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Interactive comment on "A Systematic Study of the Fracturing of Ronne - Filchner Ice Shelf, Antarctica, Using Multisource Satellite Data from 2001 to 2016" *by* Rongxing Li et al.

Anonymous Referee #2

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1 Summary

Li et. al. use multi-source satellite imagery to study the rifts on Ronne - Filchner Ice Shelf (RFIS) with a focus on a) ice-shelf wide fracture detection and propagation speed analysis and b) 2D and 3D analysis of rifts R1 and R2. They introduce this as a new framework, although it is mainly the result of an extensive implementation of existing techniques complemented by the use of 3D rift analysis using stereo images. Finally, they perform a simple linear regression to predict the next large calving event.

2 General Comment

Although the work of Li et. al. is an extensive analysis of the rifts of RFIS, the paper is ambiguous in several aspects (framing, novelty, results) and therefore need to be thoroughly revised with a strong emphasis on improving the focus of the paper, re-designing the paper accordingly and correcting the analysis and interpretation of the results. Moreover, I would like to stress the importance of the more detailed comments made by Reviewer 1, with whom I agree strongly.

3 Major comments

3.1 Ambiguity of goal

The paper is presented as a new framework (methodological focus), whereas in reality it's methodological focus or novelty is limited (using a new generation of satellites is not a methodological innovation) or is not discussed. If the paper is about implementing new frameworks / methodology, it should focus on the comparison of these methodologies (e..g how do 3D stereo images of rifts correspond to ICESAT etc.).If the paper is about the FRIS rifts (which I believe it is) it should focus on comparing the results with earlier essential works of FRIS rifts. Now this comparison is missing (e.g. to Hubble et. al. 2010 or Walker et. al., 2013) making it for an unexperienced reader impossible to assess the novelty of the paper (see also next points)

3.2 Ambiguity of novelty

It is difficult to assess the novelty of the paper given the ambiguous goal and lack of comparison with earlier essential works of FRIS (rifts). It is unclear to the reader what

this work is adding to the RFIS work of Hubble et. al. 2010 or Walker et. al., 2013). Perhaps the main novelty can be found in the stereo image analysis of the rifts, but simultaneously the paper lacks an accurate in-depth analysis of this methodology to warrant publication.

3.3 Accuracy of analysis

Li et. al. use the ICESAT and stereo images naively which may result in problematic interpretation of the results. For example, Fricker et. al. (2005) clearly show the importance of multiple reflecting surfaces within an ICESAT footprint which is essential for interpretation of ICESAT data over rifts, whereas in this paper Li. et. al. do not seem to take that into account. As such, I have my doubts about ICESAT rift depth interpretation at the walls of the rifts and/or when the rift width is small. Moreover, I doubt of the horizontal and vertical accuracy of the stereo-images can be copied from earlier studies which have been performed over non-ice/snow surfaces. Performing an accurate stereo-image reconstruction over snow/ice is a complex operation and the highly reflective snow/ice surfaces with limited unique spatial structure will most probably result in much larger vertical and horizontal errors than for typical land surfaces. As such, I wonder how accurate the stereo-DEM actually is and how reliable several results are if you assume a much lower vertical accuracy? Given the comments on ICESAT and stereo-DEM accuracy I think many of the results and conclusions should be completely reframed. This should also be addressed in the accuracy analysis.

3.4 Next major calving event?

The estimation of the next major calving event based on simple regression models is very naive and does in my opinion not correspond 'to improve the reliability of ice shelf modeling and support enhanced analyses of ice shelf stability'. The uncertainty

of the 2051 number and the relation of this number with modelling results should be addressed if one wants to make a very uncertain prediction.

4 Detailed comments

Rift propagation is an ambiguous term: is it movement of the rift with the rice or growth of the rift itself?

P5L7: four datasets? In the following section and Fig.3 only 3 are discussed

p10L28: can be used? May need? Ok, but this is all hypothetical?

p11L5-20: why discuss automation in so much detail if finally all is done manually?

P14: Changes from 1168 to 1562 fracture features needs to be separated in 2) change due to change in data-set and 2) changes due to effective changes in the field. The paper now mentions both effects, but fails to quantify the contribution of both processes. Without this separation it is very difficult to draw any interesting conclusion from this number.

Section 4.2: it seems extremely obvious that the fracture 'propagation speeds' correspond to the ice speeds (An average difference of 5 m a-1 with a standard deviation of 27 m a-1 between the two speed datasets shows a relatively high level of consistency.). Since the fracture cannot move within the ice, it would surprise me a lot of they would have different speeds. Having said that, I wonder what the goal of this analysis actually is?

Fig.6: why use a step function? This seems to indicate that the growth sometimes stopts completely?

Fig.8: uncertainty bands (correct ones!) would be very useful to avoid overinterpretation of this figure.

Fig.10: due to multiple reflection rift depths prior to complete opening are probably very dubious.

P23: “Furthermore, there is also not a strong correlation between the abrupt changes in the length and width and that of the depth. ” This is probably the result of having dubious depth data which cannot be interpreted probably without performing a more thorough analysis.

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