**Peer Review and Communication History**

**MS Title**: Perfectionistic Cognitions as Antecedents of Work Engagement: Personal Resources, Personal Demands, or Both?

**Author Names**: Marcel C. Schmitt, Elisabeth Prestele, and Dorota Reis

**Submitted**: Nov 16, 2020

**Editor First Decision—Revise & Resubmit**

Feb 10, 2021

Dear Marcel Schmitt,

Thank you for submitting your work to Collabora: Psychology. I have now received all reviews of your manuscript, “Perfectionistic Cognitions as Antecedents of Work Engagement: Personal Resources, Personal Demands, or Both?” from qualified researchers. I also independently read the manuscript before consulting these reviews. I agree that your manuscript has important strengths and also that there are some issues that need to be addressed. The multiwave longitudinal structure of your data is impressive and provides excellent opportunities to test causal effects. However, the current analyses do not take full advantage of the data strengths. I therefore encourage you to submit a revised version for further consideration at Collabra: Psychology.

The reviewers did an outstanding job in their reviews. I will highlight issues I think are particularly salient here. In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewers’ comments, outlining each change made in your manuscript or providing a suitable rebuttal.

1. Reviewer 1 (pts 1, 2, 5, 6) asks for conceptual clarification on the definitions, conceptualizations of the focal constructs. I agree that these issues could benefit for more clarity regarding the nature of the studied constructs and the validity of the items as measures of those constructs.
2. Reviewer 1 (pt 3) and Reviewer 3 (timeseries model specification) both note limitations in the type of general model form used. Reviewer 1 notes that you are unable to test direction of causality effects because your model does not include cross-lagged estimates. Reviewer 3 notes that as specified your models do not account for autoregressive effects. Autoregressive effects are likely to be quite large for the studied variables, even after accounting for person random intercepts, so modeling them is important. I suggest that you revise your modeling approach to more fully take advantage of the strengths of your mulitwave data collection, including by accounting for cross-lagged and autoregressive effects in addition to the stable between-person variance accounted for by the person intercepts. Reviewer 3 offers some specific modeling recommendations. Given the sample size of the data, I might suggest a random-intercept cross-lagged panel model (RI-CLPM), as described by (Hamaker et al., 2015, <https://doi.org/10.1037/a0038889>) or (Usami, 2020, <https://doi.org/10.1080/10705511.2020.1821690>) as most appropriate. Additionally, given the number of observation periods, you could also consider including a moving average term as described by (Usami, 2020, <https://doi.org/10.1080/10705511.2020.1821690>) and (Zyphur et al., 2020, <https://doi.org/10.1177/1094428119847278>). A RI-CLPM has several strengths over the current approach. First, it accounts for autoregressive effects. Second, it simultaneously estimates within-person cross-lagged effects for both variables, permitting you to examine direction of causality and accurately modeling uncertainty in the parameter estimates.
3. When person random intercepts are included, then person mean-centering is redundant. I suggest removing the centering so that the between-person components of your model have a meaningful interpretation.
4. Reviewer 3 (model testing approach ) raises concerns about the piecewise model testing approach used, as well as the use of significance screens for including terms in your models. Their concerns have merit. After model selection has occurred (e.g., screening variables for significance), type 1 error/coverage rates no longer have their nominal values. Accordingly, I strongly recommend building your model specification based on your hypotheses, rather than by empirically through significance screens. To this end, I suggest building a single model including all hypothesized quadratic and moderator terms. If the quadratic effects are zero, the model will estimate them as such, but the uncertainty associated with considering this term will be appropriately incorporated into your p value and confidence interval calculations. As sensitivity analyses, you could remove one moderator or the other to test the sensitivity of the estimated moderator effects to the presence of the other moderator in the model.
5. The visualizations in Figures 1 and 2 provide limited information about the data, model fit, or model uncertainty. At a minimum, please add confidence bands to indicate uncertainty in model predictions. Even better would be if the figures illustrated actual raw data along with model predictions. For examples of longitudinal data visualization with raw data, see e.g., (<https://www.sciencedirect.com/science/article/pii/S0306460318309596>) and (<https://stats.idre.ucla.edu/r/faq/how-can-i-visualize-longitudinal-data-in-ggplot2/>).
6. Reviewer 2 (pts 1, 3) and Reviewer 3 (reliability, other minor points) raise several questions about clarity and completeness of reporting (e.g., please provide confidence interval for parameter estimates, elaborate on how the ICCs are reported, and describe missing data handling). Please carefully attend to these recommendations. Please also provide the raw MPlus output as supplemental material for interested readers.

In summary, I think this is a promising manuscript and, I hope you will revise it for further consideration at Collabra: Psychology. I look forward to receiving your revision.

Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to re-submission.

If you have any questions or difficulties during this process, please contact the editorial office at editorialoffice@collabra.org.

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Brenton Wiernik

**Reviewer 1**

**Open response questions**

Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

This is my review of the manuscript entitled ‘Perfectionistic cognitions as antecedents of work engagement: Personal resources, personal demands, or both?’.
I have a number of conceptual and methodological questions/concerns which are summarized below.

Conceptual concerns:

1. The dimension of perfectionistic concerns is argued to reflect a personal demand, whereas the dimension of perfectionist strivings is thought of as a personal resource regarding work engagement. But how do these dimensions relate to each other? The authors (correctly) indicate that –in the JD-R Model– job demands and resources are assumed to be involved in two distinct psychological processes (dual process assumption). What do conceptualizations of perfectionism say about the distinctiveness of these two dimensions/facets? (How) do they operate as dual processes? How can this perspective be integrated into the JD-R Model?
2. To what extent are the newly introduced ‘personal demands’ conceptually different from ‘insufficient personal resources’? For instance, if high self-efficacy is considered a ‘personal resource’, could low self-efficacy then not be considered a ‘personal demand’? I guess my main concern is that the same dispositional characteristics may easily turn into ‘demands’ versus ‘resources’ based on the level at which they are present, or taking into account moderating effects of other characteristics that may (not) be present, etc. The notion of ‘personal demands’ requires further elaboration.

Methodological questions/concerns:

1. When examining the associations between perfectionistic concerns and work engagement, how is directionality of the effects determined? The title of the manuscript and the conceptual framing in the introduction suggest a causal path from perfectionistic cognitions to engagement, but the manuscript does not go beyond testing concurrent effects. It appears that all within-person effects represent concurrent associations. This poses a number of rather serious challenges in terms of interpreting the pattern of findings. For example:
How are perceptions of time pressure (as a moderator in this research) influenced by the IV (i.e., perfectionistic cognitions) and/or DV (i.e., engagement)?
The experience of perfectionistic cognitions might also be influenced by momentous experiences of time pressure, which render the current pattern of findings rather tricky to evaluate. The authors acknowledge this in the Limitations, but this may not be very satisfying. Why build a conceptual model, collect data on multiple time points, but then test the model in a way that does not allow you to adequately validate the model? For instance, a different/more appropriate approach could have been to study the (lagged) effects of state perfectionism at time t on engagement at time t+1 (controlling for previous levels of engagement).
The contribution of this work seems to be that previous findings at the between-person level are extended to the within-person level. However, an important limitation to this work is that the reported within-person effects are concurrent and no claims whatsoever can be made regarding the direction of effects. This should be strictly monitored throughout the entire manuscript. Otherwise, the authors need to adopt an alternative modeling approach which better allows directional claims.
2. The response rate of the study can be further specified by comparing the actual number of observations to the total possible number of observations (157 participants \* 15 daily measurements).
3. When reading the example items for PSC and PCC, it was difficult to understand how they function as valid items for daily perfectionistic cognitions. More elaboration/justification is needed here.
4. Which items were used to measure daily time pressure (besides the one example items that is provided)? Which items were used to measure daily work engagement (besides he one example item)? Has previous research used similar items to look at daily variability in engagement?
5. Can the authors provide greater detail on the magnitude of within-person variability relative to between-person variability for all measures included in the study?

**Rating scale questions**

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  | ✔ |  |  |  |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) | ✔ |  |  |  |  |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  |  | ✔ |  |

**Reviewer 2**

**Open response questions**

Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

The current manuscript examines the within-person associations between time pressure, multidimensional perfectionism, and work engagement. The authors are applauded for rooting their work soundly in previous theories and models and for their clear analytical approach. I found the manuscript to be well written in general. I recommend a revise and resubmit with only minor revisions.

1. Please include confidence intervals in all tables (including the exploratory analyses). Authors can request confidence intervals in MPlus 8.3 for multilevel models using the command OUTPUT: CINTERVAL;
2. It does not appear as though the authors completed an a priori power analysis. Although fewer between-subject observations are needed with greater amounts of within-person nested observations, the authors test several complex models with time and quadratic time thus using up degrees of freedom. Can the authors speak to the justification of their sample size and confidence that they were adequately powered?
3. In Table 1, please define all parameters in the table (w omega and p rho).
4. I found that the authors used way too many acronyms. While I appreciate the need for acronyms when there is limited space, many of their acronyms were very similar and I found myself constantly having to Ctl+F to go back and figure out what each acronym was throughout the manuscript. I suggest only using JD-R, PSC, and PCC as acronyms and spelling out the remaining constructs.
5. I examined all model syntax and raw data management. Models are sound and adequately specified.

**Rating scale questions**

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
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| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  |  |  | ✔ |  |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  |  | ✔ |  |

**Reviewer 3**

**Open response questions**

Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

The manuscript seems interested and well written. The specific content area is outside my scope of expertise, but the general data structure and modeling approach are not. The models are well described in the manuscript and faithfully rendered in the Mplus input files. However, the overall model testing approach seems somewhat backward from what would be ideal based on the theoretical background presented in the introduction. There are also some modeling details that should be addressed to take into account that these are nearly, or actually, timeseries data. Finally, there are some minor points with regard to the code availability, and reliability calculations and presentation.

Prior to expanding on these points, I should note that it is not clear whether these analyses were preregistered, as required by journal policy (I believe this should be included in the acknowledgements section).

**Model testing approach**

In the introduction, the authors make a strong case in favor of considering a quadratic effect of daily time pressure, and do not state why potential moderators would not act on any potential quadratic effects. They also do not seem to explain why the moderating effects of PSC and PCC should be examined separately (though I may have missed this). Given this, given the problems with selecting models based on significant coefficients (especially in the presence of interaction effects), and given the problems with not including interactions of covariates in tests of moderation, it seems to me that the proper starting place for this analysis is to estimate the full model with the linear and quadratic term of daily time pressure, and both interacting with PSC and PCC. Given the information in the introduction and assumptions of the models presented, this is the model that is least likely to be misspecified and therefore the most useful for answering the authors’ question about the effect of daily time pressure in the presence of these possible moderators. I would suggest, at the very least, the authors consider this as an additional sensitivity analysis.

**Timeseries model specification**

With respect to the modeling specification more generally, it seems that the analyses do not account for the possibility that a variable’s level at time t may be influenced by its level at time t-1. I believe the authors should consider including a autoregressive effects in the model unless there is a strong a priori theoretical reason to assume that observations at time t are independent of their levels at time t-1. Examples of this can be found in chapter 9 (starting at 9.30) in the Mplus manual. Specifically, something like the model in 9.32 may be more appropriate as it can accurately capture day-to-day carryover effects for each variable. This strategy is most often used for timeseries data with slightly more observations than the authors have available, so if it is not viable, they may also consider specifying a random-intercept cross-lagged panel model (RI-CLPM; Hamaker, Kuiper & Grasman 2015).

**Reliability**

Regarding the computation of ICCs, it is not clear from the manuscript or the code, how this was done (it is clear how the omegas are calculated). If the ICCs are not computed using the latent variables, it may be inappropriate to state that they represent a decomposition of between- and within-person variance, as the within-person variance may also include error variance. This may be mostly a matter of clarity in the presentation of the manuscript and code.

Related, regarding Table 1: it would be helpful to consistently use the same terms in the tables and the text—for example, “ω reliability” in the text makes it immediately clear in the table what “ω” refers to, without doubt (rather than just “reliabilities” in the description of Table 1 in the text on page 17). Similarly, I’m not sure I saw “ρ” defined anywhere as referring to the ICC, nor was the type of ICC computed described anywhere (and I haven’t been able to find the code for this statistic in the OSF repo). I would expect this to be computed as part of the MCFA to support the claim about partitioning within versus between-person variance, apart from error variance. If it is present in the code, it may help to make this clearer for the interested reader.

**Other minor points**

The authors should including the output of their code so the user does not have to purchase a copy of Mplus to examine the raw model output.

The second model input file is misnamed. It should refer to the quadratic term being included.

Even though the raw data is available, it would be helpful to include descriptive statistics on the number of observations collected from each person (e.g., the range and median, or the like). Moreover, a discussion of missing data and its handling is important (unless there is no missing data, in which case that should be communicated).

**Rating scale questions**

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
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**Author Response**

May 20, 2021

Dear Dr. Wiernik,

Thank you very much for your editorial letter and the opportunity to revise and resubmit our manuscript “Perfectionistic Cognitions as Antecedents of Work Engagement: Personal Resources, Personal Demands, or Both?”. We are glad to have received such helpful and constructive feedback from you and the reviewers. We thoroughly revised our manuscript and attempted to address all issues expressed by you and the reviewers, as detailed in the document. Our replies to the reviewers’ points are in blue.

We believe that your feedback has substantially improved our manuscript, and we hope you find the revised version acceptable for publication.

We have transferred all revised data and analysis script files to our project at the OSF (Link).

Kind regards,

Marcel Schmitt, Elisabeth Prestele, and Dorota Reis

Editor:

Dear Marcel Schmitt,

Thank you for submitting your work to Collabora: Psychology. I have now received all reviews of your manuscript, “Perfectionistic Cognitions as Antecedents of Work Engagement: Personal Resources, Personal Demands, or Both?” from qualified researchers. I also independently read the manuscript before consulting these reviews. I agree that your manuscript has important strengths and also that there are some issues that need to be addressed. The multiwave longitudinal structure of your data is impressive and provides excellent opportunities to test causal effects. However, the current analyses do not take full advantage of the data strengths. I therefore encourage you to submit a revised version for further consideration at Collabra: Psychology.

The reviewers did an outstanding job in their reviews. I will highlight issues I think are particularly salient here. In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewers’ comments, outlining each change made in your manuscript or providing a suitable rebuttal.

1. Reviewer 1 (pts 1, 2, 5, 6) asks for conceptual clarification on the definitions, conceptualizations of the focal constructs. I agree that these issues could benefit for more clarity regarding the nature of the studied constructs and the validity of the items as measures of those constructs.

We thank you and Reviewer 1 for these points. We addressed them in the manuscript and in our response to the reviewer by providing a conceptual clarification of the uniqueness of the two facets of perfectionistic cognitions (pt 1), by reflecting on the conceptual differentiation of personal demands and personal resources (pt 2) and by describing the psychometric properties of the implemented instruments in more detail (pts 5 & 6).

1. Reviewer 1 (pt 3) and Reviewer 3 (timeseries model specification) both note limitations in the type of general model form used. Reviewer 1 notes that you are unable to test direction of causality effects because your model does not include cross-lagged estimates. Reviewer 3 notes that as specified your models do not account for autoregressive effects. Autoregressive effects are likely to be quite large for the studied variables, even after accounting for person random intercepts, so modeling them is important. I suggest that you revise your modeling approach to more fully take advantage of the strengths of your mulitwave data collection, including by accounting for cross-lagged and autoregressive effects in addition to the stable between-person variance accounted for by the person intercepts. Reviewer 3 offers some specific modeling recommendations. Given the sample size of the data, I might suggest a random-intercept cross-lagged panel model (RI-CLPM), as described by (Hamaker et al., 2015, <https://doi.org/10.1037/a0038889>) or (Usami, 2020, <https://doi.org/10.1080/10705511.2020.1821690>) as most appropriate. Additionally, given the number of observation periods, you could also consider including a moving average term as described by (Usami, 2020, <https://doi.org/10.1080/10705511.2020.1821690>) and (Zyphur et al., 2020, <https://doi.org/10.1177/1094428119847278>). A RI-CLPM has several strengths over the current approach. First, it accounts for autoregressive effects. Second, it simultaneously estimates within-person cross-lagged effects for both variables, permitting you to examine direction of causality and accurately modeling uncertainty in the parameter estimates.

We thank you for this valuable suggestion. For our manuscript revision, it was our primary goal to optimize our analysis approach in order to account for cross-lagged and autoregressive effects. Thus, we attempted to apply a random-intercept cross-lagged panel model (RI-CLPM; Hamaker et al., 2015) with *t* = 15 time points, as suggested by the editor and Reviewer 3. This model would have allowed us to test the lagged effects of our variables for each of the 14 time lags separately, by also including concurrent effects and controlling for stable between-person differences. However, we were not able to estimate the RI-CLPM with so many quadratic and linear interaction terms which had to be estimated by means of numerical integration over the person-mean centered latent predictor variables. Reducing model complexity (e.g., parameter restrictions, interaction terms only for the first time lag, separate models for each single week), using another estimation method (i.e., Bayes), or running the model on a computer with higher computational power also failed, unfortunately. These failed attempts made us reluctantly conclude that our central model that included multiple interaction terms was probably too complex to be integrated into a RI-CLPM. Therefore, we decided to come back to multilevel analysis in order to test our hypotheses. However, we modified our original analysis approach by also including lagged variables at *t* – 1 (i.e., from the previous day). This allowed us to still control for autoregressive effects for our variables and test for lagged effects (i.e., effects of perfectionistic cognitions, time pressure, and their interactions of one workday on work engagement on the next workday). We tested the lagged effects only exploratorily as we were not sure whether the lagged effects would be substantial from one workday to the next (as we assessed our variables only once for each workday). We only kept lagged effects with a time-lag of one day in order to not confound these effects with effects from Friday to Monday (thus, the lagged effects from Friday to Monday were removed such that only pure day-to-day effects remained in the dataset). In order to avoid listwise deletion of missing data in our whole dataset in Mplus, we also included the variances and covariances of the independent variables as model parameters. Otherwise, we would also have lost the concurrent effects for Mondays, which would have substantially reduced our dataset (i.e., from 1962 datarows, only 1426 would have been left). As this approach led to estimation problems with the default MLR estimation (i.e., Mplus indicated that some parameter estimates were not trustworthy), we applied Bayes estimation with uninformative priors and 100,000 iterations instead. Although this modified multilevel model is more restrictive than the RI-CLPM (e.g., effects are estimated as if they were equal across all 15 workdays, which may be an unrealistic assumption) and does not allow testing cross-lagged effects, we believe that it is still a well-suited model for testing our rather complex hypotheses. In our OSF repository, we uploaded all Mplus input and output files for the attempted RI-CLPMs and our final model.

1. When person random intercepts are included, then person mean-centering is redundant. I suggest removing the centering so that the between-person components of your model have a meaningful interpretation.

Thank you for the suggestion. As we described above, we computed different models in order to account for autoregressive and cross-lagged effects as suggested by you and two of our reviewers. We finally used a revised multilevel regression analysis for our final model. In this framework we person-mean centered the independent variables in order to investigate pure within-person associations.

1. Reviewer 3 (model testing approach ) raises concerns about the piecewise model testing approach used, as well as the use of significance screens for including terms in your models. Their concerns have merit. After model selection has occurred (e.g., screening variables for significance), type 1 error/coverage rates no longer have their nominal values. Accordingly, I strongly recommend building your model specification based on your hypotheses, rather than by empirically through significance screens. To this end, I suggest building a single model including all hypothesized quadratic and moderator terms. If the quadratic effects are zero, the model will estimate them as such, but the uncertainty associated with considering this term will be appropriately incorporated into your p value and confidence interval calculations. As sensitivity analyses, you could remove one moderator or the other to test the sensitivity of the estimated moderator effects to the presence of the other moderator in the model.

We very much appreciate this valuable comment. Following this suggestion, we included all predictor variables (i.e., main effects of PSC, PCC, and daily time pressure, quadratic term of daily time pressure, linear and quadratic-by-linear interaction terms between PSC/PCC and daily time pressure) in a single model.

1. The visualizations in Figures 1 and 2 provide limited information about the data, model fit, or model uncertainty. At a minimum, please add confidence bands to indicate uncertainty in model predictions. Even better would be if the figures illustrated actual raw data along with model predictions. For examples of longitudinal data visualization with raw data, see e.g., (<https://www.sciencedirect.com/science/article/pii/S0306460318309596>) and (<https://stats.idre.ucla.edu/r/faq/how-can-i-visualize-longitudinal-data-in-ggplot2/>).

We thank you for this comment and the provided links. We thoroughly revised our figures in our revised manuscript. Figure 1 depicts the moderated regression models of daily work engagement predicted by daily time pressure at low, medium, and high values of PSC (A) and PCC (B), similarly to the two figures of the original manuscript. However, the new plots are now in color and the range of the values of daily time pressure in the data is depicted in the plots. Figures 2 and 3 are new figures. They represent Johnson-Neyman plots of the simple slopes of daily time pressure at low, medium, and high values of PSC (Figure 2) and PCC (Figure 3) including 95% confidence bands (based on Miller et al., 2013). We are of the opinion that these added figures provide an informative illustration of the quadratic interactions between daily time pressure and daily perfectionistic cognitions.

1. Reviewer 2 (pts 1, 3) and Reviewer 3 (reliability, other minor points) raise several questions about clarity and completeness of reporting (e.g., please provide confidence interval for parameter estimates, elaborate on how the ICCs are reported, and describe missing data handling). Please carefully attend to these recommendations. Please also provide the raw MPlus output as supplemental material for interested readers.

We added relevant information and output in order to be more transparent. Table 1 now contains confidence intervals for the bivariate correlations. In Table 2, which summarizes the Bayes-estimated multilevel regression parameters for our main analyses, we report credibility intervals. We are now more informative about the meaning of the ICCs values, describe missing data handling in the analysis section, and provide Mplus output in the OSF supplemental material.

In summary, I think this is a promising manuscript and, I hope you will revise it for further consideration at Collabra: Psychology. I look forward to receiving your revision.

Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to re-submission.

If you have any questions or difficulties during this process, please contact the editorial office at editorialoffice@collabra.org.

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Brenton Wiernik

Reviewer 1:

This is my review of the manuscript entitled ‘Perfectionistic cognitions as antecedents of work engagement: Personal resources, personal demands, or both?’. I have a number of conceptual and methodological questions/concerns which are summarized below.

Conceptual concerns:

1. The dimension of perfectionistic concerns is argued to reflect a personal demand, whereas the dimension of perfectionist strivings is thought of as a personal resource regarding work engagement. But how do these dimensions relate to each other? The authors (correctly) indicate that –in the JD-R Model– job demands and resources are assumed to be involved in two distinct psychological processes (dual process assumption). What do conceptualizations of perfectionism say about the distinctiveness of these two dimensions/facets? (How) do they operate as dual processes? How can this perspective be integrated into the JD-R Model?

We thank the reviewer for this important point. On p. 10, we added a paragraph on a theoretical model describing the dual processes the two dimensions of perfectionism are engaged in:

“According to the dual process model of perfectionism, perfectionistic strivings and concerns are involved in two distinct processes regarding goal attainment (Slade & Owens, 1998): Whereas (unique) perfectionistic strivings are characterized by an approach orientation which is guided by hope for success, perfectionistic concern are characterized by an avoidance orientation which is guided by fear of failure. Thus, the opposite associations of perfectionistic strivings and concerns with work engagement can be explained by different motivation orientations, which suggests the categorization of perfectionistic strivings and concerns as a personal resource and a personal demand, respectively.”

The two higher-order dimensions of perfectionism, perfectionistic strivings and perfectionistic concerns, are usually moderately correlated. If this overlap is statistically controlled for, they have been found to show opposite unique associations with outcomes of psychological well-being and health (Stoeber & Gaudreau, 2014) or, related to this study, with work engagement (Harari et al., 2018). Employees with high perfectionistic strivings are assumed to set high standards for themselves and to be highly motivated to achieve their work-related goals determined by their standards. They are driven by the expectation that positive consequences might result from achieving their goals (“hope for success”). Hence, they may invest much effort in their work and derive pleasure from their work, which should foster work engagement. Furthermore, high perfectionistic strivings might offer employees resiliency when facing obstacles such as high external demands because the expectation of success may maintain or increase the invested efforts in their work. In consequence, we believe that these features may qualify perfectionistic strivings to be viewed as a personal resource.

By contrast, employees with high unique perfectionistic concerns are highly worried about not living up to their standards. As opposed to employees with high unique PS, we believe that employees with high unique perfectionistic concerns might not consider their workplace to offer the potential of achieving high success, but rather it might contain many opportunities for them to fail. This should result in a feeling of fear and loss of control. Driven by the motivation to avoid failure by all means, they might refrain from putting much effort into their work. This state of mind impedes work-related goal attainment, and thus perfectionistic concerns are considered to fulfil the characteristics of a personal demand. Apart from that, perfectionistic concerns have shown strong positive associations with psychological maladjustment (Stoeber & Gaudreau, 2017), which corresponds with the assumed health impairment process in the JD-R Model (Bakker & Demerouti, 2017).

1. To what extent are the newly introduced ‘personal demands’ conceptually different from ‘insufficient personal resources’? For instance, if high self-efficacy is considered a ‘personal resource’, could low self-efficacy then not be considered a ‘personal demand’? I guess my main concern is that the same dispositional characteristics may easily turn into ‘demands’ versus ‘resources’ based on the level at which they are present, or taking into account moderating effects of other characteristics that may (not) be present, etc. The notion of ‘personal demands’ requires further elaboration.

Although a high level of a personal demand might have similar consequences as a low level of a personal resource (and vice versa), we believe that personal resources and personal demands are conceptually different. As we have argued that perfectionistic strivings vs. concerns as specific examples of personal resources vs. demands are involved in two distinct processes in our response to pt 1, we argue that personal resources and demands also in general are embedded in two different processes. They are not the opposite of each other, but exist as entities in their own right. More precisely, whereas personal resources should be personal characteristics which actively help employees achieve their work-related goals, personal demands should actively hinder achieving these goals. Essentially, in order for a personal characteristic to constitute a personal demand, it should not merely *not* support goal attainment (as might be the case for a low level of a personal resource), but it should directly impede it (see our definition of personal demands as “the requirements of the self that may impair individual goal attainment”, p. 8). We have to admit that the example of “low self-efficacy” which the reviewer brought up may be quite tricky for differentiating between a low level of a personal resource and a personal demand. In our opinion, in this case it is important to differentiate whether an employee with low self-efficacy attributes a potential success at work to other sources at the workplace (e.g., help from colleagues, pure chance etc.) and believes that he or she will not contribute anything valuable to that success or whether he or she expects a failure at work and he or she will be exclusively responsible for that. The first case of low self-efficacy may describe a low level of a personal resource, whereas the second case might constitute a high level of a personal demand. Both cases might result in a low level of work engagement. However, in the first case, the employee may not see the workplace as a threat, but “only” as a place where he or she cannot expect success by him- or herself. In the second case, the employee feels threatened by the workplace and expects failure that he or she will be responsible for. Thus, we believe that the approach vs. avoidance orientation framework which we proposed for the distinction of perfectionistic strivings and perfectionistic concerns as specific personal resources vs. demands, respectively, could also be applied to the distinction of many other types of personal resources vs. demands.

Methodological questions/concerns:

1. When examining the associations between perfectionistic concerns and work engagement, how is directionality of the effects determined? The title of the manuscript and the conceptual framing in the introduction suggest a causal path from perfectionistic cognitions to engagement, but the manuscript does not go beyond testing concurrent effects. It appears that all within-person effects represent concurrent associations. This poses a number of rather serious challenges in terms of interpreting the pattern of findings. For example: How are perceptions of time pressure (as a moderator in this research) influenced by the IV (i.e., perfectionistic cognitions) and/or DV (i.e., engagement)? The experience of perfectionistic cognitions might also be influenced by momentous experiences of time pressure, which render the current pattern of findings rather tricky to evaluate. The authors acknowledge this in the Limitations, but this may not be very satisfying. Why build a conceptual model, collect data on multiple time points, but then test the model in a way that does not allow you to adequately validate the model? For instance, a different/more appropriate approach could have been to study the (lagged) effects of state perfectionism at time t on engagement at time t+1 (controlling for previous levels of engagement).
The contribution of this work seems to be that previous findings at the between-person level are extended to the within-person level. However, an important limitation to this work is that the reported within-person effects are concurrent and no claims whatsoever can be made regarding the direction of effects. This should be strictly monitored throughout the entire manuscript. Otherwise, the authors need to adopt an alternative modeling approach which better allows directional claims.

We thank the reviewer for bringing up this important point. We agree that an important limitation to our original analytical approach was the sole focus on concurrent effects. This approach was also criticized by the editor and another reviewer.

In our revised model, we now also included the lagged effects (at t-1) of time pressure and daily perfectionistic cognitions including both linear and quadratic-by-linear interactions on daily work engagement (controlling for t-1 levels of daily work engagement). However, we tested these effects only exploratorily as we were not sure whether the lagged effects would be substantial from one workday to the next (as we assessed our variables only once for each workday). We only kept lagged effects over one day in order to not confound these effects with effects from Friday to Monday (thus, the lagged effects from Friday to Monday were removed such that only pure day-to-day effects remained in the dataset). In order to avoid listwise deletion of missing data in our whole dataset in Mplus, we also included the variances and covariances of the independent variables as model parameters. Otherwise, we would also have lost the concurrent effects on Mondays, which would have substantially reduced our dataset (i.e., from 1962 datarows, only 1426 would have been left). As this approach led to estimation problems with the default MLR estimation (i.e., Mplus indicated that some parameter estimates were not trustworthy), we applied Bayes estimation with uninformative priors and 100,000 iterations instead. All analysis approaches (including all prior steps that did not result in reliable solution) are documented in the OSF repository.

Prior to our revised multilevel regression analyses that included lagged effects, we also attempted to apply a random-intercept cross-lagged panel model (RI-CLPM; Hamaker et al., 2015) with *t* = 15 time points, as suggested by the editor and Reviewer 3. This model would have allowed us to test the lagged effects of our variables for each of the 14 time lags separately, by also including concurrent effects and controlling for stable between-person differences. However, we were not able to estimate the RI-CLPM with so many quadratic and linear interaction terms which had to be estimated by means of numerical integration over the person-mean centered latent predictor variables. Attempts to reduce model complexity (e.g., parameter restrictions, interaction terms only for the first time lag) or running the model on a computer with extensive working memory capacity failed. Therefore, we decided to come back to multilevel analysis, by now including lagged effects as explained above. Nevertheless, we also refer to our revised “Limitations” section of our discussion in which we advise the reader to not interpret our results causally, but rather correlatively (p. 35).

1. The response rate of the study can be further specified by comparing the actual number of observations to the total possible number of observations (157 participants \* 15 daily measurements).

In the revised manuscript we are now more transparent on this point. We wrote on p. 14:

“Compliance was good: The average frequency of daily assessments was 12.54 days per participant (*SD* = 2.92; *Min* = 2, n = 1; *Max* = 15, *n* = 51), resulting in *N* = 1,986 measurement occasions at Level 1. These constituted 83.57% of the maximum number of measurement occasions possible (15 \* 157 = 2,355).”

1. When reading the example items for PSC and PCC, it was difficult to understand how they function as valid items for daily perfectionistic cognitions. More elaboration/justification is needed here.

We now give more information on the psychometric properties of the measure of daily perfectionistic cognitions. We wrote on p. 15:

“Prestele et al. (2020) developed items for PSC and PCC that reflected the central aspects of perfectionistic strivings and concerns in the form of current thoughts. In a daily diary study among university students, they demonstrated that the six items reliably captured differences in PSC and PCC both at the within-person and at the between-person level”.

1. Which items were used to measure daily time pressure (besides the one example item that is provided)? Which items were used to measure daily work engagement (besides the one example item)? Has previous research used similar items to look at daily variability in engagement?

In our revised manuscript (p. 14), we now named all items used to assess daily time pressure and daily work engagement. Concerning the items for daily work engagement, we wrote:

“Daily work engagement was assessed with three items adapted and translated into German from the ultra-short version of the Utrecht Work Engagement Scale (UWES-3; Schaufeli et al., 2019). Schaufeli et al. (2019) developed and validated the UWES-3 in order to parsimoniously assess work engagement with three items only, each item capturing one of the three dimensions of work engagement.”

1. Can the authors provide greater detail on the magnitude of within-person variability relative to between-person variability for all measures included in the study?

In response to your comment and the Editor’s statement on elaborating the reporting of the ICCs, we updated our definition of the ICCs’ meaning in the running text (p. 16):

“Second, in order to estimate the extent to which the daily measures captured between-person differences as opposed to within-person fluctuations, we calculated intraclass correlation coefficients (ICCs) of the latent variables using the latent variance estimations of the MCFA (Hox, 2010)”.

The ICCs of all our measures are included in Table 1 (p. 19).

Reviewer 2:

The current manuscript examines the within-person associations between time pressure, multidimensional perfectionism, and work engagement. The authors are applauded for rooting their work soundly in previous theories and models and for their clear analytical approach. I found the manuscript to be well written in general. I recommend a revise and resubmit with only minor revisions.

1. Please include confidence intervals in all tables (including the exploratory analyses). Authors can request confidence intervals in MPlus 8.3 for multilevel models using the command OUTPUT: CINTERVAL;

We thank the reviewer for the feedback. Table 1 now includes 95% confidence intervals for the within-person and between-person correlations of all measures. Table 2 includes 95% credibility intervals of the multilevel regression parameters (which were estimated via Bayes in our revised analyses).

1. It does not appear as though the authors completed an a priori power analysis. Although fewer between-subject observations are needed with greater amounts of within-person nested observations, the authors test several complex models with time and quadratic time thus using up degrees of freedom. Can the authors speak to the justification of their sample size and confidence that they were adequately powered?

As our analyses relied on data that had been collected for a registered report prior to the present study, we did not conduct an a priori power analysis. However, in response to your comment, we conducted a post hoc Monte Carlo power simulation based on 1000 datasets in Mplus. In this power simulation, we used the means and variances of the measures of our study sample as the means and variances of the population measures. The simulation was based on a Level 2 sample size of 157 (equal to 157 participants in our study) and a sample size of 12 for each cluster (in our study, the average frequency of time points per participant was 12.54). As regards our hypothesized concurrent effects, we specified small to medium effects (β = 0.20) for the main effects of unique PSC and PCC and small effects (i.e., β = 0.10) for the concurrent quadratic term of daily time pressure and the linear and quadratic-by-linear interaction effects. The resampled power values (i.e., the proportion of samples in which the parameter values were significant at α = .05) ranged from .98 to 1.00. Thus, our study was adequately powered to detect small effect sizes. The Mplus output of our power simulation is included in the OSF repository.

1. In Table 1, please define all parameters in the table (w omega and p rho).

We now replaced the parameter symbols with actual terms as column names (“ω reliability” and “ICC”) in Table 1 in order to avoid confusion.

1. I found that the authors used way too many acronyms. While I appreciate the need for acronyms when there is limited space, many of their acronyms were very similar and I found myself constantly having to Ctl+F to go back and figure out what each acronym was throughout the manuscript. I suggest only using JD-R, PSC, and PCC as acronyms and spelling out the remaining constructs.

We thank the reviewer for this suggestion. We now only use JD-R, PSC, and PCC as acronyms.

1. I examined all model syntax and raw data management. Models are sound and adequately specified.

Thank you very much. We very much appreciate your thorough review of our analyses.

Reviewer 3:

The manuscript seems interested and well written. The specific content area is outside my scope of expertise, but the general data structure and modeling approach are not. The models are well described in the manuscript and faithfully rendered in the Mplus input files. However, the overall model testing approach seems somewhat backward from what would be ideal based on the theoretical background presented in the introduction. There are also some modeling details that should be addressed to take into account that these are nearly, or actually, timeseries data. Finally, there are some minor points with regard to the code availability, and reliability calculations and presentation.

Prior to expanding on these points, I should note that it is not clear whether these analyses were preregistered, as required by journal policy (I believe this should be included in the acknowledgements section).

Our hypotheses were not preregistered. The extensive dataset on which our study was based had been previously used for a published registered report. However, the overlap of the variables used in the present study and in the registered report is only minimal (see the table “Information on overlapping variables” in the file “Information on all variables assessed” in our OSF repository).

### Model testing approach

In the introduction, the authors make a strong case in favor of considering a quadratic effect of daily time pressure, and do not state why potential moderators would not act on any potential quadratic effects. They also do not seem to explain why the moderating effects of PSC and PCC should be examined separately (though I may have missed this). Given this, given the problems with selecting models based on significant coefficients (especially in the presence of interaction effects), and given the problems with not including interactions of covariates in tests of moderation, it seems to me that the proper starting place for this analysis is to estimate the full model with the linear and quadratic term of daily time pressure, and both interacting with PSC and PCC. Given the information in the introduction and assumptions of the models presented, this is the model that is least likely to be misspecified and therefore the most useful for answering the authors’ question about the effect of daily time pressure in the presence of these possible moderators. I would suggest, at the very least, the authors consider this as an additional sensitivity analysis.

We very much appreciate this valuable comment. Following this suggestion, we included all predictor variables (i.e., main effects of PSC, PCC, and daily time pressure, quadratic term of daily time pressure, linear and quadratic-by-linear interaction terms between PSC/PCC and daily time pressure) in a single model.

### Timeseries model specification

With respect to the modeling specification more generally, it seems that the analyses do not account for the possibility that a variable’s level at time t may be influenced by its level at time t-1. I believe the authors should consider including a autoregressive effects in the model unless there is a strong a priori theoretical reason to assume that observations at time t are independent of their levels at time t-1. Examples of this can be found in chapter 9 (starting at 9.30) in the Mplus manual. Specifically, something like the model in 9.32 may be more appropriate as it can accurately capture day-to-day carryover effects for each variable. This strategy is most often used for timeseries data with slightly more observations than the authors have available, so if it is not viable, they may also consider specifying a random-intercept cross-lagged panel model (RI-CLPM; Hamaker, Kuiper & Grasman 2015).

We thank the reviewer for this valuable suggestion. For our manuscript revision, it was our primary goal to optimize our analysis approach in order to account for cross-lagged and autoregressive effects. Thus, we attempted to apply a random-intercept cross-lagged panel model (RI-CLPM; Hamaker et al., 2015) with *t* = 15 time points, as suggested by the reviewer. This model would have allowed us to test the lagged effects of our variables for each of the 14 time lags separately, by also including concurrent effects and controlling for stable between-person differences. However, we were not able to estimate the RI-CLPM with so many quadratic and linear interaction terms which had to be estimated by means of numerical integration over the person-mean centered latent predictor variables. Reducing model complexity (e.g., parameter restrictions, interaction terms only for the first time lag, separate models for each single week), using another estimation method (i.e., Bayes), or running the model on a computer with higher computational power also failed, unfortunately. These failed attempts made us reluctantly conclude that our central model that included multiple interaction terms was probably too complex to be integrated into a RI-CLPM. Therefore, we decided to come back to multilevel analysis in order to test our hypotheses. However, we modified our original analysis approach by also including lagged variables at *t* – 1 (i.e., from the previous day). This allowed us to still control for autoregressive effects for our variables and test for lagged effects (i.e., effects of perfectionistic cognitions, time pressure, and their interactions of one workday on work engagement on the next workday). We tested the lagged effects only exploratorily as we were not sure whether the lagged effects would be substantial from one workday to the next (as we assessed our variables only one for each workday). We only kept lagged effects over one day in order to not confound these effects with effects from Friday to Monday (thus, the lagged effects from Friday to Monday were removed such that only pure day-to-day effects remained in the dataset). In order to avoid listwise deletion of missing data in our whole dataset in Mplus, we also included the variances and covariances of the independent variables as model parameters. Otherwise, we would also have lost the concurrent effects for Mondays, which would have substantially reduced our dataset (i.e., from 1962 datarows, only 1426 would have been left). As this approach led to estimation problems with the default MLR estimation (i.e., Mplus indicated that some parameter estimates were not trustworthy), we applied Bayes estimation with uninformative priors and 100,000 iterations instead. Although this modified multilevel model is more restrictive than the RI-CLPM (e.g., effects are estimated as if they were equal across all 15 workdays, which may be an unrealistic assumption) and does not allow testing cross-lagged effects, we believe that it is still a well-suited model for testing our rather complex hypotheses. In our OSF repository, we uploaded all Mplus input and output files for the attempted RI-CLPMs and our final model.

### Reliability

Regarding the computation of ICCs, it is not clear from the manuscript or the code, how this was done (it is clear how the omegas are calculated). If the ICCs are not computed using the latent variables, it may be inappropriate to state that they represent a decomposition of between- and within-person variance, as the within-person variance may also include error variance. This may be mostly a matter of clarity in the presentation of the manuscript and code.

Related, regarding Table 1: it would be helpful to consistently use the same terms in the tables and the text—for example, “ω reliability” in the text makes it immediately clear in the table what “ω” refers to, without doubt (rather than just “reliabilities” in the description of Table 1 in the text on page 17). Similarly, I’m not sure I saw “ρ” defined anywhere as referring to the ICC, nor was the type of ICC computed described anywhere (and I haven’t been able to find the code for this statistic in the OSF repo). I would expect this to be computed as part of the MCFA to support the claim about partitioning within versus between-person variance, apart from error variance. If it is present in the code, it may help to make this clearer for the interested reader.

We thank the reviewer for pointing this out. We followed the suggestion and now consistently referred to the composite reliability as “ω reliability” in both the text and Table 1. Also, we referred to the ICC as “ICC” in Table 1 (and not any more as “ρ”). Furthermore, we recalculated the ICC by means of the estimated variances of the latent variables of the MCFA in Mplus. We wrote in the manuscript (p. 16):

“Second, in order to estimate the extent to which the daily measures captured between-person differences as opposed to within-person fluctuations, we calculated intraclass correlation coefficients (ICCs) of the latent variables using the latent variance estimations of the MCFA (Hox, 2010).”

### Other minor points

The authors should including the output of their code so the user does not have to purchase a copy of Mplus to examine the raw model output.

We now included Mplus output files in our OSF repository.

The second model input file is misnamed. It should refer to the quadratic term being included.

We thank the reviewer for pointing this out. Since we revised our analysis approach completely, this was not relevant any more. We thoroughly checked the naming of our new input and output files before sharing them via the OSF repository.

Even though the raw data is available, it would be helpful to include descriptive statistics on the number of observations collected from each person (e.g., the range and median, or the like). Moreover, a discussion of missing data and its handling is important (unless there is no missing data, in which case that should be communicated).

We thank the reviewer for this suggestion. In our OSF repository, we now also included a csv file in which we provided descriptive statistics for each person (e.g., overall mean of PSC and PCC, number of observations).

We agree with the reviewer that participant dropout and missing data handling was not described understandably in the original manuscript. We now describe the dropout of three participants (from 160 to 157 participants) in a footnote in our method section (Footnote 2 on p. 49, text the footnote refers to on p. 14):

“The raw daily diary data consisted of 164 participants (four more than the previous online pretest). Four of those were latecomers for the daily diary assessment phase without having participated in the online pretest. In order to make sure that the sample of our study fit the necessary inclusion criteria (e.g., employees, no shift-work), which were assessed in the pretest, we excluded these four participants from the data. Of the 160 participants who took part in the online pretest, two did not subsequently take part in the daily diary assessment phase. One further participant participated in both the pretest and the daily diary assessment phase, but due to technical errors, no daily diary data could be recorded for him or her. Hence, our total sample consisted of 157 participants.”

Also, later in the method section we included a sentence describing the dropout of six measurement occasions (p. 16):

“Out of the 1,968 measurement occasions of the dataset, six measurement occasions had to be dropped as data for our dependent variable daily work engagement were missing in these.”

On p. 17, we wrote concerning handling missing data in the lagged variables:

“We set the values of the lagged variables referring to Fridays to NA in order to only specify pure day-to-day lagged effects of the variables on daily work engagement (i.e., lagged effects from previous Fridays to Mondays were removed). In order to avoid listwise deletion of all the data rows that contained missing values (among them all Monday data), we included the variances and covariances of all independent variables as model parameters.”

**Editor Final Decision—Accept**

July 19, 2021-07-20

Dear Marcel C. Schmitt,

Thank you for your patience on your resubmission.

I have now had a chance to read over your manuscript “Perfectionistic Cognitions as Antecedents of Work Engagement: Personal Resources, Personal Demands, or Both?”, along with the letter describing the changes you made. Thank you for your responsiveness to the concerns that the reviewers and I raised. I am happy to say that your paper is now officially accepted for publication in Collabra: Psychology. Congratulations on this excellent work, I think it will make an important contribution to the literature and I look forward to seeing it published! I hope your experiences with Collabra: Psychology have been positive and that you will continue to consider it as an outlet for your work.

As there are no further reviewer revisions to make, you do not have to complete any tasks at this point. Our managing editor will contact you in case there are any pre-prodution file related questions. You will have an opportunity to check the page proofs before we publish your article. Thank you again for publishing in Collabra: Psychology.

Sincerely,
Brenton Wiernik