April 7th, 2025

To Dr. Francis Cheung

Associate Editor

*Personality and Individual Differences*

Dear Dr. Cheung,

We would like to express our gratitude for the opportunity to revise and resubmit our manuscript titled “Linking Trait Gratitude to Employees’ Performance and Work Motivation: a Two-Wave Longitudinal Study” (PAID-D-25-00036) to *Personality and Individual Differences*. We greatly appreciate the time and effort you and the four reviewers invested in providing us with your thoughtful comments.

My co-authors and I have considered the suggestions made and have incorporated them into the manuscript. We hope that we have successfully addressed all the concerns raised and improved the clarity and quality of the manuscript.

Please find below a point-by-point list of responses to the reviewers’ comments. We highlighted the chages made to the manuscript using colored text.

Thank you again for considering our manuscript.

Best regards,

the authors

Thank you very much for your recent submission to the Personality and Individual Differences entitled Linking Trait Gratitude to Employees’ Performance and Work Motivation: a Two-Wave Longitudinal Study (PAID-D-25-00036). I have been assigned as the Action Editor and have now received reactions from four reviewers assigned to comment on your manuscript. I have also read the manuscript independently of the reviewers’ comments.  
  
The reviewers and I agree that the topic is interesting. However, there are a number of problems that substantially limit the contribution of this research. These issues related to the theoretical clarity and conceptual development (e.g. Reviewer 1#1, Reviewer 3#3, & the section of theoretical background), statistical treatment (e.g lacking power analysis, Reviewer 1 #2), interpertation of results and conclusion (Reviewer 5). Based on the evaluation from the Reviewers and myself, I am sorry I could not accept your paper. However, I would like to invite you to revise it and resubmit it to the Journal.  
  
If you do decide to prepare a revision, please be sure to include a cover letter that describes in detail how each of the reviewer’ concerns were addressed in the manuscript.The final decision regarding whether the paper can be accepted will be based on the reviewer’s evaluation of your revised paper. You can find al the comments at the bottom of this letter. If you decide not to resubmit a revised version, please let me know so that the Journal can close the file for this manuscript.  
  
I look forward to hearing from you and thank you again for considering Personality and Individual Differences as an outlet for your research.  
  
Sincerely,  
Francis Cheung  
Associate Editor, Personality and Individual Differences  
  
  
  
  
**Reviewer #1:** Thank you for the opportunity to review this manuscript. Overall, it presents an interesting perspective on the link between trait gratitude and job performance through motivation mechanisms. However, I have identified several limitations that, in my view, should be addressed through additional analysis or, at the very least, clearly acknowledged before publication.  
  
Major Comments  
  
1. In the introduction, the statement "To our knowledge, only two previous studies explored job performance as a potential outcome of trait gratitude (Cortini et al., 2019; Li et al., 2022)" feels too brief. It would be beneficial to expand on this very early in the manuscript by providing a more thorough discussion of these studies, including what they specifically examined, their key findings, and limitations they noted. This would help establish a clearer foundation for your study and its contribution from the start. Expanding on these points directly in the introduction will better highlight the relevance and unique contribution of your study.  
  
2. The description of the indirect effect of gratitude on performance as "marginally significant" is problematic. Statistically, an effect is either significant or not, and referring to it as "marginally significant" can be misleading, especially in the abstract without further context. This also raises questions about whether the study had sufficient statistical power to detect this effect reliably. A power analysis appears to be missing, and it would be essential to include one to clarify whether the sample size of 246 employees provides adequate power to detect small-sized effects. Based on the very small reported effect sizes (e.g., β = 0.03), it seems plausible that a larger sample would be necessary to detect the hypothesized indirect effect with sufficient power. If the current sample is underpowered, the indirect effect should not be overinterpreted, and caution should be exercised in its communication. If there is no effect of gratitude on job performance this can also be an interesting finding and communicated as such. The very small mediation effect also raises concerns regarding practical organizational implications.

Thank you very much for your observation. The total number of model parameters was 97, and the path model was estimated to use 39 degrees of freedom starting from n=246 cases, and α=.05, based on RMSEA (MacCallum, Browne, & Suguwara, 1996; Jak, Jorgensen, Verdam, Oort, & Elffers, 2021). We used ε0=.05, ε1=.01 as benchmarks for the RMSEA assumptions and the power of the full model was 1-β=64.74%. Since the fit indicators are borderline and the data volume is relatively small for such an analysis, we considered the model as plausible because the risk of committing a Type II error would have been too high. Chances are that replicated research on a larger volume would generate a model better supported by the data.

* MacCallum, R. C., Browne, M. W., & Sugawara, H. M. (1996). Power analysis and determination of sample size for covariance structure modeling. Psychological Methods, 1(2), 130–149. doi:10.1037/1082-989X.1.2.130
* Jak, S., Jorgensen, T. D., Verdam, M. G., Oort, F. J., & Elffers, L. (2021). Analytical power calculations for structural equation modeling: A tutorial and Shiny app. Behavior Research Methods, 53, 1385–1406. doi:10.3758/s13428-020-01479-0

3. The model omits a stability path for gratitude from T1 to T2, which is crucial for determining whether gratitude truly has an incremental effect on T2 outcomes. Without controlling for the stability of gratitude, the observed relationships between T1 gratitude and T2 constructs (e.g., motivation and performance) could simply reflect shared variance due to correlations at T1 and the stability of gratitude over time, rather than true incremental predictive effects. Including this stability path is in my view essential to isolate the unique contributions of gratitude and to draw valid conclusions about its causal role in predicting T2 outcomes, which you mention as one of the key limitations in the literature in Chapter 1.1 Employees' Trait Gratitude and Job Performance.

Many thanks to the reviewer on the comment. Indeed, we have modified the model by controlling for the stability of gratitude according to the literature specification (Cole, Maxwell, 2003), and the paper has been updated.

* Cole, D.A., Maxwell, S.E. (2003). Testing Mediational Models with Longitudinal Data: Questions and Tips in the Use of Structural Equation Modeling, Journal of Abnormal Psychology, 112(4), 558-577, doi: 10.1037/0021-843X.112.4.558

4. I can imagine that gratitude is strongly related to constructs such as agreeableness and honesty-humility. If a predictive effect on job performance had been found, I would be curious whether it is specifically the gratitude aspect driving these effects or if similar patterns would emerge for broader traits like agreeableness and honesty-humility. Given these potential overlaps, it would be important to consider including these constructs in the analysis. Running similar models to compare whether it is specifically gratitude that holds predictive value for job performance, as opposed to other personality traits, would provide a more nuanced understanding of the effects and strengthen the conclusions drawn from your findings. This addition could help clarify whether the effects observed are uniquely attributable to gratitude or if they are more broadly due to related traits. Currently, you even argue with this overlap to agreeableness, raising the question, whether this is all broadly about agreeableness or specifically about gratitude: "For example, people who are high in  
agreeableness might create better social relations which will satisfy their need for relatedness. In line with this theoretical perspective, we argued that trait gratitude could enhance employees' motivation."  
  
5. In Table 2, which describes the sample characteristics, it would be helpful to include details about the job types and industries represented in the sample. Given that the effect of gratitude on job performance could potentially vary across different job types or industries, this additional information would provide a clearer context for the findings. For example, the influence of gratitude might be stronger in customer-facing or collaborative roles, such as in healthcare or sales, compared to more independent, technical positions. Including these variables in Table 2 would allow readers to better understand the composition of the sample and consider how job and industry types might influence the relationship between gratitude and performance.

Authors' response: Thank you for your comment. As suggested, we have added more information on the job types and industries in our sample (see Table 3). We have taken your suggestion regarding the possibility that the relationship between gratitude and job performance may vary depending on the industry and incorporated it into our discussion (see page). We noted that the small sample size prevented us from making comparisons to determine if the strength of the relationship varies across different professions, but future studies could explore this possibility.

6. A limitation of the study is that both motivation and job performance were measured using self-report forms from the same sources, which introduces the potential for common method variance. This overlap in measurement could artificially inflate the associations between performance and motivation, making the positive relationship between these constructs appear more pronounced than it might be in reality. As a result, the observed positive associations between performance and motivation seem quite obvious and may not provide a strong test of the hypothesized relationships. To strengthen the findings, it would be beneficial to incorporate multiple sources of data for these constructs, such as supervisor ratings or objective performance measures. This would help reduce the risk of common method bias and offer a more robust test of the proposed effects.

Authors’ response: We completely agree with the reviewer’s comment. In the earlier version of the article, we addressed the issues associated with using self-report measures. However, in the revised version, we have expanded this discussion to cover the sources of common method bias, the strategies we employed to reduce this risk, and recommendations for future research to incorporate multiple data sources (please see pages 14-15)  
  
Minor Comments  
  
"According to the SDT (Deci et al., 2017), motivation can serve as a mediating factor for in the relation between personality traits and work behaviors, including job performance." "for in" is redundant here  
  
  
  
**Reviewer #2**: The article titled "Linking Trait Gratitude to Employees' Performance and Work Motivation: a Two-Wave Longitudinal Study" delves into an intriguing and pertinent subject. The decision to explore the connection between trait gratitude and work outcomes is laudable as it fills a gap in the extant literature. The use of a recent bibliography enhances the study's credibility and relevance.  
However, there are several areas for improvement. Firstly, the author should clarify whether trait gratitude and dispositional gratitude are considered synonymous in this study. This distinction is essential for readers' comprehension and should be explicitly stated. Additionally, the hypotheses in the article are not clearly marked or numbered, making them difficult to follow. Their theoretical derivation from the literature is weak, which undermines the overall argument.  
The article mentions the discussion of the types versus the quantity of motivation in section 1.2, but this distinction is not elaborated upon in the remainder of the paper. The author should clarify if they are discussing different types of motivation or the quantity, as this is vital for interpreting the results.  
In the Method section, the measures used do not include Cronbach's Alpha values, which are crucial for assessing the reliability of the scales. The Results section also lacks sufficient details on how the two time segments were compared. The author should provide more information on the comparison process to enhance transparency and robustness. Furthermore, the path analysis used does not establish causality. The term "marginally significant" is not clearly explained. The author should include the data in the text to support their claims and clarify what "marginally significant" means in this context.  
Despite these limitations, the article has several strengths. The topic is engaging and relevant, and the use of a recent bibliography adds to its credibility. The study's longitudinal design is a notable strength, as it examines changes over time. Additionally, the article provides a comprehensive literature review and addresses an interesting topic, which are commendable.  
  
  
  
**Reviewer #3:** Thank you very much for giving me the opportunity to read your research and give feedback. The manuscript "Linking Trait Gratitude to Employees' Performance and Work Motivation: A Two-Wave Longitudinal Study" describes an interesting research area. Even though I believe the research can be relevant, I do have several concerns regarding the theoretical framework, the research and analytical design, and the overall writing. I will list my concerns below, and I hope my suggestions can be of help with the development of your research.  
  
Introduction  
I feel your introduction is quite superficial, with conclusions without proper arguments and unclear statements. Here are some more specific points of feedback:  
  
1. I miss some references in the introduction, where statements are being written which need proper citations (for example, lines 7 and 13).  
2. You mentioned previous studies with similar focus (Cortini et al., 2019 and Li et al., 2022). Only later in the theoretical framework do you elaborate on how you add to this, but it would be better to already mention it partly in the introduction or not mention it there at all, as it reads there as if there is no additional value to your research.  
3. Your conceptualization of task performance and contextual performance are phrased as if they are set definitions in the literature, but they seem very specific to certain jobs or organizational contexts. Can you distinguish between the true definitions of the different types of performance and explain how they are conceptualized in your specific context? For this, there also needs to be more information on your context which is quite limited.  
4. You conclude that "Therefore, it is essential to determine whether employees' gratitude affects task performance, contextual performance, or both.", however, that doesn't follow from the previous paragraph. It is not clear why it would be essential to determine this just because they differ. I urge you to strengthen your arguments as to why you come to this conclusion.  
5. There is a spelling error in the final sentence of the introduction: relationS.  
  
Theoretical Background  
Overall, the theoretical background needs more structure. I had to read several parts multiple times and go back to re-read purely to gain a coherent understanding of what is written. It further stays quite shallow with a specific lack of depth regarding theory.  
  
6. All separate parts of the theoretical background (parts 1.1 - 1.4) need written out hypotheses. I assumed that you ended these parts with your arguments and thus your hypotheses, but they need to be written out fully for clarity and structure.  
7. You again state that "More research is therefore needed to explore the relation between gratitude and both task and contextual performance, in order to be able to determine how trait gratitude impacts these different forms of job performance.", however, for me this doesn't follow just because they are different. Why would we need to understand the potentially differing impacts?  
8. The hypotheses of 1.1 do not clearly follow from the previous reasoning. Which theoretical contributions do you refer to and why do these lead to your hypotheses? Which theoretical framework do you draw from to support your hypotheses?  
9. Similarly, the link between SDT and your hypotheses in 1.2 is not fully clear. How do the fulfillment or frustration of basic needs explain the links between (trait gratitude,) motivation and performance as researched in your work? How do you draw from this specific theory to predict and explain your hypotheses and results? Explain this better.  
10. The table with the different types of motivation helps, but it is still quite unstructured when reading the text. It would help to have a specific part on motivation, explaining the differences between the types and how you approach these in your research.  
11. The first paragraph of 1.3 is extremely shallow. A lot of untested assumptions are posited without proper argumentation and substantial referencing. The paragraph ends with "In line with this theoretical perspective, we argued that…" but this again does not follow from the previous parts. For example, why wouldn't certain personality traits not influence the needs themselves and how much is needed to fulfill them as opposed to choosing certain environments?  
12. Why don't you have a hypothesis developed for the relations between gratitude and controlled motivation purely because of mixed results from previous research? What about theory?  
13. The final part on mediation needs more explanation than what is written. Especially as there are many mediating paths in the model, it needs to be explained well. Here, again, theory could inform hypotheses instead of not developing any based on previous empirical work.  
  
Data and Method  
The data could work to explore the proposed topic, but the lack of mediation analysis (no assessment of indirect effects) strongly limits your contributions. Especially as you claim to hypothesize mediation effects and discuss in your discussion, not actually assessing mediation is problematic.

Thanks to the reviewer for the observation. We have updated the article in line with this recommendation, and we introduced and interpreted the mediation relationship.

14. Who were the participants? The abstract and discussion describes Romanian participants. For those who are not familiar, explain more about who these people are.  
15. Why a time lag of 10 weeks? Explain the reasoning for this design.  
16. Provide example items of the measures so readers can understand how the measures would work.  
17. Why did you choose to use sums of measures instead of means for your analysis? I recommend using the means of the scores on the items, so it is easier to assess the distributions of the scores on the different measures, and how they differ, as sums are more dependent on the number of items.  
Revise your Table 3 (descriptives) accordingly.

The items we used were on a Likert scale from 1 to 5, and the Likert scale is an **ordinal scale**, not a continuous scale. It is true that responses to a Likert scale are based on a continuous latent factor, so it makes sense to talk about the mean of a latent factor, but not the mean of ordinal variables and we didn’t use latent factors in our models. On the other hand, the calculation of the total score is based on additivity and assumes summative items, and this condition is also met when using averages, except that the use of averages forces a level of measurement that does not exist in the baseline measures. The mean is a continuous variable, while the total score is an interval variable, much more appropriate to the specifics of baseline measures. Furthermore, using the mean, we would consider all values from 1.00 to 5.00 to be continuous, i.e. an infinity of values, but we do not have this level of measure in the baseline variables. Moreover, variability would be greatly reduced, contaminating the analysis of the assumption of univariate normality and continuity. According to the LISREL specification postulated by Jöreskog, the minimum amplitude for a numerical variable to be considered continuous is 14 points. In the case of the sum we can evaluate this, while in the case of the mean we cannot.

Artificially increasing the level of measurement does not facilitate the analysis of distributions, but contaminates it, precisely because it assumes that the level of measurement is one of ratio, being at most interval, and in such analyses the dependence on the number of items is not a disadvantage, but an advantage, because a larger number of items can cause a sum to be analyzed continuously, while a small number of items does not. Thus, further analysis would be much better calibrated to the specifics of the measured variables. For example, if a scale had a number of 3 ordinal items, on a scale from 1 to 3, the theoretical range would be between 3 and 9, i.e. 6 points, and these scores cannot be processed parametrically because they are integers with small variability. If we work with averages, then between 1.00 and 3.00 there would be an infinity of values, forcing a stronger level of measurement in the absence of real variability in the data. This could lead to the conclusion of using strong parametric estimators in real analyses, "fooling" the assumptions of the analysis.

In addition, the use of means or the use of sums does not affect the covariance matrix, so no other data would result from the already reported data, but it could affect the type of estimators used. If a variable does not fulfill the conditions for treating it as a continuous variable or the assumption of multivariate normality is not met, then we will use other estimators, specific to ordinal indicators, since the use of inappropriate estimators may lead to biased parameter estimates. So, we thank the reviewer for the comment, but we do not consider that reporting the univariate analyses in the form we have presented in Table 3 is a methodological error.

18. You claim that the Work Motivation scale "is widely used in organizational psychology research.", so please add references here.  
  
Results  
The results seem to lack structure, similar as to the rest of the paper. It is a bit difficult to follow. It could help to structure the results based on the hypotheses.  
  
19. What sort of path analysis did you conduct? Was it SEM? Different models in PROCESS? This needs more information.  
As can be seen in the methodology and results section, path models assume the use of manifest variables only, and the only latent variables are the residuals. Therefore, a path model was used, not SEM nor any PROCESS model postulated by Hayes. The analysis was conducted in R, using the "lavaan" package, using a mediation model with longitudinal mediators, according to (Cole, Maxwell, 2003).

* Cole, D.A., Maxwell, S.E. (2003). Testing Mediational Models with Longitudinal Data: Questions and Tips in the Use of Structural Equation Modeling, Journal of Abnormal Psychology, 112(4), 558-577, doi: 10.1037/0021-843X.112.4.558

20. You claim the tested model had "acceptable fit". On which standards do you base this (including reference)? Also, I miss a few fit indexes. What was the TLI? What was the SRMR? It now reads as if you only present the data and results that are favorable.

There are no gold standards or rules regarding the decision on whether to reject or not to reject a path model, only rules of good practice. As Kline (2016) put it, except under conditions where a model is clearly unsupported by the data, the decision on whether to reject or not to reject the model rests with the researcher. However, there are a number of rules of good practice, not standards, such as those of Bentler (Bentler & Bonett, 1980), who argue that plausible models would be those with CFI > .05 (Bentler, 1990) and SRMR < .08 (Hu and Bentler 1999) or GFI > .95 and RMSEA < .08 (Steiger, 1990). However, Hu and Bentler (1999) never intended their rules of thumb for approximate fit indexes to be treated as anything other than just that.

* Kline, R.B. (2016). Principles and Practice of Structural Equation Modeling, 4th Edition, The Guilfod Press, New York, pp. 265-269
* Bentler, P. M., & Bonett, D. G. (1980). Significance tests and goodness-of-fit in the analysis of covariance structures. Psychological Bulletin, 88, 588–600.
* Hu, L., & Bentler, P. M. (1998). Fit indices in covariance structure modeling: Sensitivity to underparameterized model misspecification. Psychological Methods, 3, 424–453.
* Bentler, P. M. (1990). Comparative fit indexes in structural models. Psychological Bulletin, 107, 238–246.
* Steiger, J. H. (1990). Structural model evaluation and modification: An interval estimation approach. Multivariate Behavioral Research, 25, 173–180.

21. You have no reports of mediation analysis. To test mediation hypotheses, you need to test the indirect effects and report these. All claims regarding assessing mechanisms and mediation are unsubstantiated without these analyses.

Thanks to the reviewer for the observation. We have updated the article in line with this recommendation, and we introduced and interpreted the mediation relationship.

Discussion  
I have a few concerns with the discussion section which follow from the previous parts. As mentioned earlier, any reference to mediation and mechanisms (also in the abstract are false as no mediation (indirect effects) have been tested. As such, either remove these or test your mediation hypotheses.  
  
Thanks to the reviewer for the observation. We have updated the article in line with this recommendation.

22. How does your alternative explanation for the positive relation between gratitude and performance (employees who obtain better job performance might have more reasons to be grateful in their lives in general) work with the distinction between state and trait gratitude? It feels more of an explanation for state gratitude, whereas I believe you focus on trait gratitude?

23. You hint at practical implications here and there, but these require more specifics. What do you specifically recommend? How does this work and how would you do that?

Authors’ response: In response to the reviewer's comment, we have expanded the section on practical implications. We provide more detailed information about interventions designed to enhance employee gratitude, particularly those based on gratitude journaling. These interventions have been shown to have positive effects, such as improved affective wellbeing and reduced mistreatment. However, no prior research has explored whether they may also lead to increased motivation. Based on the results of this study, we recommend that future research explore the possibility that motivation could be an additional outcome of gratitude-based interventions. If gratitude interventions were shown to improve work motivation, they could be valuable for HR departments and managers dealing with low motivation among employees, especially since such interventions are easy to implement and cost-effective (please see page 14).

24. Related to the previous point, your recommendations on p. 14 feel more plausible when discussing state gratitude as opposed to trait gratitude. Maybe it needs more explanation. How would you increase employees' state gratitude?

**Authors’ response:** Thank you for your comment. It is true that the interventions we discuss do not target employees’ trait gratitude, but rather episodic or state gratitude. However, according to the multilevel model of gratitude in organizations, epeated experiences of episodic gratitude could accumulate over time, resulting in persistent gratitude - a stable tendency to feel grateful within the work context (Fehr et al., 2017). In other words, these interventions may help employees develop a more appreciative perspective toward their work environment, ultimately resulting in positive outcomes such as increased motivation. We have included this explanation in the text.  
  
  
  
**Reviewer #5:** Reviewer Comments on Manuscript: "Gratitude, Motivation, and Performance: Exploring the Mediating Role of Work Motivation in the Gratitude-Performance Relationship"  
Summary of the Study: The authors present an interesting study examining the role of trait gratitude in influencing employees' job performance, with a particular focus on work motivation as a mediator. The study's design combines both correlational and path analysis techniques to explore these relationships across two time points, contributing to the growing body of literature on how individual differences in gratitude may impact organizational outcomes.  
Overall Evaluation: This manuscript is an important and timely contribution to the literature on the role of gratitude in organizational behavior. The exploration of work motivation as a potential mediator in the gratitude-performance relationship adds theoretical depth to the understanding of how gratitude may influence workplace outcomes. While the study presents some valuable findings, I believe several key aspects could be improved, both conceptually and methodologically, to enhance the robustness and clarity of the manuscript. Below are my detailed comments.  
  
Strengths:  
1. Novelty and Contribution to the Literature:  
\* The study addresses a relatively under-explored area—how trait gratitude influences job performance through motivation. This is a valuable contribution, particularly given that most existing literature has focused more on gratitude's impact on well-being rather than job performance.  
\* The integration of Self-Determination Theory (SDT) is a strong theoretical foundation, and the use of both cross-sectional and longitudinal data is a key strength of the study.  
2. Comprehensive Literature Review:  
\* The authors have effectively reviewed prior literature on gratitude, motivation, and performance, presenting a well-articulated rationale for investigating gratitude as a predictor of performance through motivation.  
\* The connection between gratitude and motivation, particularly through identified regulation and intrinsic motivation, is clearly established, aligning well with existing research in SDT and workplace psychology.  
3. Practical Implications:  
\* The findings offer valuable insights for organizations. The suggestion that interventions aimed at enhancing gratitude could improve work motivation and performance provides actionable guidance for organizational practice, especially in the context of developing employee engagement strategies.  
  
Areas for Improvement:  
1. Conceptual Clarity and Theoretical Framing:  
\* The study would benefit from a clearer distinction between gratitude as a trait and gratitude as a state. While the authors focus on trait gratitude, it would be helpful to explicitly discuss how trait gratitude may differ from situational or episodic gratitude in the workplace context. Given that the gratitude literature often treats these constructs interchangeably, it would be beneficial to clarify how this distinction may impact the results and interpretation of the findings.

\* The authors mention that "gratitude was more strongly linked to contextual performance than to task performance," but the theoretical explanation behind this finding could be elaborated further. Why might gratitude be more strongly associated with contextual performance, which involves interpersonal and extra-role behaviors, compared to task performance? A more nuanced discussion of the type of work behaviors affected by gratitude would help contextualize this difference.

2. Methodological Concerns:  
\* Measurement Issues: The reliance on self-report measures for both job performance and motivation is a limitation, particularly given the potential for common method bias. The authors do acknowledge this, but the manuscript would benefit from a more extensive discussion of the limitations inherent in self-reports. Future research should explore using multi-source data (e.g., supervisor ratings of performance) or objective performance metrics (e.g., productivity, sales) to complement self-report measures.

Authors’ response: As recommended by the reviewer, in the revised manuscript we have elaborated further on the limitations of self-reports and the risk of common method bias. We have also recommended that future research include multi-source data or objective performance metrics to supplement self-reports (please see pages 14-15).

\* Sample Characteristics: The sample primarily consists of highly educated Romanian employees, predominantly women. A more diverse sample in future studies would help assess the cross-cultural validity of the findings.

**Authors’ response:** We agree with the reviewer’s comment. We would like to mention that we have already acknowledged this limitation in the discussion section on page 14, where we also emphasize the need for further research to assess the cross-cultural validity of the findings. (“Third, the characteristics of our sample (Romanian employees, the majority of which were women and highly educated) limit the generalizability of the results. Although conducting the study in an Eastern European context can be perceived as a strength, as it bring more diversity in gratitude research, future studies will need to investigate whether these findings can be replicated in other cultures”).

\* Outliers and Normality: The authors note that the assumption of normality was violated, yet the results from the path analysis are presented without a full discussion of how the non-normality was addressed. A clearer rationale for and detail on the use of robust techniques in the path analysis would enhance the transparency of the methodological approach.

Thanks to the reviewer for the observation. We have updated the article in line with this recommendation, and we have introduced and missing information. Since the assumption of multivariate normality was not met and the indicators are scalar variables, the full-informational maximum likelihood maximum likelihood variance with a mean and variance adjusted variance (MLMV) could not be used. The estimation will still be plain "ML", but now with robust standard errors and a robust (scaled) test statistic.

* Maydeu-Olivares, A. (2017). Maximum Likelihood Estimation of Structural Equation Models for Continuous Data: Standard Errors and Goodness of Fit. Structural Equation Modeling: A Multidisciplinary Journal, 24(3), 383–394. https://doi.org/10.1080/10705511.2016.1269606

3. Statistical Considerations:  
\* Path Analysis and Mediation: The authors test a path model with motivation as a mediator between gratitude and performance. While the results provide some evidence for mediation, the marginal significance of the mediation effects (e.g., β = 0.03, p = 0.07) calls into question the robustness of these findings. I would recommend that the authors conduct additional sensitivity analyses (e.g., testing for indirect effects through bootstrapping) to provide more confidence in the mediation results.

Thanks to the reviewer for the observation. Indeed, the mediation effect was identified as marginal and the robustness of the analysis was ensured by using appropriate estimators for the small amount of data and by the assumption criterion, the "MLMV" method providing sufficient power without the need to switch to WLS or DWLS estimators. A bootstrapping analysis was also conducted using a number of 2000 samples with seed = 34534 to ensure replicability at review. The results were identical to those obtained by "MLMV", so we chose this method for reasons of economy (it is more economical in terms of processing time) and for methodological reasons, since bootstrapping uses the central limit theorem, but CLT assumes that the samples are independent and randomly drawn from a population in which the phenomenon is normally distributed. Since the "population" is formed by the 246 observations and we do not have distributions assimilated as univariate normal to the variables included in the analysis, we considered MLMV to be a methodologically more appropriate method because it applies the necessary corrections.

Moreover, the total number of model parameters was 97, and the path model was estimated to use 39 degrees of freedom starting from n=246 cases, and α=.05, based on RMSEA (MacCallum, Browne, & Suguwara, 1996; Jak, Jorgensen, Verdam, Oort, & Elffers, 2021). We used ε0=.05, ε1=.01 as benchmarks for the RMSEA assumptions and the power of the full model was 1-β=64.74%. Since the fit indicators are borderline and the data volume is relatively small for such an analysis, we considered the model as plausible because the risk of committing a Type II error would have been too high. Chances are that replicated research on a larger volume would generate a model better supported by the data.

* MacCallum, R. C., Browne, M. W., & Sugawara, H. M. (1996). Power analysis and determination of sample size for covariance structure modeling. Psychological Methods, 1(2), 130–149. doi:10.1037/1082-989X.1.2.130
* Jak, S., Jorgensen, T. D., Verdam, M. G., Oort, F. J., & Elffers, L. (2021). Analytical power calculations for structural equation modeling: A tutorial and Shiny app. Behavior Research Methods, 53, 1385–1406. doi:10.3758/s13428-020-01479-0

\* Causal Inferences: The authors cautiously note that the cross-sectional correlations might overestimate the role of gratitude in predicting job performance due to common method bias, but the manuscript would benefit from a more explicit acknowledgment of the challenges of drawing causal conclusions based on correlational data. A clearer discussion on the limitations of causal inference, given the study's design, would help readers critically evaluate the results.

**Authors’ response:** We agree with the reviewer’s point about the challenges of making causal inferences in our study. To address this, we have clearly stated this limitation in the discussion section (please see page 14).

4. Interpretation of Results:  
\* The authors imply that interventions aimed at increasing gratitude could enhance performance, but I would encourage them to be more cautious about making practical recommendations based on the marginal mediation effects. Further research with larger samples and stronger mediation effects would be needed to substantiate this claim.

**Authors’ response:** We fully agree with the reviewer’s comment. In response, we have removed the section suggesting that gratitude interventions might indirectly impact performance. We now only focus on the potential of gratitude interventions to improve employees’ motivation. (please see page 14).

5. Future Research Directions:  
\* Given the marginal findings, future research should focus on longitudinal designs with larger and more diverse samples (e.g., community participants) to clarify the causal direction between gratitude, motivation, and performance.

Authors’ response:

6. Additional Comments for the Authors:  
\* Figure 1 Correction:  
In Figure 1, "Intrinsic Motivation T1" is displayed twice. One of these labels should likely be "Intrinsic Motivation T2." Please verify and correct this error to ensure clarity and accuracy in the figure.

**Authors’ response:** Thank you for pointing this out. We have verified the figure and corrected the label.

\* Grammatical Issues:  
There are a few grammatical errors in the manuscript. I recommend conducting a thorough proofread and revision to address these issues. For example: The sentence "These findings seem to be in line in previous ~~theoretical~~ (Deci et al., 2017) …" should be revised to "These findings seem to be in line with previous theoretical (Deci et al., 2017) …"

**Authors’ respomse:** Thank you for your attention to detail. As suggested, we have reviewed the manuscript and corrected the grammatical errors, including the suggested revision.