

Response to Frédéric Parrenin: Assessing the potential for an ice core in the southern Antarctic Peninsula to elucidate Holocene climate history

Review of "Assessing the potential for an ice core in the southern Antarctic Peninsula to elucidate Holocene climate history" by H. Davis et al.

This article investigates the potential for an ice core in the Northern Ellsworth Land, at a triple ice divide point between the Amundsen, Bellinghousen and Weddell seas (so called ABW site, 1,200 m ice thickness). This is done with a conjunction of age modeling, radar observations and shallow ice coring. Two IRHs could be traced down to ABW, dated 2.62 and 4.72 ka. Another IRH dated at 6.94 ka could be traced elsewhere in this region but not down to ABW. The model of Martín et al. (2015) was used to evaluate the age-depth relationship at ABW and elsewhere along the radar profiles. It is a 1D model with a steady velocity profile but with a transient surface accumulation forcing. The model is actually able to invert the surface accumulation rate needed to fit some age markers. The accumulation is therefore inverted at ABW for the last ~5 ka with a linear by parts assumptions (there are actually two segments) and before that, it is forced with the Wais Divide scenario. It is found a very strong decrease in accumulation since ~5 ka ago, but the authors also suggest a possible ~600 m Holocene ice thinning explaining these age observations. The maximum age of the ABW profile is also evaluated depending on various estimates of the basal melt rate. It is found that the ABW record probably extends back to at least the onset of the Holocene and possibly back to the Last Glacial Period (LGP), with an acceptable vertical resolution. A spatialisation of this basal age estimate is done along the available radar profiles.

The manuscript is well written and I enjoyed reading it. The figures are generally pleasant and informative, the structure is clear, the references are appropriate.

Thank you for your positive comments on and interest in the manuscript.

Major comments

The modeling part is based on the inverse model by Martín et al. (2015). While I appreciate the quality of this model, I think it is only half appropriate in this study. Indeed, as the authors point out, there are two possible explanations of this un-steady age-depth profile: either a change of surface accumulation rate or a change of ice thickness (or a combination of both). While the Martín et al. (2015) model well explores the first option, it is not appropriate to explore the second option. A rough 600 m estimate of a possible ice thickness change is done by keeping the same $a-dH/dt$ term but assuming a is constant. But this is not accounting for the coupling of ice thickness change with ice flow! I put it as a challenge to the authors if they can come up with a more quantitative estimate of ice thickness change, possibly with a figure illustrating possible scenarios.

Thank you for this challenge! We will investigate this in more detail and look to include a more quantitative estimate of ice thickness evolution at ABW, showing some scenarios alongside our estimate where we assume no thickness change at ABW.

Minor comments

- l. 39: extra question mark
- Figure 4, legend: remove "correspond to"
- l. 281: SINCE the mid-Holocene

Thank you for these minor comments. These will be corrected in the next iteration of the manuscript.
