**0. General advice**

The document deals with the subject in a relevant manner. The theoretical arguments are clearly presented and provide a good understanding of the topic. On the whole, the paper in Stage 2 is a good continuation of what was discussed in Stage 1. The results presented, as well as the figures and tables, are clear and enable a good understanding of the conclusions the article seeks to share. However, here are a few suggestions for points to watch out for, which I hope will enable you to improve this article.

**1. Major issues**

***1.1. The comparison of the “work” and “caregiving” contexts in the suggested pay***

The arguments presented concerning the moral dimension and the differences in terms of intrinsic motivations of the “work” and “caregiving” contexts are, in my opinion, very relevant in giving readers avenues of exploration for future research on the differences in moral judgment between these two contexts. For example, as mentioned in the article, if participants feel that caregiving is done while being paid less, it's because they probably feel that other (perhaps more intrinsic) motivations drive them to perform these tasks. This could in fact have an influence on participants' perception of these different protagonists and thus explain the differences between the “work” and “caregiving” contexts.

The results showing a suggested difference in wages between the “work” and “caregiving” contexts do indeed support these arguments. However, in my opinion, it is very risky to present these results as they stand.

The limitation you mentioned regarding the results showing a difference in the wages awarded between the “caregiving” and “work” contexts is indeed very true. The reference salary may well have had a strong influence on the salary awarded in these two contexts, and this constitutes a confounded effect which prevents us from correctly concluding that there is a difference in responses between the “work” and “caregiving” contexts. It should also be pointed out that including this “context” variable (work vs. caregiving) in the suggested wage predictor model is not consistent with the fact that the 2 studies are presented as independent and distinct, nor with the fact that the other models tested to predict the other VDs do not include this variable. Moreover, the use of this variable was (unless I'm mistaken) not presented in Stage 1.

Insofar as the conclusions that can be drawn about the context variable (work vs. caregiving) are limited by the existence of a confounded variable, and that it breaks the coherence of other components of the item, is it still relevant to present these results and conclude on them?

Even though these results would be interesting to illustrate, I recommend that the authors remove them so as not to overcomplicate the article and mislead the reader.

In the “limitations” section, it may be pointed out that, due to the design of this work as two separate studies, the comparison between the two contexts has been rendered irrelevant, and that future work is recommended if these two contexts are to be compared.

***1.2. Explanations of the differences with Celniker et al. (2023)***

With regard to the unexpected findings on partner choice, it is highly relevant to mention the difference in context with previous studies: Celniker et al. (2023) did indeed focus on partner choice in the context of a game, whereas the present studies focus on partner choice in a context of cooperation in the world of work or caregiving. In order to guide future studies on the subject to possible explanations for these differences, it may also be relevant to mention another difference between the 2 studies. This difference lies in whether or not the measure of partner choice refers to the same task on which these potential partners are presented. In the present studies, the protagonists are presented as making efforts or not in the same domain as that on which the participants will be led to imagine themselves cooperating (e.g., a cooperation task that remains affiliated with the world of work). This is not the case in Celniker et al. (2023), where the partner is presented as doing or not doing a lot of effort in sports tasks (marathons), but where the choice of partner is measured in a “trust game” task that does not belong to the same domain as marathons. This difference in protocol may be relevant to the discussion.

**2. Minor issues**

***2.1. Information in the “sample” section***

In the participants section, it might be useful to specify information on the socio-economic status of participants, as well as their level of education (these data are collected in the questionnaire).

***2.2. Typing error***

At page 8, in “This observation was replicated well, yet it appears to vary between cultures in magnitude (Mexico: d = .14–.28, Germany: d = .34–.37, France: d = .38, US: d = .60, South Korea: d = .71, (Celniker et al., 2023; Tissot & Roth, 2025).”

There's an extra parenthesis just before “Celniker”.

**3. Conclusion**

The article makes an interesting contribution to the literature. In particular, it extends the “effort moralization effect” to the context of caregiving and it clarifies the framework in which the efforts observed affect our appreciation of individuals and our desire to work with someone (thanks in particular to results that diverge with those of Celniker et al., 2023). These studies also show that effort moralization effect appear as not moderated by gender. I would still recommend that the authors clarify the points made above, in particular the comparison between the contexts of “work” and “caregiving”.

On a personal note, I would recommend a resubmission and adjustments by the authors with a view to future publication.