Author's response to reviews

**Title:** Can self-reported disability assessment behaviour of insurance physicians be explained? Applying the ASE model

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**Author's response to reviews:** see over
Subject
Submission revised manuscript

Dear Sir/madam,

We hereby submit our revised paper “Can self-reported disability assessment behaviour of insurance physicians be explained? Applying the ASE model”. A detailed reaction to the reviewers’ comments is enclosed with this letter.

We believe that our manuscript has improved considerably as a result of the reviewers’ suggestions.

We hope that you will find this revised manuscript acceptable for publication in your journal.

On behalf of all authors,

Yours faithfully,

R. Steenbeek
Reaction to reviewers’ comments

**Reviewer 1: Uwe Matterne**

We would like to thank the reviewer for her valuable comments. Below, we explain how we have addressed the points that were raised. We believe that our manuscript has improved considerably as a result.

**Major compulsory revision:**
1. “While it is now stated that self-efficacy was adapted to the purpose of the study it is still not clear how this was achieved and what items the scale consisted of.”

Self-efficacy was measured with the ten items formulated by Scholtz et al. (2002). These were adjusted so that we could measure the insurance physician’s belief in his ability to carry out work disability assessments. The items related specifically to self-efficacy during the disability assessment interview and comprised, amongst other things, the ability to resolve difficult issues involving clients, the ability to stick to the proper procedure when a client is being difficult, the ability to follow the planned course of the assessment interview, the ability to apply different solutions when a problem arises during the assessment interview, the ability to find a way of dealing with situations no matter what happens during the assessment interview, etcetera. We have added these adjusted items from the Scholtz scale to the Methods section in the text (p. 9).

2. “Most of the descriptions of the constructs in the result section under the heading ‘Measurement models of latent variables’ do actually belong into the method section as they describe what these constructs mean.”

We understand from the remark of Reviewer 1 that there is some confusion on this point. Measurement models of latent variables are part of the results of a full SEM model (Jöreskog and Sörbom 1996). As mentioned in the Methods section (p. 10), we had to split the measurement models from the path model for technical reasons ((i.e. a not positive definite correlation matrix of measurement errors). However, in that case, the assessed measurement models with their factor loadings are still a result of the measured SEM model: we do not know from the information in the Methods section how the latent ASE constructs will load (direction and strength) on the observed variables (scales and dimensions). Hence, the constructs cannot be described under the heading ‘Measurement models of latent variables’ in the Method section, as Reviewer 1 suggests. Given the confusion on this point, we have stated in the Methods section (p.10) that the measurement models should be considered as part of the results of the estimated model.
3a. “The discussion is still quite general in most parts. It often reads like a rephrasing of the results while giving some interpretation. Discuss it more in light of other research but make sure the investigated intention and behaviour constructs in those studies are relevant to this study. Even though you said in your author reply that ‘more’ discussion would be too speculative I believe the discussion can still be improved.”

We extended the discussion with relevant TPB-based papers on guideline adherence.

3b. “You also need to state more clearly why the hard work you put into this study was necessary and perhaps provide some implications for practice.”

We have added a new paragraph “Practical relevance and future research” (p. 24) to the Discussion section.

3c. “Also, since a lot of modification went on in the model-building process, the findings would need to be replicated, most preferably by confirmatory path analysis/SEM in an independent sample.”

We have included this point among the weaknesses in the Discussion (p.23).

3d. “Perhaps, future research could also develop items that correspond more to intention and behaviour in terms of saliency and adhere to the TACT principle in general.”

We have included this point among the weaknesses in the Discussion (p.23).

3e. “You correctly identify the problems associated with intention and state that many other model specifications would be possible, but I think your future perspectives could go further. Please revise the discussion accordingly.”

We have included this point among the weaknesses in the Discussion (p.23).

4. “I do not understand at all why it is stated that the moderating role of barriers and knowledge is investigated and then nothing indicating such analyses can be found in the result section. There are no interaction terms in the model nor did you make group comparisons. In the first submission you talked of the mediating role of knowledge and barriers (mediation) that was, however, not tested; now you talk of moderation instead, but still, where are these analyses? From what I have read, knowledge and barriers were simply added to the path model as predictors of behaviour; their potential moderating role on the relationship between intention and behaviour was not examined.”

We agree that we did not investigate the moderating role of barriers and knowledge. We considered both variables only as a part of the ASE model. Their potential role as moderators was not of interest to our research question. We have removed the word “moderating” from the text (pages 2, 7, 11) to avoid confusion on this point.
5. “While the current manuscript now reports the effect sizes (R2) more clearly, it should be stated that most of the variance in behaviour and intention is unaccounted for despite the many variables included in the model, i.e. is due to unmeasured influences or/and measurement error. 7% of variance and about 15% in behaviour is poor considering the findings of meta-analyses of TPB studies in which around 27% in behaviour and 39% in intention are explained. I again wonder how valid the constructs actually are considering these results, even though the Steenbeek et al. paper (2011) based on the same sample, claims the constructs to be valid.”

We agree that the explained variance of Intention and the two Behaviour variables is less than has been found in other TPB studies, as reported in the meta-analysis by Armitage and Conner (2001). We have added this issue to the Discussion section (‘other studies’, p.23), where we also address the validity of the constructs in our investigated path model (p. 23). We have now explained that the 48 scales and dimensions which were investigated in the Steenbeek et al. paper (2011) and which were considered valid because of good properties, were used for the measurements of the latent ASE constructs on a more general level in our model. It is possible that the measurement of the latent ASE constructs in this paper is at such a high, general level that it produced relatively weak relationships between these ASE constructs in our estimated path model.

6. “There are still grammatical mistakes in the manuscript and the style can also be improved in several instances.”

The revised manuscript has been corrected by the same agency which translated the original, the VU University Language Centre, Amsterdam.

Minor details:
1. “I would change inter-doctor variation to between-doctor variation.”

In the literature the expression ‘inter-doctor variation’ is used more often than ‘between-doctor variation’. We therefore prefer the former.

2. “I would transpose Table 3 to have predictors in rows and predicted variables in columns.”

We have transposed rows and columns in Table 3.

3. “The ‘perfect’ correlations between the same variable in Table 2 can be deleted.”

We have deleted the diagonal in Table 2.
Reviewer: France Legare

We would like to thank the reviewer for the valuable comments. Below, we explain how we have addressed the points that were raised. We believe that our manuscript has improved considerably as a result.

Major compulsory revisions:
1) *I acknowledge the contribution of Stephane Turcotte, MSc, biostatistician, to this review.* The authors have written a clear methods section. However, there is still an issue that needs to be dealt with: the authors report that their confirmatory factorial analysis did not support the final model (barriers and knowledge components). In other words, the final model presented in the figure is not supported by the CFA that indicated that barriers and knowledge should be only one factor. In the final model, they are represented by 2 factors. We suggest that the editors asked advice to a biostatistician who is knowledgeable about SEM for a final decision on this aspect.

In our opinion, the final model presented in Figure 2 is completely in line with the results of the measurement model in Table 1 (columns “B” and “K”). This model indicates that barriers and knowledge are two separate latent ASE constructs. For technical reasons, these two latent ASE constructs resulted from one measurement model that supports the theoretical distinction between the two ASE constructs with empirical evidence. As we think that the text may have been somewhat confusing here, we have revised the Methods section (p.11/12) and the Results section (p. 14).

2) *Also, in the figure presenting the final model, the arrow that goes from behaviour assessment to self efficacy (0.22) should be supported not only by the use of a Chi square but also by the use of RMSA and CFI values.*

We have added the fit values of the final model and the competing model to the Results section (p 18) and to “Appendix 2: The development of the model” (p 2).

Minor Essential Revisions
"The results section and the interpretation section of the manuscript would be easier to read if all items were phrased in the same direction."

We mention on p. 11 that all the factor scores for the ASE constructs were defined in such a way that they point to a higher score on the meaning of the ASE construct. In addition, throughout the manuscript, we formulated the associations between the ASE constructs in compliance with the directions (+ and -) that were found in the path model (Table 3 and Figure 2): a positive association was formulated as “more A was related to more B” and a negative association was formulated as “more C was related to less B”.

We found only one formulation in the Results section which used the phrase: ‘less C was related to more B’. We changed this (p. 18) to: ‘more C was related to less B’.
Finally, we changed the text in the Results section (p. 18) because we may have used the term “autonomously” in a confusing way in relation to “more eager, in control and willingness to compromise” (i.e. more Behaviour: Process).

Other remarks
“Quality of written English: Needs some language corrections before being published.”

The revised manuscript has been corrected by the same agency which translated the original, the VU University Language Centre in Amsterdam.