A more intuitive introduction and examples would be very useful. A general writing suggestion is to link this work to test-retest reliability, this work seems more along those lines than exploration. I liked the posterior predictive check, model comparison, and parameter recovery. I disagreed with the conclusion that the parameter recovery was "satisfactory". Recovery seemed weak to moderate, especially for the important sigma\_det parameter. Although I have far from a complete intuition of the model, it seems like a lot of the data space doesn't contribute much to the parameter estimates. For example, it seems like responses that are consistent with the difference in EV would not add much information (a power analysis would be useful). Can the authors think of a task in which every trial adds information? Maybe some sort of continuous response? It also wasn't clear if the model was tested on only the repeated trials and how many repeat trials there were.  
  
The authors assume that each trial stands alone, but results from previous non-repeated trials might matter. For example, if I just did poorly on one trial, I might act differently on the next trial than I otherwise might. That is, there are a number of factors that might reduce the influence of deterministic noise. Perhaps addressing this issue could be part of the additional model testing. What factors would affect the random vs deterministic noise? How do these two sources of noise jointly vary in the model (and show the joint distribution and recovery distribution), i.e., can they be measured independently?  
  
There are some other writing issues. In particular, I think the methods section should go before the results. It is a simple enough task, so it won't distract from the flow of the paper. Further, there is a lot of information in the methods section that is needed to understand the results and modeling. Relatedly, there is a lot of information in the paper that is scattered and hard to find, e.g., number of repeated trials, which trials were used in modeling, figures aren't always referenced (e.g., fig 2), directed vs random exploration isn't well explained or motivated, high vs low information trials aren't motivated (and their relation to the results are not well explained), on p7 what trials are the p(low/high mean) drawn from (all or only repeat), in general it was unclear which experimental trials were used in the analyses and modeling, the basic question of why deterministic noise would increase with horizon wasn't well motivated.  
  
The random noise vs decision noise distinction makes sense, but is poorly named and explained. It took until the modeling section for me to fully understand it. The examples in the introduction didn't help. Also, the discussion surrounding the definitions and differences between these two sources of noise is very brief. For example, talking about intrinsic variability in the brain is a rather loose explanation These are deep questions. I'm not suggesting the authors make this a philosophical paper, but perhaps they could define the noise more succinctly (and model-based?) to avoid such issues.  
  
The captions tend to explain the results rather than explain the figures.  
Fig 4 lower left panel has tools in it.  
P3 capitalize hence.  
P4 missing a ) after high info.  
P8 extra . After horizon 6.

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