

Peer Review Comments

Article: Rose, N., et al. (2019). Model-Based Manifest and Latent Composite Scores in Structural Equation Models. *Collabra: Psychology*, 5(1): 9. DOI: <https://doi.org/10.1525/collabra.143>

Article type: Original Research Report

Editor: Victoria Savalei

Article submitted: 08 February 2018

Editor decision: Accept submission

Revision submitted: 31 August 2018, 02 December 2018

Article accepted: 11 January 2019

Article published: 22 February 2019

Responses for Version 1

Reviewer A:

1) General comments and summary of recommendation

Describe your overall impressions and your recommendation, including changes or revisions. Please note that you should pay attention to scientific, methodological, and ethical soundness only, not novelty, topicality, or scope. A checklist of things to you may want to consider is below:

- Are the methodologies used appropriate?
- Are any methodological weaknesses addressed?
- Is all statistical analysis sound?
- Does the conclusion (if present) reflect the argument, is it supported by data/facts?
- Is the article logically structured, succinct, and does the argument flow coherently?
- Are the references adequate and appropriate?:

This paper presents two structural equation models for representing weighted composite scores as pseudo-latent variables. These models allow composite models to be run with item-level data, thereby solving many common problems in structural equation modeling that typically necessitate using a 2-stage procedure (e.g., first imputing missing data at the item level before computing composites). As someone who has repeatedly tried and failed to find a way to do this, I found these solutions clever, impressive, and exciting.

I found the paper very difficult to follow. In large part this is due to unconventional terminology (e.g., capital "I" for the number of manifest variables threw me off every time I encountered it, referring to variables as functions seems to introduce needless complexity, referring to a vector of predictors as a "multidimensional predictor" was confusing, etc.). Everything is defined and explained, it's just done in a way that will be difficult to read for even a relatively sophisticated psychology readership. To be clear, this is not a problem with the scholarship. But to have more impact, the authors may want to look at, for example, the literature introducing latent growth curve and latent change score models, for ways to write about these sorts of models that are more readable and intuitive.

To try out the models, I generated data on 9 items, which formed 3 composites (sum scores), which I modeled as a mediation model (composite 1 → composite 2 → composite 3) (see attached R code). With some effort, I managed to specify both the AFM and PIM models in lavaan, in addition to the standard sum-score model (i.e., using composite variables as input). I got matching chi-square test statistics, matching regression coefficients, and matching standard errors. I was unable to get the

intercepts of the PIM model to match those of the AFM and composite models –I probably specified something incorrectly, but if so, my model seems to match the description in text, so something could be made clearer. In addition, it took me a few efforts to figure out that in the mediation model I ran ($X \rightarrow M \rightarrow Y$), not only do the composite indicators of each variable need to covary with those of the immediately downstream and upstream predictors/DVs, but also the composite indicators of X and Y need to covary, though Y is not directly regressed on X. More generally, it would be useful if the authors could specify the rules of covariance in such a way that can apply to more general SEM models with many paths and predictors, rather than just the examples of the composite being either a predictor or a dependent variable.

Though the models are equivalent and produce matching degrees of freedom and chi-square test statistics, most SEM fit indices, of course, do not match those of the model fit directly to the composite scores. This is in part because the independence models diverge, and in part because the sample covariance matrices are of different size, making indices like SRMR diverge. So for example, in the models I fit I got CFI of .853 and TLI of .559 when the model was fit to composite scores, but CFI of .911 and TLI of -2.218 (!) using the latent composite model. Information criteria (AIC, BIC) also strongly diverge. It would be useful to spell out where fit indices are expected to be comparable, and which should be avoided. For example, the authors present CFI and TLI in their empirical example, but I am not convinced that these indices are actually appropriate in this context.

“In a first step, an equivalent model should be estimated without the composite scores. In a second step, the composite scores can be included. The two models must be equivalent in terms of model fit.” I’m not sure what this suggestion means. Is the suggestion to make sure that the latent composite model is equivalent to a model fit directly to computed composite scores? If the latter is available, why bother fitting the latent model? Part of the confusion here, I think, is that it’s not clear whether “the composite scores” refers to latent composite variables or manifest composite scores.

The subscript ‘ $m = 1 \dots M$ ’ is used initially to refer to a vector of weights (i.e., to one of an infinite number of possible linear combinations) but later used to refer to something else. I found the whole description of possible linear combinations in the beginning to be unnecessarily technical – perhaps the m subscript could be removed here, otherwise another letter should be chosen later.

I found the use of W, S, M, and C to refer to weighted, summed, averaged, and general composites unnecessarily complicated. Consider just using ‘C’ throughout, defining the weighted case everywhere, and then referring to Table 1 for what weights should be chosen to result in the special cases of unweighted sums and averages. Similarly, introducing different terminology for the manifest and latent variable components adds confusion (e.g., I don’t see a need to distinguish I and Q – but if the authors persist in doing so, then Table 1 should refer to I and Q instead of just Q).

In Figures 1 and 2 that describe a particular model, the use of the variables “I” and “Q” instead of the particular value (i.e., 3) for these models introduces confusion.

2) Figures/tables/data availability:

Please comment on the author’s use of tables, charts, figures, if relevant. Please acknowledge that adequate underlying data is available to ensure reproducibility (see open data policies per discipline of Collabra here):

The data used for the empirical example has not been made available. Code to reproduce the results is given in an appendix, but this is rather useless without the data.

3) Ethical approval:

If humans or animals have been used as research subjects, and/or tissue or field sampling, are the

necessary statements of ethical approval by a relevant authority present? Where humans have participated in research, informed consent should also be declared.

If not, please detail where you think a further ethics approval/statement/follow-up is required.:

n/a

4) Language:

Is the text well written and jargon free? Please comment on the quality of English and any need for improvement beyond the scope of this process.:

The text is well written, though very technical.

Reviewer C:

1) General comments and summary of recommendation

Describe your overall impressions and your recommendation, including changes or revisions. Please note that you should pay attention to scientific, methodological, and ethical soundness only, not novelty, topicality, or scope. A checklist of things to you may want to consider is below:

- Are the methodologies used appropriate?
- Are any methodological weaknesses addressed?
- Is all statistical analysis sound?
- Does the conclusion (if present) reflect the argument, is it supported by data/facts?
- Is the article logically structured, succinct, and does the argument flow coherently?
- Are the references adequate and appropriate?:

In this paper, the authors introduced two models for the construction of model-based composite scores of manifest and/or latent variables in SEM. The description of the methods is thorough and informative. I believe it will make a nice contribution to the literature. I list several clarification questions/comments below.

Although the paper is generally well written, I find that the introduction section (especially the 2nd paragraph) is a little weak and unclear. In the 2nd paragraph, the authors made three points: 1) The issue of measurement error can be avoided by using composite scores of latent variables; 2) Composite scores of latent variables in multidimensional measurement models are promising for addressing various research questions; 3) Model-based composite scores (even based on manifest variables) offer a valuable and uncomplicated alternative for handling missing data. It seems that the authors put too much emphasis on the benefits of using composite scores of LATENT variables. At this point, the readers might ask, "Why need to bother constructing composite score based on manifest variables?" I think the three points are solid, the problem is the way they were organized. Since the entire paper

focuses on the model-based composite scores of both manifest and latent variables, I suggest the authors to first emphasize its advantage in handling missing data. Or, you can consider in the entire paper only talking about model-based composite scores of LATENT variables. It can also make the paper more succinct.

In the discussion section, the authors mentioned about the second-order factor model. Actually, the readers would have some questions related to this at the very beginning. Why bother using the model-based composite scores rather than directly constructing a latent variable based on the manifest variables or constructing a second-order factor? What are the differences between these two approaches? And in what scenario, the model-based composite score is a must? These questions need to be addressed at the beginning of the paper.

It would be better to provide more explicit guidance for the readers who want to use this approach. For instance, in the empirical example and Appendix B, provide how the values of the fixed loadings were calculated.

2) Figures/tables/data availability:

Please comment on the author's use of tables, charts, figures, if relevant. Please acknowledge that adequate underlying data is available to ensure reproducibility (see open data policies per discipline of Collabra here):

The tables and figures are appropriate and easy to read.

3) Ethical approval:

If humans or animals have been used as research subjects, and/or tissue or field sampling, are the necessary statements of ethical approval by a relevant authority present? Where humans have participated in research, informed consent should also be declared.

If not, please detail where you think a further ethics approval/statement/follow-up is required:

Not applicable.

4) Language:

Is the text well written and jargon free? Please comment on the quality of English and any need for improvement beyond the scope of this process:

Please check the equation numbers in the text to see whether they are referring the appropriate equations. See "Equation 7" on page 10, and "Equation 23" under Equation 22.

Page 4, lines 3-4, "construction of model-based composite scores" appears twice in one sentence.

Editor Decision for Version 1

Editor: Victoria Savalei

Affiliation: University of British Columbia, CA

Editor decision: Revisions required

Decision date: 24 April 2018

Dear Dr Norman Rose,

After review, we have reached a decision regarding your submission to *Collabra: Psychology*, "Model-Based Manifest and Latent Composite Scores in Structural Equation Models". Our decision is to request revisions of the manuscript prior to acceptance for publication.

The full review information should be included at the bottom of this email. There may also be a copy of the manuscript file with reviewer comments available once you have accessed the submission account. A summary of the requested edits from the editorial team can be found below. Please consider these points and revise the file accordingly:

Editorial Revision Requests:

I agree with the first reviewer that this paper solves a long-standing problem, and the authors to be congratulated on that! However, the paper is difficult to read, and will likely have little impact among psychologists using SEM unless it is revised in a fundamental way to improve clarity. Consider also that *Collabra* is not a statistics journal, and while we have a section for quantitative methods, ideally the papers within it should be understood more broadly. Let me offer a few suggestions for revision, many of which overlap with the reviewers' comments:

1. The proposed technique has at least two applications, which are presented side by side, often resulting in confusion and distracting the reader. Possibly due to this being my own area of research as well, I find the application to manifest variable composites with missing data to be by far the most interesting. Because of this, I wonder if it may be best if the paper just focused on this application. The second application (to latent composites) could be saved for a separate paper, or it could be added as a separate section once the first application is covered (perhaps with less detail?), or as appendix, or simply mentioned in the discussion. Unless my reading was inaccurate, it seems the generalization from manifest to latent composites is straight-forward, so there is no need to have this second example run through the entire paper. This is just a suggestion and I am well aware of my own bias here. But given that the two applications running side by side is confusing (see also comments by the second reviewer), consider sequencing them in some way if you choose to keep both. Understanding the manifest variable models well first will help the reader understand the next application.
2. It is also distracting to follow three versions of the composite. You cover the weighted composite first and then cover the special cases. For most impact, it may be better to do it rather the other way around. I would guess the most common type of composite is the average; it may work better to focus on just this one composite and then note the simple extensions to the unit-weighted in a sentence or a footnote. I'm not sure covering the weighted composite is even necessary (how often is such a thing used? How difficult really would it be to generalize from weights of 1/1 to something else?). The extension could be relegated to an Appendix. This is not about paper length, but rather about readability. Fewer distractions would help the reader focus on understanding the models.
3. I agree with the first reviewer that the notation is horrendous, and in particular that the function notation could be dropped to drastically improve readability.
4. I wonder if the presentation of each model shouldn't start with the reference to the corresponding figure, so that the description can be more easily followed.

Beyond clarity, I have some additional comments:

5. Removing some of the complexities above will free up room to focus more deeply on implementation: see the first reviewer's comments about generalizing to more complex models. Again, you want to have impact: it may even be a good idea to create a "toolbox" of sample syntax for several most common models and post it online, so that the reader can see how easy it is to treat missing data using this method for a type of model that they have.

6. The issue of fit indices raised by the first reviewer is a good one; in your example, you report and interpret the fit indices as if they do not change, but in fact, they do. This is either a serious limitation or an opportunity for further development. In fact, finding a complete dataset and examining the output of the model run using manually created composites versus the two models you proposed is highly informative—it seems that for your method to be thorough, everything should match with complete data. And what about the intercepts, should they ever be of interest? It would be useful if the discussion section contained a section on which types of models and model statistics this approach will produce equivalent results for, and where it may either fail or require further developments.

7. It would be nice if the discussion addressed the comparison of the two models—which do you recommend?

8. This comment can be ignored given the obvious conflict of interest, and it should certainly not be taken to mean that you should cite the papers below! But I can't not point out the connection between your approach and my own work, should it be useful to you or yield additional insights. I have worked on an analytic method called the two-stage maximum likelihood (TSML), which deals with item-level missing data when the model is at the composite level. This method requires special programming (unlike your models, which can simply be set up in existing software), and the essence of it is as follows: 1) Fit the saturated model using FIML to all the components, 2) Convert the estimated mean vector, covariance matrix and the asymptotic covariance matrix from the first stage into the corresponding quantities for composites using simple algebra, fit the SEM to these quantities, with robust corrections to standard errors and test statistics using the correct asymptotic covariance matrix (Savalei & Rhemtulla, 2017a). It's quite simple to implement, but again, requires special programming for an applied user. The TSML method is essentially the analytic equivalent of item-level multiple imputation, which is another approach to item level missing data when the model is at the composite level. I have also worked on a related GLS method, which should be more efficient than TSML in theory, but we did not find it to be the case (Savalei & Rhemtulla, 2017b). I do not know if you can answer this question, but what I really want to know is whether your method is essentially "trickery" for obtaining TSML estimates or if it produces more efficient estimates because it doesn't formally divide estimation into two steps. However, the components-to-composites part of the model is always saturated, as it is for TSML. In the absence of the composites, the difference between TSML and FIML when treating missing data (e.g., Savalei & Bentler, 2009) is the same as the difference between multiple imputation under the saturated versus under the structured model. Does the imputed missing data benefit from the assumed structural model? Thus, whether your method is the same as TSML or more efficient is essentially the question: does the "imputation" of missing data at the item level benefit from the overall proposed model for the components, or not? Interesting question. I resisted the temptation to compare TSML and your models on a dataset, but I look forward to the paper appearing in print so I can do so.

9. Presenting lavaan syntax and output in addition to Mplus would be more consistent with the open science principles that Collabra follows. The first reviewer provides sample syntax (you should be able to see this file in the Collabra system). Finding a dataset that can be shared, so that the reader can run the syntax on the data and reproduce the example(s) in the paper, is also recommended.

In summary, let me reiterate that I think the ideas in this paper are fantastic, but at the same time a lot of work needs to be done before the paper is readable by a more general audience and can have impact.

Savalei, V., & Bentler, P. M. (2009). A two-stage ML approach to missing data: theory and application to auxiliary variables. *Structural Equation Modeling*, 16, 477-497.

Savalei, V., & Rhemtulla, M. (2017a). Normal theory two-stage estimator for models with composites when data are missing at the item level. *Journal of Educational and Behavioral Statistics*, 42, 405-431. doi: 10.3102/1076998617694880

Savalei, V. and Rhemtulla, M. (2017b). Normal theory GLS estimator for missing data: An application to item-level missing data and a comparison to two-stage ML. *Frontiers in Psychology*, 8, 767. doi: 10.3389/

fpsyg.2017.00767

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If you have any questions or difficulties during this process, please do contact us.

Please could you have the revisions submitted by [GIVE 6 WEEK DEADLINE]. If you cannot make this deadline, please let us know as early as possible.

Kind regards,

Victoria Savalei

v.savalei@ubc.ca

Author's Response to Review Comments for Version 1

Author: Norman Rose

Affiliation: University of Tübingen, DE

Revision submitted: 31 August 2018

29 May 2018

Dear Sir or Madam,

Dear Prof. Savalei

Thank you very much for the opportunity to revise our manuscript "Model-Based Manifest and Latent Composite Scores in Structural Equation Models". Unfortunately, the manuscript has changed quite fundamentally to address yours and the reviewers' comments properly. Due to changing the order of the manuscript and to reduce mathematical/technical language most parts of the paper were completely rewritten. The Tables and Figures were revised. We put greater emphasis on manifest composite scores with missing values in the component variables. We also included the issue of item parcels in measurement models to link the paper to your work. We also added a simulation study to demonstrate the performance of model-based composite scores in presence of missing data. Currently, we are still working on deriving general rule for model building with Composite scores as requested by one of the Reviewers. We feel, that this point is challenging. The underlying principle or the rational is simple, but no simple rules are implied that work for all possible model. In order to make the approach accessible for applied researchers, we started to create commented R syntax and an accompanying document with detailed explanations for models with manifest and latent composite scores (incl. item parcels of dichotomous variables in measurement models, manifest composite scores as independent and dependent variables in explanatory SEM, latent composite scores in higher order factors models etc.). Although a lot of work has already been done, we are not able to complete the revision within the six weeks (until next week). We apologize for the delay. Therefore, we kindly ask for an extension of the deadline by at least one months.

Thank you very much in advance.

Yours sincerely,

Norman Rose

31 August 2018

Dear Editors,

We just uploaded the revised manuscript, figures, data sets and supplemental material. We found no final "send" button and therefore, we are not sure whether the resubmission process has been completed by the upload. So, we would be thankful for a short reply whether the submission of the manuscript has been successful.

Thank you very much in advance,

Kind regards,

Norman Rose

Attached document:

<https://s3-eu-west-1.amazonaws.com/ubiquity-partner-network/ucp/journal/collabra/143-2316-1-SP.docx>

Editor Decision for Version 2

Editor: Victoria Savalei

Affiliation: University of British Columbia, CA

Editor decision: Revisions Required

Decision date: 29 September 2018

Dear Dr Norman Rose,

We have reached a decision regarding your submission to *Collabra: Psychology*, "Model-Based Manifest and Latent Composite Scores in Structural Equation Models". Our decision is to request revisions of the manuscript prior to acceptance for publication. A summary of the requested edits from the editorial team can be found below. Please consider these points and revise the file accordingly:

Editorial Revision Requests:

I have not sent this revision out for review; instead, I have read it carefully myself.

The paper is much clearer compared to the original version. At the same time, the paper is still quite dense. There is a lot of material here. It is not a "teaching" paper. It will likely be read by methodologists, but may be hard for applied researchers to follow. I assume that the authors' intent is to keep the paper at this level. I infer this from the fact that some of the simplification suggestions (choosing either manifest or latent composites as main focus; moving weighted composites into an appendix, etc) were not followed. This is fine with me, as the introduction of code and examples in Supplementary Materials makes the mastery of the ideas in this paper possible for anyone willing to try. But you should know that readability is not at the level of high impact for the average psychologist. Below are my comments on this revision that do still need to be addressed:

Explain figures; refer to them in text. For example, Figure 1 is not referred to anywhere in the paper except once, in explaining Figure 4. When should a reader trying to learn from the text look at this figure for reference? Further, the figure itself is not clear. What are the dotted lines? What are the number -1, 1, 3, etc? What are Y1, Y2, Y3? Where in the text is there a 3-variable example that would tell me how these variables are formed? (if they are composites) Please check for these types of issues with regards to all other figures.

The simulation for missing data is a misapplication of the "proration" or available-case analysis method, making the results seem too favorable for the studied method. No one would ever treat missing data by computing a sum composite via imputing a score of zero for the missing values! The way missing data are treated when creating composite scores under this method is to either a) use the average of all available items, or b) equivalently, impute person-level means for the missing items (e.g., Schafer & Graham, 2002; Mazza, Enders, Ruhlman, 2015; Savalei & Rhemtulla, 2017). When this method is used instead, I will not be surprised if there is very little bias in parameter estimates... your method's standard errors should be better, however. To increase bias in parameter estimates, you could generate items with different means (again, see references above for a discussion of why this matters).

As *Collabra* supports open science, please include all materials pertaining to the simulation, and clear references to them: code to run the simulation, code to generate the data (ideally, the seed would be specified to generate the exact numbers in the paper, but if not that's ok), code to create relevant summaries of data. As I will not send the paper out for review again, please do not blind the manuscript but instead include file names and links everywhere, so that I can check them for readability.

One of the sections that Reviewer #1 asked you to add was instructions for how to specify the PIM model within a more complex SEM. You have provided this section starting on p. 13. It is quite lengthy and difficult to follow. This is another place where picking a simple example and working through it as the main example (perhaps even including a figure) would be hugely helpful. For instance, Reviewer #1 used a simple mediation model as an example. Alternatively, or in addition, refer to Supplementary materials files for such examples, but refer to them specifically. The file is huge and has many sections. Sentences such as: "An illustration of how these rules are applied to a simple latent mediation model

with three indicators per latent variable in lavaan and Mplus are given in section 3.2.1 of Supplementary materials” could go a long way in improving clarity. The more you do this here and throughout the paper, the better the readability will be for more applied researchers, as well.

Reviewer #1 also had commented, in the first draft, on the fact that fit indices such as CFI, TLI, SRMR, AIC, BIC diverge between the original model for the composites and the PIM model when the data are complete. This comment outlines a very important limitation of the method. In your response, you write: “In our approach, we combined the target model with the PIM to construct the model-based composite score. This is a more complex model with more variables and parameters—at least in the case of manifest model-based composite scores. However, the degrees of freedom, the value of the discrepancy function, the χ^2 -value of the model fit, and the corresponding p-values of the two models do not have to be equal. Furthermore, the RMSEA, the results of the test of whether the RMSEA value is less than .05, and the confidence interval of the RMSEA must be equal. Many fit indices that penalize model complexity or depend on further information will differ between the two models. We apologize that we were not clear about this in the first version of the paper. In the revised version, we clarified this point.”

To be honest, I fail to see where this discussion appears in the paper. I have searched for terms: “fit”, “CFI”, “SRMR”, but did not find this very important discussion included. It should be written for applied researchers, and it should include caution that some fit indices will stay the same whereas others will change and should not be interpreted in the same way (perhaps not interpreted at all, which is a downside of this method). In the example Reviewer #1 used, TLI was a high negative value. Thus, in the revision, I would suggest the following: 1) have an explicit discussion about fit indices and their expected differences as a separate section, and why it occurs (again see earlier comments by reviewer #1. For maximum clarity, you could even include lavaan or Mplus output for fit indices for two models run on complete data, to see what stays the same and what changes). 2) Include qualifications for the interpretation of some fit indices in your empirical example section, where you report CFI and interpret it, without a reference to this problem, and 3) List this as a limitation of the method (and subject for future research perhaps) in the discussion. Another idea is to propose solutions: for instance, perhaps a custom independence model can be set up for the CFI to bring its value in line with that under the composites model with complete data.

Sections that could be shortened if addition of new material makes the paper unwieldy: a) introduction with many examples of composite scores is quite long (the reader also naturally wonders where it is headed), b) the empirical example is quite long. In general, the writing is verbose and high-level, where a simpler presentation could be more enlightening to an applied reader.

Small comment: The sum score is called S in one part of the manuscript and C in another part. This could be confusing.

Mazza, G. L., Enders, C. K., & Ruhlman, L. S. (2015). Addressing item-level missing data: A comparison of proration and full information maximum likelihood estimation. *Multivariate Behavioral Research*, 50, 504–519.

Schafer, J. L., & Graham, J. W. (2002). Missing data: Our view of the state of the art. *Psychological Methods*, 7, 147–177.

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If you have any questions or difficulties during this process, please do contact us.

Please could you have the revisions submitted by October 28th. If you cannot make this deadline, please let us know as early as possible.

Kind regards,

Victoria Savalei

v.savalei@ubc.ca

Author's Response to Review Comments for Version 2

Author: Norman Rose

Affiliation: University of Tübingen, DE

Revision submitted: 02 December 2018

Dear Victoria Savalei,

Dear Sir or Madame

We submitted the second revision of the manuscript entitled 'Model-Based Manifest and Latent Composite Scores in Structural Equation Models'.

We thoroughly revised the manuscript, re-run the simulation study, and addressed all the concerns and recommendations. We uploaded our responses to your comments in an extra word document (Rose2018ReplyLetterRev2.docx).

In our view the paper has again significantly improved, and we sincerely hope that you will share our impression.

Sincerely yours

Norman Rose

Attached document:

<https://s3-eu-west-1.amazonaws.com/ubiquity-partner-network/ucp/journal/collabra/143-2587-1-SP.docx>

Editor Decision for Version 3

Editor: Victoria Savalei

Affiliation: University of British Columbia, CA

Editor decision: Accept Submission

Decision date: 11 January 2019

Dear Dr Norman Rose,

We have reached a decision regarding your submission to Collabra: Psychology, "Model-Based Manifest and Latent Composite Scores in Structural Equation Models", and are happy to accept your submission for publication, pending the completion of copyediting and formatting processes.

As there are no further reviewer revisions to make, you do not have to complete any tasks at this point. The accepted submission will now undergo final copyediting. You will be contacted once this is complete to answer any queries that may have arisen during copyediting and to allow a final chance to edit the files prior to typesetting. If you wish to view your submission during this time, you can log in via the journal website.

Kind regards,

Victoria Savalei

v.savalei@ubc.ca