CAROLINA WREN MANUSCRIPT REVIEW HISTORY
REVIEWS (ROUND 1)

Editor Decision Letter

Thank you for submitting your manuscript to the Journal of Consumer Research. Two experts and an AE read your manuscript and provided helpful suggestions. A third reviewer disappointed us by failing to submit a timely review. However, the consistency among the review team in their assessment of the manuscript gave us confidence in moving forward with the review process. I have also read your paper and the reviewers’ comments carefully in coming to a decision on the manuscript. This letter provides my decision.

We like this paper. The ideas are interesting, the studies are persuasive, and the writing is clear and easy to follow. I would like to offer you the opportunity to revise the paper. I do not view this revision as major. However, it remains to be seen whether the new data that are requested will align with your theoretical predictions. Given this uncertainty in results, I am unable to offer anything more than a revision invitation at this time. However, what remains to be done is very straightforward as indicated below (and reinforced in the AE’s report):

1. Please run the study recommended by Reviewer B.
2. Please address Reviewer B’s comments regarding study 2’s results.
3. I understand Reviewer A’s comment about study 1. However, I agree with the AE that retaining it is fine as long as it can be justified.
4. Like the AE, I like the Decision Quicksand title. Moreover, I believe that the metaphorical associations linked to quicksand are consistent with the time sink and effort of removing one’s self only to fall deeper that you empirically observe. As such, I am fine with keeping the current title.
5. Please address Reviewer A’s comments regarding the decision to engage in a trivial decision in the first place in the Discussion section.

If you choose the revise the paper (and I hope you do) it will be assigned to a new Editor, because my term ends on June 30. However, I will ask the incoming editor to have your paper reviewed by the same review team. As such, please submit an overview report indicating how you have addressed the comments noted here and in the AE report.

Congratulations on a fine initial submission. We appreciate your submitting your work to JCR and look forward to seeing your revised effort.

AE Report
Comments to the Authors:

This paper argues that when trivial decisions seem difficult we can find ourselves “sucked in” or spending far more time than we might otherwise like on them. We asked three reviewers for their perspectives. Unfortunately, one of the three reviewers was unable to complete their review in a timely manner. I debated soliciting a replacement reviewer but given the high correspondence between the two reviewers and myself decided it was unnecessary. All three readers thought the idea underlying the paper was quite interesting and novel. Both reviewers recommended with provide an opportunity for a revision and I agree with this assessment. As I’m continuing on as AE I’ll be happy to handle the revision although the editor may be replaced by one of her successors (of course, this presumes the editor feels a revision is warranted).

My report can be very brief. I think reviewer B has nailed what will make a decent paper a really good one. You need a study along the lines he/she lays out. This should be a very easy study to run, and it seems that it should flow directly from your theorizing, so hopefully its not a controversial suggestion.

Reviewer A and I would like to see a little more work in the GD discussing why this interesting phenomenon might occur (e.g., why do we do something that seems to make so little sense). Reviewer A believes study 1 can be dropped. I’m more mixed in my perspective. Please either make it more clear why it fits and is necessary or drop it from the new package.

A minor point, but Reviewer B doesn’t like the current title. I actually quite liked it. I’ll leave it to you whether you stick with it or shift.

Best of luck with your research.

Reviewer A

Comments to the Authors:

The authors of this manuscript suggest that people become mired in unimportant decisions that seem unexpectedly difficult. They argue that this effect is stronger for trivial decisions, because people don’t expect trivial decisions to be difficult, whereas they draw weaker inferences when, consistent with their expectations, an important decision is difficult.

The manuscript is easy to read and well argued, and the authors document the effect convincingly in their four studies. I have only relatively minor suggestions about how the manuscript might be improved.

First, the effect rests on the idea that people gauge the importance of decisions (as trivial or important) before they begin investing energy in making those decisions. Once people have decided that a decision is trivial, why should they even consider investing heavily in the decision at all? Decades of research suggests that people won’t invest heavily in decisions that seem trivial or provide insufficient motivation, and since the effect relies on this initial calculation, I’m
surprised that people devote resources to the task when they’re originally convinced that it’s a trivial decision. This isn’t a criticism so much as a call for further discussion—why might decision-makers who already relegate the decision to the “trivial” pile subsequently spend so much time on the decision, even if it feels unexpectedly difficult? Do they revise their assessment of its importance (upgrading it from trivial to important)? Or is the whole process implicit, so they continue to believe the decision is unimportant, but somehow feel compelled to invest considerable time anyway?

Second, though the package of studies is compelling, the first study is considerably weaker than the others. The importance manipulation seems to manipulate other constructs, and I’m not sure it really manipulates importance particularly well at all. Revocability is very different from importance—some revocable decisions are critical, and some non-revocable decisions are trivial, and conflating the two dimensions seems to admit alternative explanations. For example, merely giving people the option to revoke a decision might prompt them to think more deeply to later avoid having to ruminate over the soundness of their initial decision—and this effect might be more pronounced for trivial decisions because they’re loath to reconsider seemingly unimportant decisions later. As the authors note, the easy and difficult decisions are substantively different—the content differs between them—which means that the decisions actually demand different resources and might also seem differentially important based on their content alone. (I’m also not sure the study is a “field demonstration” as the authors label it on p. 9, but that’s a minor point.) As the authors ultimately admit, Experiment 1 is not about metacognition at all, and I feel the package might be more compelling in the absence of that first, more transparently flawed, experiment.

Reviewer B

Comments to the Authors:

The studies show some surprising results. And the theory is quite surprising. The main novel prediction of the theory seems to be that decision difficulty can extend decision time, but only when that difficulty is unexpected. That seems like the explanation for the most surprising result that unimportant decisions take longer than important ones when both are difficult. One weakness in the evidence for the theory is that the authors never manipulated expectations of difficulty directly, but rather manipulated importance with the idea that any differences would be the result of importance influencing expected decision difficulty. Here is a design that if it worked, would be much more persuasive for the key part of the theory: give everyone a difficult choice. Tell half the participants that this is an easy choice and tell the other half that it is a difficult choice. I think you would predict that the decision should take longer when the expectation is that the choice will be easy.

Clarify study 2 results: One question here is whether the manipulation of font overwhelmed the direct manipulation of importance on their measure of importance. If it did, that would be surprising, but consistent with their theory. If it did not, the theory, which relies of difficulty operating through perceived importance, would be inconsistent with these data. Please report all means for perceived importance, including the high importance cells.
The authors focus on the effect of difficulty within low importance decisions throughout, but the more surprising comparison seems to be within the difficult decisions, where they fairly consistently find that unimportant decisions take longer than important ones. In other words, the fact that unimportant decisions take more time when they are difficult vs easy could be predicted from a variety of existing theories, but the fact that difficult decisions take more time when they are unimportant compared to important is a very interesting finding. In fact, it must be the case that hard decisions will not always take more time when they are unimportant vs important, so the authors could add some thoughts about the boundaries of this effect. In addition to the means requested above, it would also be useful to change the moderated mediation analysis to examine the effect of importance on decision time within the high difficulty conditions.

The title seems a bit overstated – the data you have do not directly deal with the idea of quicksand, which I imagine to be a spiraling of unimportant decisions into very lengthy processes.