

Referee report for tc-2015-114: “Numerical simulations of the Cordilleran ice sheet through the last glacial cycle” by J. Seguinot et al. (manuscript version tc-2015-114-manuscript-version2.pdf)

I would like to congratulate the authors to their thorough and thoughtful revision of their manuscript. In my mind all issues raised by me and my fellow reviewers have been addressed more than adequately. It is wonderful to see how much the presentation of the study has improved by the addition of the new material, especially the sensitivity study.

As an example I want to refer to Figure 7 in the manuscript, which contains now much more information by the addition of possible error bounds. Figure 2 and 3, being technical might not be of uppermost interest to all readers the authors want to reach (e.g. glacial geologists), however Figure 7 now wonderfully summarizes the model capabilities with its uncertainties in a way which should be accessible for all readers. I also enjoyed seeing the results of the sensitivity study making its way into the geological discussion (e.g. page 15 line 30-32) and I think it adds value there too.

Finding to my delight the manuscript now in a directly publishable state thanks to the large efforts of the authors, I still have to make two very minor technical comments. These serve the purpose to clarify even more two fine nuances in the model presentation and should avoid confusion by newcomers to the field of numerical glaciology which read the manuscript at hand.

Page 4, Line 29. What the authors call here as the “equivalent stress” is often termed “effective stress” in standard glaciological textbooks (e.g Cuffey & Paterson) and I would recommend to change the term for the second invariant of the stress tensor to be “effective stress”. Also that the second invariant of the stress tensor,  $\tau_e^2 = \frac{1}{2} tr(\tau^2)$  is not generally correct. The authors are surely familiar with equation 3.18 in Cuffey&Paterson, page 59. Maybe changing the text to :” and  $\tau_e$  the effective stress defined in our case by ...” would help avoid confusion.

Right below (page 5), equation (2) is not directly taken from Aschwanden et al, 2012, Eqs. 63-65 as the citation suggests. I presume the authors present the current implementation in PISM of the work they refer to. Maybe it is worthwhile to state that, e.g. “ through a piece-wise Arrhenius-type law based on Aschwanden et al., 2012: “ Or leaving the equation reference out of the citation might also be an option.

I have no further comments to make and close by congratulating the authors again to their great work.

Kind regards,  
Alexander H. Jarosch