I sign my name as I support reviewer transparency: Gilad Feldman

Thank you for inviting me to review this manuscript. I'm happy to take part in a Registered Report and especially in the wonderful PCI-RR initiative.

Editor/authors are welcome to use my review as they please, post/share it etc. I waive any copyright that I might hold to these.

Disclosures related to this review and my background, I believe it's important to state clearly what my expertise is so that editor can take my comments in perspective and authors can decide what they'd like to take away from what I write:

1. My work is cited in this manuscript, with several failed replications from the literature on victim identified victim effect, and we have a few more that aren’t cited. I came into this literature hoping to find a solid phenomenon but my views on this domain have changed given our evidence, and I feel like we should work as a community to better define terms, like differentiating singularity (or better yet, scope insensitivity) from identifiability. Some might see my work in this domain as a conflict of interest in reviewing, and so I feel like this should be taken into consideration. That said, given that this is a Registered Report, I feel less conflicted.
2. My reviewer "code" is posted on: <http://mgto.org/reviewing/> . Many of my comments will reflect my support for open-science and the need for reviewers to urge authors to do well on open-science practices.
3. My comments are NOT meant as criteria for rejection, but rather as assistance in helping authors do better. I aim to help and support you, not hinder authors. I offer my review as a guide for improvement, and I am very happy to work with the authors to help them in this journey. They are welcome to reach out to us and ask further questions, and I'll do my best to help (giladfel@gmail.com). I leave it up to the editor to decide on which things s/he thinks is a must for you to implement. I typically aim to see potential in any manuscript.

I would like to start by saying that I would eventually want to see this project proceed to an in-principle acceptance and a Stage 2 publication. I like the direction and idea, and I feel like this would be a valuable project.

== Notes/comments/suggestions ==

You wrote:

Although this effect appears to be highly context-dependent (Kogut, 2011a; Kogut & Ritov, 2005b, 2005a; Moche et al., 2022) and has recently failed to replicate (Maier et al., 2024; Majumder et al., 2022), a great body of evidence indicates that, in general, it is advantageous for victims to be identified (see Lee & Feeley, 2016 for a meta-analytic review).

If you look carefully at our Maier et al. (2024) you’ll see that we conducted a re-analysis of Lee and Feeley (2016). First, you’ll notice that the effect of Lee and Feeley (2016) is generally weak, but our re-analysis showed evidence for publication bias which once corrected for shows no effect. This is how we summarized it in the most recent preprint version of Majumder et al. (2022) which is still in the review cycles:

Two findings are especially noteworthy. Lee and Feeley (2016) conducted a meta-analysis of 41 effects from 22 experiments and concluded only a weak identified victim effect (*r* = .05), with the three highest-powered studies in the dataset showing effects that are almost zero (e.g., one study: 12,802 participants, *r* = 0.004). Further statistical analyses on the studies included in Lee and Feeley (2016) by Maier et al. (2023) uncovered “moderate evidence of publication bias and strong evidence for the absence of an identified victim effect (BF01 = 14.93), with a model-averaged mean effect size estimate of *r* = 0.002 (95% CI [0; 0.004]).”. In that same article, Maier et al. (2023) also reported a failed conceptual replication of the seminal study by Small et al. (2007).

In addition, most of the citation you used from this literature are fairly outdated. In the recent version of our preprint, we added many more recent citations of failed and mixed results studies, some of which are summarized in this table:

Table 1
*Identifiability and singularity/compassion-fade: Mixed findings in the literature*

| **Article** | **Factor** | **Main quote regarding**  |
| --- | --- | --- |
| Byrd and Białek (2021) | Identifiability + Singularity | Concluded no strong support for the identified victim effect on compliance with public health recommendations. |
| CORE & Feldman (2023) | Singularity / Compassion fade | No support for singularity; people donate larger proportions when there are more affected children. |
| Erlandsson (2021) | Singularity | No support across all decision modes for a clear preference for saving a greater number of non-identified victims in joint evaluation and forced choice. |
| Gordon-Hecker et al. (2024) | Singularity | Empathy depended on the pain experienced by each individual but not on the number of individuals in the group. |
| Hagman et al. (2022) | Singularity | Compassion was higher for the eight than for the one child, both in the help request and no-help request conditions. The interaction term between help requests and the number of victims was not significant. |
| Hart et al. (2018)  | Singularity | In both Study 1 and Study 2, no significant differences were found between the individual and the group conditions. |
| Lee and Feeley (2016) – meta-analysis | Identifiability | A meta-analysis of the literature until 2015.Summarized a statistically significant yet very small average effect size of *r* = .05 |
| Maier et al. (2023) – meta-analysis re-analysis | Identifiability + Singularity | A reanalysis of the meta-analysis by Lee and Feeley (2016). Found moderate evidence of publication bias, adjusting for which, there was no evidence for the IVE, with adjusted effect down to *r* = .002  |
| Maier et al. (2023) replication of Small et al. (2003) | Identifiability + Singularity | A failed replication of Small et al. (2003). No support for identified victim effects. |
| Moche and Västfjäll (2021) | Identifiability | In three studies (overall *N* = 1508) with different samples from different countries, there was no main effect of identifiability on any of the measures. |
| Moche et al. (2022) | Singularity | Weak effects, with just below the alpha threshold for the Israeli sample, and in the Swedish sample, a signal for WTC was detected but not for actual donations. |
| Moche et al. (2023) | Singularity | Three adults elicited more help than one adult. |
| Moche et al. (2024) | Identifiability | IVE is contextual and much weaker than originally thought. |
| Morvinski and Gordon-Hecker (2023) | Singularity | Adding a photo has a positive effect on donations, yet results are inconsistent with singularity effect and scope insensitivity. |
| Vu et al. (2024) | Identifiability | No effect of identifiability on altruism |

You’ll also notice the differentiation between singularity and identifiability which we summarized in this way:

We considered the identified victim effect as an umbrella term that combines several phenomena, which scholars in this literature at times tend to confound. Some scholars emphasize “identifiability” - whether the victims are identified or not, whereas some emphasize the number of people affected, which we refer to as “singularity” - contrasting a single person to a group dichotomizing two related phenomena of “compassion fade” and “scope insensitivity” looking at the number of people as a continuous factor.

Requests:

1. Be more humble and cautious about this effect.
2. Cite not just the classics, but also the more recent mixed/failed evidence.
3. Be very clear about focusing on identifiability, and disentangle that from citations that conflate singularity or scope insensitivity.

I think you might have similar issues with offender identifiability. I would generally suggest caution and humility looking at the evidence from the classics and aim to focus on more recent well-powered pre-registered studies, and leave open the question of what to expect here.

Here are a few additional comments/notes/suggestions, as I read through the methods/results/supplementary:

1. Identifiability:
	1. Looking at your materials , like in the mixed evidence literature, your identifiability is not just identified vs. unidentified, but also whether there is a photo. This is likely to create a strong effect and impact, do note that you will not be able to disentangle the use of photo from identifiability. No need to add another condition, this is already ambitious, just need to note this and be cautious about how to interpret the differences between the two conditions and what it is that they manipulate.
	2. Beyond that, I don’t have much to say about the introduction and/or value, other than that it’s interesting.
2. Model
	1. There is a lot going on this in model, with moderation and mediation (several suggested mediators).
	2. This needs a lot of power, I don’t think you’re powered for that, more on that below.
	3. A diagram summarizing all the links in one figure would be helpful to summarize the already good layout of the hypotheses.
3. Power analysis
	1. Your power analysis doesn’t take into consideration the mediation model. I know it’s complex, but if you can’t tackle that well, might want to acknowledge that, and focus the power analysis on the core hypotheses, and make the mediations secondary and exploratory. I have doubts about this sample being able to tackle such a complex model.
	2. Reproducibility: Please include the script/screenshots from the power analysis.
	3. Gpower sometimes has issues with interactions (especially mixed, which is not the case here), and so can sometimes be underpowered. You might want to double-check with R packages/tools like ANOVA/super-power: <https://shiny.ieis.tue.nl/anova_power/> that would run the simulation for you. <https://github.com/arcaldwell49/Superpower> ; <https://aaroncaldwell.us/SuperpowerBook/> and if you pursue that please make that reproducible code/screenshots so that it’s clear what was the input and output of the analysis.
4. Exclusion:
	1. Please state clearly which participants you’ll exclude. Is it index >2 for too fast?
5. Attention checks:
	1. From what I see in the materials, these appear on Page 8. If this doesn’t come with the scenario, then this might be more of a memory test than an attention test.
	2. Please indicate the attention checks clearly in the manuscript. Looked like you’re expecting 10% exclusions, is that your common experience with these checks?
6. Materials:
	1. Any reason not to display the questions together with the situation on the same page? Participants have a tendency to move forward quickly, would they have the chance to go back and recall, if not – why not show these on the same page?
	2. You wrote “In the offender identified condition, offenders will be identified by name, age, and picture (taken from the Basel Face Database; Walker et al., 2018). Names, ages, and pictures will be randomly selected from six possible variations each. Gender of the offender will be counterbalanced. “. In the supplementary I see only one, maybe I missed something. Please, make =everything= you’re planning to run available. We can’t help you ensure this is the best plan unless we see everything you’re planning.
	3. English: It looks like you mostly used items from published scales, but some of it reads a bit off, so perhaps it’s my bad English or it’s lost in translation. I’m not a native speaker. I would recommend carefully examining some of the questions or simply using similar questions used and validated in this literature in English. For example: “hard treatment”, maybe “harsh treatment”?. “I am angry at the offender” I think might be better with “I am angry with the offender”, as some websites write “Angry with is used when referring to people or animals… Angry at is used when referring to objects or nouns (not people or animals)”, but it might not matter, not sure. In any case, just suggesting that you double-check.
7. Planned analyses
	1. I would strongly recommend that you simulate data and include the planned R code analysis on that. We do that in all our Registered Reports, but we have it easy, because we use Qualtrics and that creates simulated data in seconds. This has a lot of advantages, in that it helps us all align, and sometimes reviewers can improve our code or catch mistakes. It also makes the plans more strict, so that we know exactly what we pre-registered to do. Vague descriptions of an analyses are very different from the specific planned code that runs it on simulated data.
	2. I think in general Welch t-test is a good default, might make life simpler. (<https://doi.org/10.5334/irsp.82>)
	3. Given the mixed literature, are you sure you want to run a one-sided test? In any case, please plan to report the effects and confidence intervals, and plot everything for us. A simulated data/code could help us get a better idea of what the final results section would look like.
	4. Given the mixed literature, you might want to prepare for null effects. What would you do to quantify support for the null? Might be good to prepare a plan for equivalence/Bayesian in advance.

It's important for me to state again that I aim to help and support you, not hinder you. I see the reviewers' job, especially in Registered Reports, as helping authors do better. I am happy to review follow-ups, help with any of these, and help you get this manuscript to the level needed for an in-principle acceptance. All these are suggestions to improve, not reasons for rejection. I leave it up to the editor to decide on such things and what s/he thinks is a must for you to implement. I typically aim to see potential in any manuscript, and I believe all evidence should be shared openly. You are welcome to contact me anytime if you have questions (giladfel@gmail.com)

Good luck!