

Reply to PCIRR 2nd decision letter:
Dinner et al. (2011) conceptual replication and extensions

A track-changes comparison of the previous submission and the revised submission can be found on: <https://draftable.com/compare/KwcsKVWlaIxd>

A track-changes manuscript is provided with the file:

“PCIRR-RNR2-Dinner-et-al-2011-eplication-extension-mainmanuscript-trackchanges.docx”
(<https://osf.io/rv2nm>)

Reply to Editor: Dr./Prof. Chris Chambers

Two of the three reviewers were available to review your revised manuscript, and we are now very close to being able to award Stage 1 IPA. There are two remaining minor points to consider in the review by Laurens van Gestel -- one concerning a suggested nuance to be added to the introduction (which seems sensible to me) and another concerning the weight that should be attributed to informal claims that have not been published in the peer-reviewed literature. My own personal view is that the academic community sometimes places too much relative weight in the veracity of claims made in peer-reviewed journals compared to other sources (such as preprints), but I agree with the reviewer that the finding being presented only in the way it has makes it difficult to evaluate. I think the best solution in this case is not to edit out the (informal) reference to Thaler (2021) but to acknowledge (perhaps in a footnote) just how informal it is, and that the claim cannot be fully evaluated. That way the reader will be in the best position to make up their own mind.

Thank you for the reviews obtained, your feedback, and the invitation to revise and resubmit.

We appreciate the constructive suggestion for how to quickly resolve the issue. We added the following footnote:

We note that we were unable to identify the source for the data Thaler presented in his talk, and therefore have not been able to evaluate or verify its claims.

I recognise that you are on a tight study schedule, but hopefully these remaining edits should take very little time to make. In the meantime, I will begin preparing the Stage 1 recommendation so that we issue IPA immediately following receipt of the final revised submission.

Thank you, we are looking forward to the IPA and proceeding to Stage 2.

(We note that when finalizing the survey, we realized that two exploratory measures added following the discussion in the last revision were not clearly elaborated in the manuscript. These were meant to prompt participants' knowledge of United States federal and state CFL bulb phase-out. We therefore clarified those in section "Exploratory: Knowledge of federal and state CFL bulb policy change")

Reply to Reviewer #1: Dr./Prof. Laurens van Gestel

I am happy to see that the reviews contributed to redesigning the study and that the authors appreciated the reviewer comments. I think the manuscript has improved a lot, and am confident that the current study will be a valuable contribution to the literature. The reviewers have extensively replied to most of my comments in a satisfying way.

Thank you very much for your support and the positive opening note.

There is one point of concern remaining for me, which relates to the discussion of the literature on defaults effects, which is a point I raised before but, in hindsight, did not elaborate on enough. I can follow the authors' reasoning, but would have liked to see a clear distinction in the discussion of the literature when it comes to literature on defaults specifically (e.g., Jachimowicz et al., 2019) versus nudging more generally (e.g., Mertens et al., 2022). The authors now added a definition of nudging in their revised manuscript, and rightfully indicate that nudging should be seen as an umbrella term and I think this is important when discussing the literature.

The discussion in the manuscript about publication bias in research on nudging in general is rightfully mentioned in the manuscript (Maier et al., 2022; Szaszi et al., 2022), but it is also connected to claims about publication bias about defaults specifically. To be clear, I would not be surprised if publication bias is also present in the literature on default nudges, but the only peer reviewed work that I am aware of on exactly this topic currently (perhaps strangely enough) suggests the following: "If anything, our publication bias analyses highlight that larger effect sizes are underreported, suggesting that researchers may not bother to report replications of what are believed to be strong effects." (Jachimowicz et al., 2019, p. 174). Moreover, Szaszi et al. (2022) state: "Until then, with a few exceptions [e.g., defaults (6)], we see no reason to expect large and consistent effects when designing nudge experiments or running interventions."

Therefore, I think it's important to disentangle discussions about the literature on nudging in general and defaults as a specific type of nudge,

and would like to suggest the following nuance to be added in the manuscript:

"Several recent analyses concluded default effect as having medium to strong effects, and among the strongest of the “nudge” interventions (Hummel & Maedche, 2019; Mertens et al., 2022a; Jachimowicz et al., 2019) though follow-up critics showed that the literature [on nudging in general] is heavily affected by publication bias and that effects adjusted for that seem much weaker (Maier et al., 2022; Szaszi et al., 2022).

Thank you for the suggestion, we adjusted accordingly.

Finally, regarding the graph mentioned in the talk by Richard Thaler, this is very important data that we should take seriously, but I can't assess the scientific quality of it as it has not been published in a peer reviewed journal (at least, not that I'm aware of). I find this a rather difficult point to raise but, as much as prof. Thaler is to be respected, I believe we cannot build on this graph for the moment. The authors now rightfully acknowledge that this is based on informal reports, but by adding an argument to it (which by itself is plausible, but not published in the literature), it does become quite substantial. I therefore think that this paragraph needs revising, but am also open to leaving it up to the editor to decide what to do with citing unpublished work. Moreover, if I missed out on something or am wrong in this regard, I would be totally happy to withdraw my concerns about this issue.

Yes, we understand. We followed the recommendation by the recommender/editor to add a footnote indicating we were unable to identify or verify the source.

Reply to Reviewer #2: Dr./Prof. Sylvain Chabé-Ferret

I find the round 2 manuscript of the Stage 1 RR to be much improved. A lot has been clarified. I also welcome the removal of the treatment condition related to past behavior and the addition of the neutral manipulations for the two remaining conditions (status quo and default).

As is stands, I think that the study can go to data collection and I recommend that the Stage 1 report be accepted as is.

Much appreciated. Thank you for the positive supportive note.