

REFeree REPORT FOR THE PAPER
“SENSITIVITY OF THE WEDDELL SEA SECTOR ICE STREAMS
TO SUB-SHELF MELTING AND SURFACE ACCUMULATION”
BY WRIGHT ET AL.

The paper studies the sensitivity of several ice streams in the Filchner-Ronne Ice Shelf region to changes in surface accumulation and sub-shelf mass balance. The goal is to try to understand the responses these ice streams may have to potential future variations in surface accumulation and sub-shelf melt/freez rate.

The research findings are very interesting and important contributions not only to the cryosphere/glaciology community, but to the general public. It is a well and carefully written article, hence I recommend it for publication in Cryosphere, after addressing a few minor comments, observations and suggestions, detailed below:

1. The material in this article is quite complex and dense, and I understand the need and effort to keep the discussion clean and simple. However, one of the difficulties I had while reading through was that the authors mention (several) parameters in the model, but they never concretely state the model, the equations, etc. I would strongly suggest adding the minimum necessary equations to make the presentation self contained and to help the reader follow the discussion.
2. The authors mention (pag 5482) that “to invert the model equations using as input measurements of present-day ice sheet geometry (Fretwell et al., 2013), temperature (Pattyn, 2010) and surface velocity (Rignot et al., 2011).”. This is a bit confusing, as it suggests that the inversion for the parameter(s) is done based on all these data (observations), while later in the next paragraph the data misfit is defined based on surface velocity observations. Please reformulate. Also, I suggest adding “satellite surface velocity observations” to make sure it’s clear what are the data used in the inversion. Finally, “invert the model equations” is not correct, if the purpose is to invert for the parameters present in these equations/model.
3. The underlying inverse problem would have been ill-posed even with one inversion parameter (field). Please reformulate (pag 5482, line 10).
4. The authors mention that the inverse solution is obtained with 16 (optimization) iterations. It would be beneficial to know what inverse solution method did the authors apply (i.e., steepest descent, nonlinear CG?). Also, please comment on the (relative) reduction in the norm of the gradient.
5. It would be useful to show the used (InSAR) observed surface velocities in the Filchner-Ronne Ice Shelf region and the corresponding recovered velocity fields (obtained with the inverse solution). This would give some more understanding to the reader about what the authors mean by: “the model is unable to match the observed ice fluxes at those ice streams which flow through well-defined narrow channels” (pag. 5485, lines 25).
6. There is a detailed (good) discussion on the mesh refinement. It would be

nice also to show the mesh used for a typical simulation.

7. **Grammar, typos, consistency, etc.** *consistency issues*: lines 10: modeled speeds versus modeled ice velocities; $t = x$ yr versus $t = x$; south-north versus South-North; *grammar*: pag. 5487 (lines 25): to apply.