

## ***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 #1**

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### **Review of “A Systematic Study of the Fracturing of Ronne - Filchner Ice Shelf, Antarctica, Using Multisource Satellite Data from 2001 to 2016”**

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### **General Comments**

This is a large amount of work and a challenging estimate of the evolving behavior of the second-largest (composite) ice shelf area in Antarctica formed by the Ronne and Filchner Ice Shelves (RFIS). This study makes use of both the Landsat 8 Operational Land Imager (OLI) data (launched early 2013) as well as high-resolution stereo sensor data from Worldview-2 (launched late 2009) and the Zi Yuan-3-01 (also ‘Ziyuan’,

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launched in early 2012) satellite systems as well as data from one previous study that utilized the first MODIS Mosaic of Antarctica (MOA 2003/2004). The study updates this previous fracture map and details one or more rifts that have been forming in the Filchner Ice Shelf (FIS) area in the last 10 years or so. These large ice shelf rifts appear similar in many respects to previous ones that enabled a large calving event from the FIS front in early 1986. The study compares the motion of fractures detected in 2003-2004 and 2014-2015 to InSAR data that predates the first MOA mosaic and to Landsat-8 velocity data roughly contemporaneous to their recent OLI mosaic. From the available data sets, including previous publications and available earlier imagery, the study goes on to attempt a prediction of when the next large calving event will occur from the front of the FIS and estimates that it will be in 2051. The paper suffers from apparently hurried preparation, complicated, and difficult to interpret figures, and insufficient novelty compared to earlier studies on ice shelf rifts however.

### Specific Comments

The **Abstract** sounds more than a bit aspirational given that predicting large calving events is quite difficult and many environmental factors may very well change in the coming decades. In addition, it isn't clear if Rifts 1 and 2 are 'newly' detected (page 1 line 15) given there is no clear comparison to the Hulbe et al. 2010 paper and at least one is known from the Walker et al. (2013) paper as described later in their text. Further, the rifts appear to be visible in the 2008/2009 MOA but these data are not used in this study. While word limits for abstracts can be difficult to deal with, to omit the importance of Sentinel data from 'new data' is quite curious. This may be why isn't specified here that the Grand Chasm was part of the FIS prior to 1986. Also, see comments about acronyms below.

The first line of the **Introduction** section (1) needs to include glacial ice tongues (page 1 line 28) as an important to glacial ice buttressing (e.g. Pine Island Glacier). It needs to be specified that 'half the loss' is for Antarctica (page 2 line 2). The Wilkins needs to be named next to the Larsen B given the references cited (page 2 line 5). For a study

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of this sort, it is curious to not include 'for many decades' and also to give selected references over that time at the end of the first sentence (page 2, line 10). In the next sentence, the word 'propagate' really needs to be clarified at the outset that this means the change in length, width, depth, and distance of movement of a given rift not just net movement. Lines 13-17 may be usefully reconsidered as *mélange* is left out of the discussion at this point and it also isn't clear what the connection is between crevasses, fractures, and rifts (see Walker abstract). Also, the generalization about rift orientation relative to flow should include longitudinal rifts at the ice shelf front as these are quite important (e.g. Hulbe et al. 2010).

On page 3, the discussion of SAR (please always put the acronym in parenthesis after the words that form the acronym, there are too many instances in the paper to track), the utility of Sentinel-1 data is omitted as in the Abstract (page 3, line 3). The availability of stereo imagery significantly predates the availability of Worldview-2 and Zi Yuan-3 (no need to specify 'Chinese' given the 'nationalities' of other sensors are not similarly specified) data (page 3 line 11) and the paper is remiss in not mentioning what may have been accomplished with ASTER and SPOT (especially IPY era) stereo imagery along with appropriate references. On page 3, line 22, has the resolution of the Landsat data improved or is thus just relative to MOA, please clarify? Also, the two velocity data sets utilized appear to be based on interferometry for two dates relative to a multi-year average for 2013-2016 from Landsat 8 (page 3, line 23; references?) yet other data appears to be available (e.g. <https://nsidc.org/data/nsidc-0720>)? The use of 'floor' (page 4, line 2 and elsewhere) seems quite inappropriate for the *mélange* filling an ice shelf rift, 'shape' may be better replaced with 'structure', and 'changes' probably should be 'variation'.

The **Study area and data** section (2) jumps directly into Section 2.1 with out providing an overview paragraph. In the first sentence, the word 'individual' is used to describe the linked Ronne and Filchner Ice Shelves when 'composite' would be more appropriate (page 4, line 7). On line 9, 'It' should be clarified to mean specific parts of

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Antarctica. Further, it would be very helpful to know exactly which basins are being included in the mass balance discussion otherwise it is next to impossible to assess the numbers given (also see comments on Figure 1). The use of 'onward' to describe a discrete calving event is oddly vague (page 4, line 17). On line 21 of page 4, the text would better read 'rapid development of large rifts precedes'.

In Section 2.2, it would be useful to clarify 'fracture propagation' (page 5, line 13) as you appear to mean in the flow direction although it also means rift expansion. You should probably also include some discussion as to why the second MOA data set was not suitable for inclusion in the analysis and why insights from Walker et al. were not utilized. Further, a good deal of the information on Page 6 (lines 4 to 18) is not needed here, just point to appropriate references (what are CE90 and LE90?). Otherwise you would have to include the same detailed description of the WV-2 instrumentation (this also applies later in the paper). Discussion of base-to-height ratio can be confined to later portions of the paper (if needed). You should be selective in the references used (page 6, line 23). For example, you should include the subsequent comments in the Journal of Glaciology if using the Zwally et al. 2015 paper. Also, the Xie et al. reference appears to be only a regional study not a broad mass balance study. I suggest you use Sorensen et al. (2011, The Cryosphere) for Greenland and Babonis et al. (2016, ISPRS) for both Antarctica and Greenland. The combined use of different imagery types (page 8, lines 1-7) calls out for some mention of what the differing resolutions means to the overall analysis (see comments on the consistent use of Sentinel-1 data, missing in Table 1, but found in the text on page 8).

The **Methodology** section (3) should start off with a summary of what is being mapped and how from the disparate data sets were utilized rather than a graphical summary (Figure 3). Currently the first sentence just repeats the caption for the framework figure that seems quite out of place in the paper but appropriate for a talk perhaps. The phrase 'long-term' (page 9, line 7) is out of place given the main focus of the paper is on <20 years of data. Later in the study, when insights from the 1950s are included,

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would be a more suitable time to use this phrase. Please add a discussion of ‘fracture extraction’ (page 10, lines 1-2) given that you indicate that some are snow covered and you opted for manual mapping. Please be specific when using terms like ‘rate’ (page 10, line 4) or ‘speed’ (elsewhere) to indicate whether you mean the lateral motion of fractures toward the ice shelf front or their overall extension or both. Either add ‘high-resolution’ before ‘3D’ (page 10, line 6) given that you have elected to not include ASTER and SPOT DEMs in the study. The sentence (page 10, lines 10-12) is best suited for the summary at the end of the paper. This whole paragraph could use some editing to focus its message.

In Section 3.1, the ‘sampling theorem’ (page 10, line 17) needs more detail and supporting references as it seems quite odd that fracture tips need to be 30 m wide in order to be detected reliably in Landsat 8 panchromatic imagery. Further, the question of being able to detect fractures that are snow covered needs to be detailed. Further, it isn’t clear to me from all the information provided on filtering etc. . . as to how that did or didn’t matter to the manual extraction of fractures. This includes discriminating blowing snow features given their relatively ephemeral nature relative to long-term fracture features.

In Section 3.2 it appears that ‘correspondences’ (page 11, line 22) means the size, shape, location, or other distinctive features of a fracture as it evolves, please clarify? This also applies to ‘corner points’ (page 12, line 4) and how a 3D anaglyph check (page 12, line 6) was accomplished? Further, it isn’t clear how smoothing the velocity fields from the two time periods used in the study (page 12, line 11) allows an improved comparisons to the fracture lateral propagation data? Doesn’t that just reduce the magnitude of anomalies?

In Section 3.3, the displacement value of a tie point of less than ‘2 mm’ is used (page 12, line 21) but it isn’t clear how this relates to the native resolutions (2.1 to 3.5 m) of the different cameras on board ZY-3? Is this a typo? And again, much of the subsequent text should be condensed to reduce excessive detail with the use of appropriate

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references. Only novel techniques not documented in the literature need such fulsome remarks. The discussion of base-height ratio needs to be clarified here as to its importance to the results given the ZY-3 value is presented earlier as 0.87 and then as a range in Table 1. It is discussed later (page 25, line 10) but it isn't clear why the value is halved from the 'nominal'. The two WV-2 DEMs in the study with only partial rift coverage makes the text's statement of the value of such high-resolution DEMs for 'ice shelf stability analysis and the improvement of physical models of ice shelves' (page 13, lines 12-13) seem speculative at best. This statement is also not aided by their data being only a small part of Figure 10. A further illustration of misleading statements is given by "ICESat tracks are generally perpendicular to the rifts of the Antarctic ice shelves" (page 13, line 14) as this is clearly not true in the Antarctic Peninsula just as one example. This is compounded by stating the 'a number of points (laser shots or footprints?) along an ICESat track may fall in a rift' (lines 14-15) when Figure 10 apparently shows only 1 footprint 'inside' the rift in almost all profiles plotted. See associated comments regarding Figure 10. The use of 'Universal Polar Stereographic' is not as clear as stating something specific such as 'EPSG:3031' (page 13, line 21). Further, registering the 'high-resolution' DEMs to the ICESat data is concerning since the ICESat data is from many years earlier and further inland and also requires its own tide correction as is apparently indicated by the variability in the ice shelf surface in Figure 10. There is no sign in the text that this was done nor that the ice shelf surface captured by the ICESat profiles was then projected forward in space and time to be more compatible with the DEMs (see evident slope in Figure 10, Panel c).

In the **Results** section (4), it would be very useful to discuss, and ideally show, selected examples of snow-covered crevasses that were detected in the Landsat 8 OLI data (and not in contemporaneous MODIS data for additional value to other researchers). And related to this, it would be useful to discuss what can and cannot be seen in the 15 m Band 8 panchromatic imagery from Landsat 8 even for 'open rifts' given the '2 pixel wide' sampling theorem mentioned previously (page 13, lines 27-28). At the top of page 14, much of what is discussed was previously mapped by Hulbe et al. (2010)

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but there doesn't seem to any discussing or visualization of how the new analysis is the same or different (one assumes 'IS' = 'islands'?). The text regarding the number of rifts needs clarification given, apparently, 11 rifts perpendicular to the ice shelf fronts that are distinct to the four large (how is 'large' defined?) and apparently transverse rifts that are assigned a number (page 14, lines 13-14). What kind of open rifts are the other 100+ features identified?

The last paragraph on Page 14 speaks to the need to utilize the 2008/2009 MOA, and or an earlier Landsat 7 mosaic from the same time as the first MOA perhaps, in this analysis to help clarify which fracture features now seen in the Landsat 8 mosaic are due solely to improved imagery resolution compared to the MOA mosaic and which are due to ice shelf evolution – this is crucial aspect of the overall analysis isn't it? Further, both Figure 4 and 5 suffer from an inability to distinguish details of the two RFIS analyses and their combined propagation information. Also, the text discussing the loss of longitudinal rifts during other calving events appears to suggest that new ones formed recently 'in 2014/2015' but it is not documented how/when they were detected. Also, oddly given their role in the 1986 calving event, only 4 of the 11 'Longitudinal Rifts' (= 'LR' apparently) are identified in Figure 4 (and only Fig. 4, page 14, lines 20-25).

The discussion of transverse Rifts 1 and 2 (page 15) is apparently based on the multi-sensor data sets but this is not stated so it is hard to take the use of 'recently' (page 15, line 12) literally given the results presented in Figure 6 (~2006 and 2010 for R1 and 2, respectively?). Similarly, the text that 'they may lead to major calving events' seems better suited for the Discussion section or the Conclusions (page 15, line 12). Also see comments on being able to discriminate the different sensors in the time series shown in that Figure. This text block also seems oddly ahead of itself given that Figure 5 is discussed in the next section (page 16). It really must be discussed if Figure 5's '651 corresponding points' (page 16, line 5) are from individual fractures observed in both the 1168 features identified in the MOA study and the 1562 features observed in this study or if some are from different points on a given feature observed twice. Also,

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the percentage of features NOT observed seems quite high and should be discussed further than there are 'differences in resolution'.

Also, this section could use geographic indicators such as the features on the figures or, with a latitude longitude ID from a grid added to the figures for locations in the midst of the shelves. Further, it isn't clear if there is a pattern to the velocity differences that might be related to changes in ice shelf thickness between the two time periods studied? As stated elsewhere, please replace 'frontline' for 'ice shelf front' or similar wording. Further, the longitudinal rifts at the shelf fronts might usefully be examined for their influence on apparent velocity differences between the inland shelf ice and the ice closest to the front (1400 m/yr vs a max of 1270 m/yr). See the need for additional velocity data noted earlier.

Section 4.3 brings the discussion back to the evolution of Rifts 1 and 2 (page 17, first paragraph, see related text on page 15). From the list of imagery sources, Sentinel-1 data was either not used or not mentioned, it isn't clear which is the case. In the next paragraph (page 18, lines 2-6), rates for Rift 1 are given in either m/d or m/yr and the dates associated with the values are only specified for the major changes in 2006. One assumes the multiyear rates are derived from the slopes of the data shown in Figure 6 but that isn't clear. For Rift 2, the large observational gap (page 18, line 15 to page 19, lines 1-2) could have been augmented with MODIS data almost certainly. This would also have provided a test of what can and cannot be measured in the lower resolution imagery. Rates are given in both m/d and m/yr but again only limited temporal information is given in the text.

For Section 4.4, it would be useful to spell out 'bundle adjustment' as this is not a common acronym for most readers (page 19, line 10). Further, it would be good to actually see the ZY-3 images to assess what clouds may have obscured the area as it is clear that only the immediate area of the rift was processed. Again, use of the term 'floor' for the mélange is quite hard to accept given it has height and thickness (structure) within the rift (page 19, line 16). Given that the ZY-3 DEM does not fully

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capture the rift in 2014, it is hard to accept the stated '44463 m' length of the rift at that time (page 19, line 18) as anything more than an estimate the rift was 'greater than 44.4 km long' unless other data was used? Also, the significant digits stated in the text is further undercut by the 500 m smoothing window and related text shortly thereafter. Figure 7 needs to make clear what smoothing has or has not been applied as well.

The text in the paragraph at the bottom of page 20 needs to be reconsidered. I'm not sure what an 'easement transition' is but the data plotted in Figure 8 should be discussed in the order plotted rather than intermingled as it is currently in the text. The 'rift surface' (page 20, line 10) appears to be from a location outside the rift itself but this is not clear from either the text or the figures. For both the 'rift surface' and the 'rift floor' the usage of 'flat' seems belied by the data in Figure 7b (apparently after smoothing). Figure 8 shows a ~5 m pinnacle toward the western end among other elevation variations (also see the mélange 'structure' in Figure 9). I cannot find the 'cavity' indicated by the text (line 11). For 'rift base' (page 20, line 17), see other comments on the mélange having height and thickness relative to sea level and the apparent lack of tide corrections to the altimetry data.

The discussion of the ICESat data at the bottom of page 22 and top of page 23 (seems to acknowledge that the laser shot spacing is too coarse to determine rift width (page 23, line 3) at Rift 1 but fails to acknowledge that the few shots that did apparently hit the rift may have been partially on the edges or walls as well as partly within the rift given the ~70 m footprints. Further, it is crucial to note that Figure 6 indicates the maximum width of Rift 1 did not reach 400 m until ~2008 and was much thinner for most of the ICESat campaigns. This is the likely part of the explanation of the variable rift depths from the two ICESat profiles prior to 2008, separated by only ~6 km near the center of Rift 1.

In addition, the text in the next paragraph needs to be reworked given that fractures are inherently 'pointed' toward their tips given how fractures propagate in length (a reference here might be useful as Rift 1 appears to be quite 'normal' in this respect).

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The usage of 'flat' followed by 'high roughness' is also problematic. See the previous set of comments as to whether the ICESat data really show that the rift deepened in 2003-2008.

The abrupt changes (page 23, line 12) in length and width may be real for Rift 1 but a large observational gap makes this statement uncertain for Rift 2 (see previous comment on checking this with MODIS). In any case, the depth estimates from the altimetry prior to 2012 for Rift 1 are problematic and so most of this paragraph appears to relate to Figure 6's data. Most of this text block should be removed to the conclusions in any case.

The next section on accuracy starts with projection information that should be detailed earlier (page 23, lines 20-26). At the end of this page, the previously given RFIS becomes the FRIS for some reason (common usage is Filchner-Ronne). In addition, the analysis of the outcrop positions uncertainty in the area from both the 2013/2014 Landsat 8 OLI mosaic and the LIMA mosaic, composed of Landsat 7 ETM+ images, seems to attribute any resulting 'positional accuracy' (differences) all to the OLI imagery when it is highly likely to be proportional to the relative geolocation accuracy of the pair of Landsat satellites (page 23 into page 24). Further, the 'high level of consistency' is impossible to assess given that there is no detailed, unsmoothed spatial map of velocity differences (page 24, line 11). The uncertainties discussed for the various sensors should be added to Figure 6 (page 24, lines 12-15, and see comments on revising that figure to discriminate sensor type).

The discussion of the issues limiting the ZY-3 DEM (page 24, line 16 to page 25, line 16) also appear to apply to the WV-2 DEMs but this is not detailed. In any case, this material is far too long for the Results section and should be reduced/removed to the 'Data' section to qualify all such high-resolution DEMs or added to the Supplement to detail what can and cannot be done in such situations. This would be of interest to glaciologists as these types of imagery data are becoming more widely available. One remaining issue not covered here for both the WV and ZY data is the degree to

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which various levels of smoothing remove both noise AND signal from the final altimetry products. The usage of 'daft' is a unique typo.

The **Discussion and Conclusions** section (5) begins with the problematic 'increase' stated for the number of fractures but without providing contemporaneous comparisons of shelf areas clearly visible in both MODIS and Landsat imagery. As commented on previously for the Results, there seems to be a great deal of overlap with the Hulbe et al. (2010) study and or Walker et al. (2013) studies so this paper must make it clear what is really new from this study (page 26, lines 1-7). The velocity variation discussion also makes one wonder why more temporal resolution was not attempted for both fracture propagation and inter-comparisons with other velocity maps (is 40 m/yr a significant variation over more than 10 years given the two types of data utilized?, page 26, lines 8 and onward). The related discussion for Figure 12 is similarly compromised by a lack of temporal resolution. The use of velocity data from only two time periods ensures a dramatic jump although this study shows that Rift 1 expanded significantly starting in 2006. See previous discussion of Figure 2 to see if 'marine ice' is the appropriate parameter to relate to change given that the average 'basal' melting (a key word, missing from line 27) and shelf thickness change over >10 years is not well defined, at least in the data presented here.

The analysis presented in Section 5.2 is interesting and might qualify for a short paper in its own right but is speculative given very sparse data on propagation of the rifts responsible (both transverse and longitudinal) prior to the April 1986 calving as well as details on the longitudinal rifts mapped in this study. The underlying assumption of 'We assume that the overall setting of the FIS before and after the 1986 event was not significantly changed' (page 30, lines 5-6) which implies conditions are unchanged to the present day. The results from Paolo et al. (2015) seem to be entirely dismissed by this assumption. That said, the resulting estimate of the 'average' time to the next large calving from FIS seems possible yet the 'unknowns' appear to be too large to take the estimate too seriously. It is hard not to think that only in iceberg calving prediction

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would a 15-year difference between estimated calving events be called ‘very close’. In short, more data, and as the authors note, ‘better models’ (page 30, line 27) are clearly needed to make a ‘good’ prediction but I commend the authors for trying to do so given the obvious uncertainties.

And finally, a second **Conclusions** section (6) attempts to summarize the study. The phrase ‘a new framework of systematic ice shelf fracture mapping’ seems to be an overreach given that studies led by Hulbe (and/or Walker) appear to have conducted similar studies albeit with different goals. The utilization of newer data sets clearly improves overall resolution of fractures but this is not fully tested relative to other imagery data sets including, especially since 2014, the Sentinel-1AB radar and its interferometric results.

The larger areas covered by ZY-3 stereo imagery, relative to Worldview-2, is certainly of interest to rift propagation studies but the impact of shadowing and apparently required smoothing to reduce noise suggests the need for further calibration/validation (clarify the text’s use of ‘quasi real-time’ given these data were acquired in early 2014, page 31, line 10). In the second paragraph in this section, there needs to be much more careful writing. There is only casual reference to ‘basal’ melting in this study; it remains unclear if marine ice or overall ice shelf thickness is the more important parameter to ongoing changes to the FIS; ‘rapid changes’ appears to refer to the 2D data in Figure 6; and only Rift 1 was (almost completely) mapped in 3D. The ‘most active fracturing activities’ at the FIS front needs further support in any final paper as does the increased number of fractures given the differences in imagery resolution over the two time periods.

### Figures, Tables, and Supplement

Figure 1 needs to have a latitude longitude grid added rather than the somewhat random tick marks. Also, the spurious black box needs to be removed; it was apparently meant to connect to the Antarctic inset map but it mostly serves to indicate the authors were rushing the paper into the review process. Further, one has to assume that by

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'features' in the caption they actually mean 'grounding zones'. Also, the ice shelf front would usefully be a distinctive color that is then also used in Figure 4 to distinguish the front positions from grounding zones. Also, as it is discussed in the text, the basins in the inset flowing into the RFIS area should be highlighted in a larger version of the inset map including a distinction between WAIS and EAIS areas (the scale bar could be moved to the side with it). Also, given the time period of this study, why wasn't the 2008/2009 MOA used as the base image?

Table 1 also has a number of issues including an obvious typo in the main header ('D' missing in the pdf). The column regarding 'base-height' ratio is not needed and it is sufficient to discuss this detail that applies only to the WV-2 and ZY-3 imagery. The ICESat data should be described as 'nominally' 70 m footprints with 172 m spacing along track. Also, the number of points is an odd way to describe the 'quantity' of the laser altimetry data utilized – was this for each of the two tracks for each of the 6 or so campaigns plotted? Finally, the data sets in each subsection should be given in consistent date order, either youngest to oldest or the reverse. The use of Sentinel-1 data is apparent from the text, also Figure 3, but is omitted in the table.

Figure 2 is quite complicated and needs to be reworked substantially. I strongly suggest a pair of figures side-by-side to show the 'before' using a Landsat image composite from that era relative to the Landsat 8 image composite from 2013. Critically, the date of the 'Grand Chasm' 1986 imagery is incorrect on the figure – there is a partial view on March 1, 1986 or a partly cloudy full view of the rift on February 4, 1986, both from Landsat 5 TM (oddly, this is mentioned at the end of the paper although the sensor is omitted). Some clear explanation of what the colored 'wedges' mean as is also called for given those important rifts are not 'transverse' to flow (also green text on a dark grey background is very difficult to read). It seems that the 'bottom marine ice' layer mostly obstructs the details of the base imagery from being observed (the color scale is far too small, has an odd range (0 to 186 m?), and is difficult to resolve even with magnification in the pdf (black printing on dark blue). Further, it isn't clear that the 'marine ice' is as

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important to the study as the overall shelf thickness and also the 'flow volume' feeding into that portion of the FIS. Chuter and Bamber's (2015) recent thickness map show the ice is thinner next to Berkner Island (which is not labeled) and this suggests this is 'local ice' from Berkner Island's 'streams' (page 14, line 10) that are actually smaller outlet glaciers (see also the Hulbe et al. 2010 main map). See comments on this part of the text as well. The inset for the stereo coverage is not labeled as the basis of Figure 7. The location of the ICESat profiles needs to be stated as 'approximate' given that the ICESat data crossed Rift 1 inland of where the rift is shown in the imagery (see also Figure 7). Also, throughout the paper 'frontlines' or similar usage should be replaced with 'ice shelf front' or 'front position' as mapped at a specific time.

Figure 3 seems appropriate for a talk but does not add substantially to the paper that spends many paragraphs on methodology. This figure also doesn't include the historical data discussed near the end of the paper although it does include the Sentinel data at least.

Figure 4 in particular needs to be reworked. Overlaying the 2014/2015 crevasses and rifts as separate colors on top of the combined crevasses and rifts from the Hulbe et al. analysis makes the figure a mishmash of colors lacking clarity even when at considerable magnification in the pdf. As with Figure 2, a pair of images showing what is, and isn't, resolved in the lower-resolution 2003/2004 MOA data relative to the higher-resolution Landsat 8 mosaic seems important to show the reader (ideally with very large versions online) especially as this is the basis of Figure 5. This also begs the question of why the 2008/2009 MOA version was not part of the study. All maps should have a latitude longitude grid as it isn't clear that Figures 1 and 4 cover the same areas. See comment above about using different colors to discriminate between grounding zones and ice shelf fronts (also why only some grounding zones?).

Figure 5 is an interesting composite of data sets but only by referring to Figure 4 can you get a sense of what is apparently not observed in the 2003/2004 MOA data given that many fractures in Figure 4 do not have corresponding arrows in Figure 5 (note

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the % of fractures not observed in both data sets given in the text). Also, the arrows are far too dense to get a sense of their individual multi-year average flow speeds. Insets may help with this problem as may color coding the arrows or, as a last resort, manually editing the number of arrows in the figure (again, a larger version could be shown online). Also, having a single arrow whose length does not correspond to the more recent composite Landsat velocity color scale is not helpful (also why the random velocity increments in this scale?). Curiously, the Landsat velocity composite appears to map a large area of fast ice (not labeled) in front of Berkner Island which makes it hard to see the two ice shelf front positions (this should be masked). There are again 'selected' latitude longitude tick marks rather than a clear grid (area appears to be the same as in Figure 4).

Figure 6 suffers from a lack of clarity as to the 'points' where the various imagery was actually measured and the 'lines' connecting those points; they should be distinctly different from the actual data. Further, though it is too small to see as presented, it is clear that the mix of imagery used (see text) have different accuracies or uncertainties. Larger 'horizontal plots' would allow the points to be color-coded by type of imagery and might allow the 'variability' of different sensors to be communicated to the reader with error bars. The dates should be standardized to show the first of each year and a grid added with 6-month increments so that summer and winter observations can be easily discriminated. Why does the length axis allow for 'negative' lengths? Some common horizontal grid lines between the two scales would also aid the reader. Panel c has a wildly undersized scale bar and the area shown is not an inset on any of the earlier maps as far as I can tell. All the text in the figure panels is too small as well.

Figure 7 similarly suffers from size issues including the elevation scale, text labels, and the main image in Panel a (should refer back to Figure 2). Further, the perspective view is not clear on the figure itself (the text says from Profile 1 looking east – how far?). Also, Panel b would be far more effective if the colorized elevation data were paired with the associated high-resolution DEM imagery showing the rift walls and infill. I'm

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not sure what the small 'color object' is next to the extraneous '(b)' but they should both be removed (see previous concern about 'rushing' the paper into review?). Finally, Figure 2 indicates there is stereo coverage over the whole box (as well as some WV-2 coverage, not indicated here) but only the area along the rift was processed, all due to cloud? Showing more detail of the full coverage would be very valuable to potential users of ZY-3 data. Was Figure 7 spatially averaged as is indicated for Figure 8 or is this the '5 m' DEM data? Either way, the caption should state this important detail. The location of the ICESat profiles needs to be stated as 'approximate' given that the ICESat data crossed the evolving rift inland of where the rift was found in 2014 (see applies to Figure 2).

Figures 8, 9 could easily be combined and should refer back to Figure 7 explicitly (the end of the profile 'N' needs to be shown on Figure 7). This includes showing the position of the 'ice shelf surface elevation' (not 'rift') profile. Were all the plotted values smoothed with the '500 m moving average'? If so this needs to be clearly stated in the caption. Also, the 5x5 (25 m x 25m?) smoothing applied to the rift bottom surface roughness needs to be stated in the caption (does this remove actual ice pinnacles that appear to be shown in Figure 9?). Further, the two levels of smoothing make one wonder about how good the data really are.

Figure 10 is an interesting comparison of what can and cannot be seen using a low-resolution satellite altimetry profile (~172 m between ~70 m ICESat footprints) relative to three more recent high-resolution DEMs that cross the rift between where approximately the two ICESat tracks crossed it. The plots show both the change in position of the rift on the moving shelf (were velocities derived from these position changes?) as well as an apparent increase in depth of the rift prior to 2010 from the ICESat data. It isn't clear why the DEM data from roughly the positions where the ICESat tracks crossed the rifts aren't shown, only the data along D-D' which seems to exaggerate the recent change in width relative to the ICESat data. And, although the text cautions that the ICESat data cannot be used to assess rift width, a similar caution is not

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made for the apparent depths in the caption. The ‘gently curving shoulders’ at the rift edges in the ICESat profiles relative to the sharper DEM rift edges indicate this issue; ICESat footprints may be partially across the shelf and into the rift to some degree as well as possibly directly in the rift. The only way to know this for sure would be to find temporally equivalent imagery and to plot the ICESat data in detail across the evolving rift. A color-coded plot of the ICESat data in XY space (with color for depth, Z) might help show this issue. Finally, it is not clear if any appropriate ocean tide corrections have been applied to these data and the elevation axis is apparently relative to the ellipsoid rather than mean sea level. This correction and clarification would enable some estimate of mélange thickness to be made for the recent stereo DEMs.

Figure 11 and 12 could also be combined. More critically, the velocity data in the Rignot compilations (<https://nsidc.org/data/nsidc-0720>) would be a useful addition especially using different periods after the 1997/2000 data used as the baseline. This would allow the expansion of Rift 1 that is quite evident when comparing the 2003/2004 MOA and the 2008/2009 MOA imagery to be better temporally resolved. As it is, all that is shown is that at some point in a ~15 year period the ice north of the rift began to accelerate.

Table 2 is an interesting summary of the similarities of the earlier rift in this part of the FIS and the current pair of rifts. Some clarification is needed including the table’s text indicating the ‘2 rifts joined’ in 2011-2012 (?) and why the ‘Average change rate of Rifts 1 and 2’ is only 2011-2016 not from 2004 given that the Grand Chasm has a much longer averaging period. Does this matter or not? Also, it would be useful to have some  $\pm$ uncertainty values on the depths and widths. The spacing between numbers and units should be standardized.

## Supplement

While interesting, the calculation in this section appears highly speculative (also see Table 2 comments) and this reviewer has elected to comment only on the main paper as it needs a great deal of attention prior to further consideration.

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