

General Comments

The comments here concern the second revised version of the manuscript by Seehaus et al. on changes in glacier dynamics in the northern Antarctic Peninsula since 1985. The authors have made substantial revisions to the original and revised manuscript in response to the reviewer's comments. These revisions include clarifications, corrections, additional references, as well as an added section and new figures in the supplemental material. The additional scrutiny appears to have led to significant changes in some of the reported velocity changes (individual glaciers & regions) and also to the discovery of errors in some of the figures/tables.

I appreciate the efforts by the authors to clarify their adopted approach, but would like to point out here that I am still rather unconvinced about the exact implementation of the across flow median velocity values in the presented analysis and my earlier worries on several issues still stand. The profiles presented in the manuscript's supplement illustrate to some degree the difficulties and problems that arise. If, as the authors mention in their reply document (1st reply), the erroneous/incomplete profiles were not used for the analysis there is little reason to include them, but in the way it is written currently it looks like all depicted profiles passed the quality check (Pg. 9 Ln. 12-15). Therefore, I mention here specifically: unrealistic fluctuations appear to occur along some of the profiles (e.g. Fig S155) and some profiles are incomplete (and were apparently not rejected as mentioned in the text – Pg. 6 Ln. 29/Pg. 7 Ln. 1). A crucial issue, however, is the treatment of velocity in the shear margins, some profiles seem to be cut off rather abruptly, some are smoother and go down to zero velocity, some appear to cover more glaciers and include intermediate (stationary?) areas, some have very high values at the margins (or do not seem to include the margin – e.g. S150). The sensor capabilities and to a certain degree the algorithm settings largely determine how well these margins can be captured. One should therefore be cautious when interpreting extracted values as they could reflect sensor limitations instead of real velocity changes.

That said, in concordance with my main wish in the previous review rounds, at least the methods are now better documented and the inclusion of ice velocity cross profiles in concordance with calculated velocity median values (in the supplement) provide some means of traceability. I likewise welcome the inclusion of additional ice velocity maps for this reason as it shows the source material of some of the numbers and its potential or limitation. Also, the reported ice velocity changes for Drygalski (which I took as a primary example in my previous review) has been revised considerably.

A few more **specific comments** for consideration:

- The IV profiles are extracted from close to the terminus, but it is not mentioned in the text from which year nor is it very clear from fig 1.

- In the conclusion it is now mentioned “Upcoming sensor [SIC] probably facilitate the region wide measurement of recent surface elevation, since current estimates have got only partial coverage or have got some issues due to the complex topography of the AP. “. Just a thought, what type of sensor is going to overcome the partial coverage and issues due to complex topography, or is this wishful thinking (in which case 'hopefully' is more apt than 'probably')?
- As mentioned above, please clarify whether profiles depicted in supp. material passed the quality check and if they were used or not used in the analysis.

Grammar

Pg. 2 Ln. 27: “) The” → missing point

Pg. 7 Ln. 17&18: on average

Pg. 12 Ln. 2: van der Veen

Pg. 13 Ln. 8: observed

Sup Mat.

Pg. 2 2nd paragraph: 2256 measurementS

Pg. 2 3rd paragraph: using obtained by the second approach → seems like a word is missing here

Pg. 2 3rd paragraph: This mismatch does not influencE the subsequent

Pg. 2 5th paragraph: 'is little' → is small
