

Interactive comment on “Changes of Wilkins Ice Shelf over the past 15 years and inferences on its stability” by M. Braun et al.

M. Braun et al.

Received and published: 8 March 2009

General comments: This paper reviews recent changes on the Wilkins Ice Shelf on the Antarctic Peninsula. [...] My specific comments follow, but I take the opportunity to make one comment. There is a morass of emerging terminology regarding ice shelf retreat. (I accept a share of the blame in this regard, but certainly not all of it). Terms like "break-up", "disintegration", "collapse", "retreat", all appear to be used interchangeably, and really without much clarity. To me the term, "retreat" satisfactorily describes an ongoing process that probably occurs over periods of several years to decades. Whereas "break-up" implies the kind of change that occurred on Larsen A and Larsen B over periods of weeks to months. However, I note that several authors use "collapse" in this regard, and that seems even more descriptive. In this paper, I never really understood if the authors were drawing a real distinction, between "collapse", "break-up"

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Interactive Discussion

Discussion Paper



and "distintegration".

ANSWER: We agree that different terminology has been used for similar phenomenons in the literature and that we ourselves also lacked to use a consistent terminology. We now define the following terms in the first paragraph of the introduction:

Calving: Ordinary mass loss mechanism of an ice shelf. Time scale: month-years.

Retreat: Reduction of size of an ice shelf, while it still remains existent. Time scale: month-years.

Disintegration (from the latin dis-integrare = to destroy the unity of): Complete disappearance of an ice shelf . Time scale: no constraint.

Break-up: Sudden, fast release of fragments of variable size. Often also small, sliver icebergs, are formed. Time scale: hours-days.

Collapse: Disintegration on short timescales. Time scale: days-weeks.

This terminology is now used throughout the entire manuscript.

Overall, I believe that this paper requires some considerable editing and tightening up. It makes too many poorly argued but potentially influential statements; and does not appear to offer any really clear conclusions. There are some important and valuable aspects to the paper (new velocity fields, insights into the role of ice rumples in fracture) but these are actually not given the space they deserve in the discussion and so appear to be presented in summary. It's hard to recommend a simple route to improving the paper. If it is to cover all the same ground, and fully support its conclusions, it may need to be considerably longer, alternatively, if one of the many themes was to be pursued in a more streamlined way, much of the material that the authors have assembled may need to be discarded.

ANSWER: We apologize for too influential statements of which we have not been aware of in the manuscript preparation. The reviews and the short comments were very

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Discussion Paper



helpful to recognize and correct them. While gathering all the material, we probably lost the view for a streamline. We followed the suggestion of the reviewer and excluded some of the material. In particular the recommendation of both reviewers to create a map of the structural characteristics was very helpful and allowed to keep some material (e.g. dolines) while still tightening the manuscript.

Specific comments:

I attach a .pdf file that includes specific recommendations for changes in the text, for the purposes of improving readability and grammar. Those comments are not significant to the quality of the manuscript. However, there are several places where the text was difficult to understand and sometimes ambiguous, and I believe editorial effort would be required to make the manuscript publishable.

ANSWER: All recommendations of this pdf-file have been included.

Page 342:

Line 3: ANSWER: We have completely reworded the introduction section and tightened it up in regard to the focus of the paper. We also corrected these statements and included the terminology definition we refer to in the paper.

Line 4: ANSWER: Done in revised manuscript

Line 8: ANSWER: Done.

Line 19 -20: ANSWER: Changed.

Line 26: ANSWER: Changed.

Page 343 Overall, the introduction is generally just a long list of previous observations with virtually no synthesis or judgment or criticism, attached. Up to a point this is not a problem, but it really doesn't set up the problems that will be addressed later in the paper, and in places presents apparently contradictory evidence without comment, which is very confusing for the reader.

ANSWER: We agree that the introduction was not focused enough on the relevant information for this paper. Hence, we have fully revised the introduction section and tightened it up.

Line 1: ANSWER: Following our definition of the terminology, 'break-up' and 'disintegration' are not identical. The use of the terminology has been adapted everywhere.

Line 2: ANSWER: It is correct, that not all of these ice shelves have lost the entire mass and we have changed the introduction distinguishing carefully between the types of changes of ice shelves that were classified. However, a fact file of the British Antarctic Survey showing the size of the floating area in various years including 2008 reveals that Prince Gustav Channel and Larsen A are entirely disintegrated.

Line 13: ANSWER: As our focus of the paper is not contribution to sea level rise but structural changes and break-up of WIS, we have excluded all these statements as they are not really relevant for the paper.

Lines 19-21: ANSWER: See above.

Line 22 -25: ANSWER: We agree that this might be confusing for the reader. Our paper is not focusing on the analysis of passive microwave data nor are we interested in opening up a discussion on possible ambiguous findings due to e.g. different record lengths, algorithms for processing etc. We also do not intend to fully review the knowledge on climate change of the peninsula. Our intention is to provide the reader with the information that there are observed regional climate changes that might be highly relevant for our observations. Hence, we quote respective literature. However, we agree that the phrase on passive microwave results might be confusing, hence we changed the wording here as we did for the entire introduction.

Line 26: ANSWER: We did not link water temperatures of the West coast with thinning on Larsen. But we agree that the statements coming after each other without making this clear might cause this impression. We have changed the entire introduction.

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Page 344 Lines 5-10: ANSWER: We have revised this paragraph and included these references.

Line 8: ANSWER: Rephrased to 'The -9°C annual isotherm is being suggested an approximation of the limit for the viability of ice shelves (Morris and Vaughan 2003, Vaughan et al 2001)'

Line 22: ANSWER: This sentence has been removed as the review is given in the subsequent section.

Page 345 Line 4: ANSWER: Done.

Line 5: ANSWER: The statement is indicated from Vaughan et al. (1993) and from our work. To emphasize that surface accumulation is nevertheless dominant, we included this sentence: 'Although surface accumulation is dominant, there is some contribution from inflow.'

Line 12-13: ANSWER: We have changed the color bar of Figure 3 in order to make this ice thickness changes more visible. The corresponding text has been clarified.

Line 18-26. ANSWER: We apologize for this. The reference is included now.

Line 27 - 4: ANSWER: We agree that these observations would require more discussion. However, it is not the subject of the paper to review results from passive microwave data nor to comment extensively on the processing, length of time series etc. We give proper reference on this and also state that there seem to be ambiguous results. Going into more details will not help to make the paper more concise what is requested by the reviewer at several occasions. However, we followed the recommendation of the referee and refer now to all three papers in this section regarding general findings for the Antarctic Peninsula and outline the specific findings for WIS in section 2.

Page 346 Line 20: ANSWER: The reviewer is right that in the context of our paper this work needs more consideration. As it is a review of ice shelf retreat on the peninsula

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with a strong focus on the Larsen events and with less specific details of Wilkins, we refer to this paper now in the introduction.

Page 347 Line 7: ANSWER: Yes, we refer to the dataset we have available and also clearly state its origin in the subsequent sentence, the GIA (Sievers and Bennat 1989). We do not claim that there are no other Landsat datasets of this area, but have clarified this 'The Landsat dataset available for this study consists'.

Line 14: ANSWER: We deleted it.

Line 20-28: ANSWER: The reviewer is right that normally a list of the used images is provided. In this case, we really based the analysis of time series of SAR and multi-spectral images e.g. in 2008 ENVISAT deliver several images a week. Only a very few images of the collection are displayed in the paper with respective dates, but the multitude of information provided by the time series is in fact used to look on changes and timing etc. Hence, we do not see that such a very long table stretching over several pages could really fulfill this task and be of help. We believe references to the respective dates in the text fit the purpose much better and keep the paper tight.

Page 348 Lines 14-16: ANSWER: We agree with the reviewer that these are major limitations of the data set on the ice shelf. We were aware of this point and hence we provided the details on how the velocity field was generated. We also state that this limits the absolute velocity information, but that the relative field provides new information. The difficulty is indeed that phase unwrapping over the grounding line was not feasible. In the area only very few structures are available that can easily be tracked. Some efforts have been undertaken based on Landsat and ASTER data and we mention that this did not reveal changes in speeds. However, the limitation of this tracked information is also that the features are in areas of rather low flow speeds and hence the spatial resolution and registration of the image is a critical issue of quality. Furthermore, most of these features undergo changes of shape (e.g. dolines due to compression) and the repeat Landsat images range over 1986, 90 and 2000 and in

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Discussion Paper



particular the later image is after the major changes on the ice front. Hence, it is difficult to judge the absolute quality of such tracking information over longer periods. One of our next analysis steps targets to look into velocities and possible changes using speckle and offset tracking based on the comprehensive SAR archive. This shall also incorporate data closer to the ice front. However, this is certainly beyond the scope of this paper and we will take up this issue and further discuss the here presented finding when a very detailed analysis is done and hopefully more information became available.

Line 26: ANSWER: Yes, although the German ä is in German equivalent to ae, the correct anglicised spelling is Handel. It has been changed in the text and Fig.1.

Page 349: Line 11: ANSWER: Yes, we agree. The paragraph has changed entirely and the parenthesised quote is now an entire sentence.

Section 3.3: ANSWER: Yes, a direct comparison between the ICESat and Geosat data would be valuable. A comparison would however require a careful analysis of the entire GDRs from Geosat in order to assess the accuracy (see the discussion in Cooper & Hinto, 1996, 'Correction of satellite radar altimeter data on ice-covered surfaces in Antarctica using an integrated Geographical Information System', Int. J. Rem. Sens. Vol 17(7), 1367-1376). Having a more streamlined manuscript in mind, this would be contradicting and thus we encourage the reviewer to compare the ICESat tracks shown with the elevation shown in Vaughan et al. 1993.

Section 3.4: ANSWER: Section 3.3 explains the ellipsoid / geoid conversion: 'Elevations are transformed from the Topex/Poseidon ellipsoid to the OSU91A geoid, so that the given elevations are freeboard heights.' Following the short comment of M. King, we have improved the conversion into a newer geoid model. Therefore, the according lines in Section 3.3. were changed.

Page 351 Line 8: ANSWER: Wording has been changed as recommended to 'record'

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Line 8: ANSWER: This is a very useful comment and we include now a sentence on this referring to Scambos et al. (2000): 'Scambos et al. (2000) already stated that the positions of the northern and northerwestern ice front were stable between 1947 and 1986 based on a map of the American Geographical Society showing the ice front from 1947 and historic maps and imagery of the Defense Intelligent Satellite Program from 1961.'

Line 25-27: ANSWER: We followed the suggestion of the reviewer.

Page 352 Line 14-16 ANSWER: We agree with the reviewers comment. Re-considering the pattern of melt features and the poor knowledge of the prevailing meteorological conditions in this area make it difficult to keep such a definite statement. We have hence eliminated this sentence.

Line 19-5: ANSWER: We have revised the discussion of the dolines and make it now less absolute in terms of numbers as the unique identification is not always easy in the optical remote sensing data. Nevertheless, the numbers indicate an increase. We have now extended the discussion of these surface structures following Bindschadler et al. (2002) and include a possible relevance of the increased number. Their location is now shown in Figure 5.

Page 353 Line4 -8: ANSWER: Yes, there is something new. We apologize that we missed to cite Swithinbank (1988) here. Swithinbank identifies a 2x2km lake, while we found this to be 15km² open ocean. The formation of a lake (either tidal or melt pond) is considerably different. However, the reviewer is entirely right in highlighting that we didn't focus on the new versus the old information. We have changed these lines to:

'Figure 5 displays an area of 15 km² open water between Dorsey Island and the adjacent coast of Mozart Ice Piedmont. Swithinbank (1988) identified this as a 2x2km lake.'

ANSWER: Since the original Fig. 5 has been removed and replaced with a map of

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the structural characteristics, the text refers now to a polygon in this map only and no longer to a separate panel in a figure.

Line 8-18: ANSWER: We admit that there is no proof that satellite imagery reveal the plasticity and thus we should not say that we 'detected' a plastic zone and therefore we deleted the sentence. As we suppose that we deal with an ice rumple, which does not divide the ice flow, we do not see a reason why the ice should be considerably thinner downstream. The 1998 break-up stopped eastwards along this line, which is the reason why we think it is has rather the properties of a plastic zone. However, we just infer this and propose these ideas and this was not denoted appropriate, which we have changed now.

Line 22: ANSWER: No, this terminology was not invented by the authors, but is often used in fracture mechanics - maybe more related to engineering problems. We decided to cite a text book here.

Page 354 Line 3: ANSWER: We wrote for the blue ones: Shear rifts mapped in the Landsat image from 1990 are displayed in blue colour. Those rifts moved with the ice flow downstream and experienced tensile stress, which is verified by their new shape in form of a wing crack. The green ones cannot be assigned to a shear mode crack, as we see no link to a shear stresses acting parallel to the plane of the crack. As it is unlikely to be a tear mode crack, we infer that they are mode I, tensile mode, cracks.

Line 11: ANSWER: 'contain' has been changed to 'limit'.

Line 15, ANSWER: The description of the feature H has been revised according to the reviewer's suggestion to focus more on how it was maintained. We have revised Figure 6 and provide now two subset images before and after the June/July break-up as these points out the H-crack location, as well as the effect of the grounded areas.

Page 355 Line 1-4: ANSWER: The dark shades in this original image are related to flow units that join. That dark shades of thin kind refer to shear margins, or zones of

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Discussion Paper



different rheology has been shown by Ala Khanzendar for the Larsen B ice shelf prior to its break-up. As we zoom now more into the area of the open ocean, this information will not be visible in the revised figure. The reason why it is maintained at the same location is discussed in the revised paragraph.

Line 5 -16: ANSWER: This paragraph has been completely revised due to the replacement of Figure 6.

Line 22: ANSWER: We agree that this figure is overcrowded with information. The colour bar in the inset is in fact the distance along track and the second inset, the lower one, shows only a part of the track. As the reviewers recommended to tighten up the manuscript we decided to exclude this figure and the section discussing the along track profile.

Page 356 Line 2-4: ANSWER: We used the ICESat data without tidal correction (yes, they are also provided with tidal correction). In general the residual is composed of everything contributing to dh/dt according from mass balance, the tides and is also influenced by hinge zones close to grounding lines. However, it is true, we do not have evidence that the residual here results from banded vertical motion. This section is however entirely eliminated in the revised version.

Page 357 Lines 7-11: ANSWER: This is correct. As we deleted the respective figure, the section including this sentence has been anyway deleted and does not appear anymore in the revised version.

Page 358 Line 10: ANSWER: These are ice rises. We just classify Burgess as an ice rumple.

Line 14: ANSWER: In a double-differenced interferogram, the grounding zone is characterized by a dense sequence of interferometric fringes where the vertical deflection due to tides occurs. As we use not a standard terminology, we rephrased this paragraph in order to express more clearly what we refer to. Changed to: 'At these

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locations, double-differenced interferograms (not shown here) do not show a typical pattern of tidal deflection normally apparent by a dense sequence of interferometric fringes. Hence, this indicates that no hinge zone is developed here.'

Page 359 Lines 9: ANSWER: It is interesting to note that the referee has observed similar rift propagation at other occasions. The authors would be very interested to know to which ice shelves the referee refers to.

Page 361 Line 12: ANSWER: Yes, we agree - 'disturbing' has been replaced by 'great'.

Line 13: ANSWER: It has been removed.

Line 21: ANSWER: We apologize for having chosen a term that might have been translated from German to English too bluntly. We refer to a cascade of events happening. Break-up #1 happens, failure zones start to develop. Break-up #2 happens, failure zones extend and new ones form and so on. Once the break-up will happen for the first time along the failure zones formed by former break-ups. This has happened on WIS in July 08. We hope (and ask the editor for advice) that the removal of sequence is appropriate.

Line 25 - 16: ANSWER: We do not intend to dissuade from the thought that there is interaction between melt and fracture - we just believe that it is not the exclusive reason for break-up events and ice shelf disintegration. In this paragraph we refer to melt PONDS only. Melt ponds that flood rifts are however not detectable on WIS, which is the reason why we infer that 'the existence and formation of melt ponds is not responsible for the changes and even more, that melt-pond drainage into crevasses played no role in all break-up events on WIS'. As we know from Vaughan et al. 1993 that the RES signals were captured in brine and that a drill hit liquid water at 5.5m depth, there would be anyway much liquid available in the porous ice matrix without melt, which could have filled crevasses (and caused them to expand throughout the entire ice thickness). In order to prevent that readers may infer that melt water is entirely not connected to the break-ups, we include a sentence here saying: However,

Full Screen / Esc

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Interactive Discussion

Discussion Paper



it is not excluded that melt water has an influence different from this one.

Page 364 Lines 1-13: ANSWER: It would be highly speculative to give any date on future events as long as the processes leading to break-up are not fully understood. As we could see WIS was rather stable for 10 years after the 1998 break-up and then suddenly several events happened.

Lines 13-16: ANSWER: We have deleted the sentence.

Page 365 Line 2: ANSWER: The loss of the projected 2100km² is supposed to happen soon - has partly happened already in June/July. If this forms a stable ice front can only be accessed by stress estimations from numerical simulations.

Lines 12-14: ANSWER: We have integrated the referee's wording suggestion.

Lines 15-18: ANSWER: Yes, although we have mentioned the viscosity in line 16 as well, our sentence does not reflect that these are competing scenarios. It is changed to 'This is of particular relevance, since the WIS consists of a bulk mass of warm ice, where decreased fracture toughness and increased liquidity compete.'

Interactive comment on The Cryosphere Discuss., 2, 341, 2008.

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