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The Cryosphere
Discussions

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Interactive
Comment

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

Anonymous Referee #2

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General comments

This paper reviews recent changes on the Wilkins Ice Shelf on the Antarctic Peninsula. It does this by reference to published material, satellite imagery and laser satellite altimetry. The aim of the paper is to make inferences about the likely processes that have led to recent phases of retreat in along the northern ice front. While presenting a wealth of new, and detailed observations, for me, the paper achieves rather little new clarity on its central issue, which is, what processes have caused recent change and what can we learn that will help us predict the future behaviour of this and other ice shelves.

The authors rightly observe that Wilkins Ice Shelf may be unusual compared to other retreating ice shelves in several respects and thus its behaviour might not be typical. The authors set about looking at many sources of data, and have incorporated many lines of reasoning in their paper. This has resulted in an extremely complex paper, in which several highly significant (and potentially contentious) conclusions are embedded in the text, and then not highlighted again in the conclusions. Some of these conclusion are not argued very thoroughly, and could be challenged. I have huge sympathy, with the authors, who have taken on a very large task, and tried to be comprehensive in their consideration of the available data but the paper that has emerged is tortuous and difficult to follow.

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" and "distintegration".

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

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need to be discarded.

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.

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Line 3: I don't think it is true that seven ice shelves have "disintegrated" on AP between 1995 and 2002. Seven ice shelves may have "retreated" over this period, but I don't think they have all "disintegrated" - which, to me, implies rapid and almost complete loss.

Line 4: hereafter replace "data set*" with "dataset*"

Line 8: replace "a bonding of the ice shelf to", with "a part of the ice shelf that connected"

Line 19 -20 It seems perverse to use a percentage area in one line and an area in km2 in the following one. This could usefully be rationalised.

Line 26: P&V showed acceleration not of the "glacier tongues" but of the "glaciers" themselves.

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Overall, the introduction is generally just a long list of previous observations with virtually no synthesis or judgement 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.

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Line 1: The mixing of terms like "break-up" and "distintegration", which I assume are intended to mean exactly the same thing is not helpful. Either it should be stated that these terms are intended to be synonyms or, better still, one term should be used.

Line 2: It is simply not true that all of these ice shelves has completely disintegrated. There is still some significant floating ice for Wordie, Mueller, Prince Gustav Channel ice shelves, Larsen A.

Line 13 the reference to Alaskan glaciers only applies to P&V, 2007; the other references do not mention it. And actually, I don't think the references Wingham et al., 2006; Shepherd and Wingham, 2007 which rely on satellite radar altimetry, include statements about the northern Antarctic Peninsula (i.e. north of Alexander Island).

Lines 19-21 "Recent estimates of the contribution of the Antarctic Peninsula glaciers to sea level rise by melt water indicate the impact of two factors: warming rates as well as prolonged melting season" - I don't understand this sentence. Surely, the two factors that increase the sea-level rise contribution, are increased surface melting leading to greater runoff, accelerating glaciers (due to loss of ice shelves and the effects described by P&V).

Line 22 -25: It appears that the authors are simply giving two opposing pieces of evidence (based on similar data) but do not give the reader a resolution, or even a comment on the disagreement.

Line 26: I don't think that there is any real reason to link changes in water temperature on the west coast of the AP with the thinning reported by Shepherd on the east coast. Especially, when no mention is made of the paper by Nicholls et al. (Nicholls et al., 2004) which noted no change much closer to Larsen C.

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Lines 5-10: The statements as they stand suggest that Glasser and Scambos, 2008; Vieli et al., 2007 all suggest that structural discontinuities and rheological criteria are

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responsible for ice shelf break-up. Firstly, I don't think that these papers suggested that atmospheric warming was not a factor in beginning the process. Secondly, I think that several other papers have also suggested structural processes that could have aided breakup (Doake and Vaughan, 1991; Doake et al., 1998; MacAyeal et al., 2003).

Line 8: it might be a small point but the -9 C mean annual isotherm, was only suggested as being an approximation to the real limit of viability, which is most likely related to the summer temperature, and the production of significant volumes of melt water.

Line 22: I don't entirely understand this sentence, "respond" to what?

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Line 4: Since Wilkins is changing in size this statement (and other reference to size) should have a date attached.

Line 5: Where does the statement concerning Lewis Snowfield come from. I would have thought that most of the ice in Wilkins Ice Shelf fell on Wilkins Ice Shelf not on the glaciers that feed it.

Line 12-13: "This coincides with jumps of ice shelf elevation at the junction between the two inflowing ice masses in this area." - I can't reconcile this statement with the diagrams. I don't see two distinct inflowing ice masses - I'm not saying this is wrong, it just needs a clearer description.

Line 18-26. As noted in the figure caption all the thickness data in the BEDMAP data were actually rather old, and available to the Vaughan et al. 1993 paper; and since the interpretation that sea water (brine) infiltration was the reason that no return signal was captured by radar was the main conclusion of that paper, it seems a little harsh not to cite it here.

Line 27 - 4: Having already mentioned that the Torinesi et al. and Tedesco et al. papers do not agree in their analysis of satellite microwave data, another microwave analysis is (Ridley) is introduced here, again without any comment about the apparent disagree-

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ment. What is a reader supposed to think from all this contradictory assessments? I would like to see more critical analysis.

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Line 20: In a section entitled "Previous knowledge on Wilkins Ice Shelf", I'm surprised that the authors did not consider the Scambos et al (2003) paper worthy of more discussion. There was a great deal of insight in that work.

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Line 7: I assume that this refers to the "Landsat dataset" that the authors have acquired not to the entire Landsat dataset available.

Line 14: I don't understand "respective" in this context.

Line 20-28: would it be easier to list the reference numbers, satellites and acquisition dates for the images concerned.

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Lines 14-16: Was there really no alternative to using the ancient J8 ice velocity to begin the phase-unwrapping procedure? I realise that the tidal flexing probably means that using areas of stagnant ice and rock outcrop is not possible, but are there no more recent tracking velocities available. As the authors note in following lines, this makes the absolute magnitudes of the velocities almost worthless, although does not damage the directional info.

Line 26: I wouldn't really get worked up over this one, but although GF Haendel was a German composer, I believe that the naming of the geographic feature was done in English as Handel Ice Piedmont, and so I think it should be using the anglicised spelling.

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Line 11: the parenthesised note, should still be grammatical.

Section 3.3.

I note the online comments from others on this section of the paper, and the difficulties they described, especially with repect to geoid corrections, etc. I will not repeat those comments here, but note that the tidal correction of satellite altimetry data should be reliable.

It seems likely that a comparison of ice surface elevation (and by implication, ice thickness) could now be attempted between the Geosat Geodetic Mission data and the ICESAT data, it would be great to see this done.

Section 3.4

I think that a discussion of ellipsoid / geoid conversion is definitely required here. I don't think that this can be glossed over.

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Line 8: what do the authors mean by a "continuous record"?

Line 8: it is probably worth noting that prior to 1990, the northern ice front appears to have been very stable since the first exploration (was that by Charcot on the Belgica?). Data from Corona images in 1960s, and early Landsat in 1970s show the icefront. Scambos, I think noted these points (Scambos et al., 2000).

Line 25-27: This seems to be a very strong statement about the general behaviour of WIS. I suggest that it should be moderated. E.g. "WIS is apparently an ice shelf that produced icebergs at a comparatively low rate under normal circumstances, but has experienced several break-up events, during which iceberg production increased enormously."

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Line 14-16 "The structure of these features indicates that dust blown-out by predominant northerly winds from the rocks of Rothschild Island may alter surface albedo and hence lead to this persistent pattern" - this statement appears without any justification or other comment. I personally do not believe it is justified, and I doubt that it is true.

Line 19-5: It is not clear from the text why the authors find the areas of dolines so "peculiar", nor whether they are significant. They compare the numbers of dolines found in 1990, with the number found in 2004-06, but do not say whether the difference is significant.

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Line4 -8: The area of open water close to Dorsey Island was noted by Swithinbank, 1988; is there anything new to say?

Line 8-18: I don't understand the reason for describing the area downstream of the ice rumple as "plastic". Plastic implies a very specific rheology (or at an approximation to it). How can this be determined simply from the imagery? I can see some correspondence between the downstream "wake" from Burgess ice Rise/rumple, and the kinks in the ice front. Could these not be due to an area of thin ice, rather than ice with an altered rheology?

Line 22: I am unclear if the designation of "mode 1" to longitudinal cracks, and "mode 2" to shear cracks are designations invented by the authors, or one derived from some previous work with which I'm not familiar. The former probably requires a better description of why this designation needs to be made, and the latter requires a reference.

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Line 3: I don't understand why the authors identify in Figure 6b, the rifts in blue as "shear rifts", and the ones in green as "tensile" ones

Line 11: I think the use of "contain" here is ambiguous, I think the authors mean "limit" rather than "include".

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Line 15, I believe that the "melt pool", which the authors suggest is a actually a "hole" in the ice shelf, was correctly identified a "hole" by Swithinbank, 1988; Vaughan et al. noted that this was the lowest point on the ice shelf and also suggested it was a hole. Bearing this in mind, does not preclude further discussion of this unusual feature in the current paper, but as a reader, I would like to see that discussion moved forward. The extraordinary thing to my mind is that the feature has remained broadly the same shape at least since the 1970s. Given this longevity it makes more sense to be to discuss this feature in terms of a "hole" rather than a "crack" - it is clearly something rather different to normal crevasses or rifts. (My opinion is that given that this feature has existed for around 30 years, the process of how it formed it probably unimportant, the real question is, how was it maintained?)

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Line 1-4: Maybe it's the quality of the images presented in the figures, but I don't see any evidence to accept that the "dark elongated shades" are shear margins. And thus, I don't think I accept that the hole is caused by the joining of the shear margins. This feature has remained roughly in the same location and has maintained a similar shape for more than 30 years, it really doesn't sound like a dynamic feature.

Line 5 -16: I cannot follow much of the argument contained in this section, I assume it relates to Figure 6a, but exactly which features, I am not sure.

Line 22: I find it difficult to see which part of the ICESAT track is being analysed. It really does not look like the whole of the track drawn in the inset. I think that the colourbar in the inset refers to the distance along-track in the graphs - it would be easier if the figure caption told us this.

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Line 2-4: I don't see any justification for the statement that the differences recorded in the ICESAT profile are not a "pure tidal motion". I believe that the ICESAT data actually have a tidal correction included in them, and so the residual should be composed of the difference between the predicted tides and the real tides, difference due to across-track differences, and changes in the ice shelf thickness between passes.

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Lines 7-11: I think that without any examination of the winds, tides or tsunami it is quite unsupportable to say that the rift propagation seen here supports (or contradicts) the Bassis study.

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Line 10: The authors have already noted that most of the named "ice rises" on Wilkins Ice Shelf are misnamed, and should be termed "ice rumples". Here they suggest there are "172 ice rises". Are these ice rises or ice rumples?

Line 14: I don't recognise, or understand the term "fringe belt".

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Lines 9: The observation that rift propagation seems to be linked to nearby but not connected break-up is an interesting one, and although not fully developed here does seem to make sense with what we've seen during other collapses.

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Line 12: Why "disturbing"? No one is suggesting that Wilkins loss actually has a harmful effect on human, ecological or physical conditions. The authors should either justify why they are concerned, or omit such value judgements entirely.

Line 13: Why "Alarming"? - As above.

Line 21: the phrase "sequence cascade" is used many times, it is not one that I recognise. Does it denote some particular type of behaviour that has been observed elsewhere and can be cited?

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Line 25 - 16: I agree with most of the statements that are made in this section, although, I personally am not ready to give up on the possible interaction of melt and fracture. The assertion that melt-pond drainage into crevasses played no role in all break-up events on WIS", is a strong one and seem only to be based on the melt-ponds area (not volume) being generally constant for a couple of decades. Given that Scambos could tie some break-ups to periods high temperature, seems to make this still a strong contender.

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Lines 1-13: I largely agree with the authors assessment of the likely future changes, but I would actually challenge them to put general dates on their predictions - are they talking months, years, decades, or centuries.

Lines 13-16: I really don’t understand the sentences here.

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Line 2: Do you expect the loss soon, or in the coming decades?

Lines 12-14: this seems to be a very complex way of saying "the presence of ice rises in the ice shelf produce areas of weakness, along which failure can occur when break-up events begin".

Lines 15-18: The authors may be right that under a constant warm ice is more prone to fracturing, but it might also be possible that under a constant strainrate warm ice is less viscous and so can deform more quickly, avoiding fracture. (I don’t know the answer, but I think expressing some uncertainty here might be appropriate)

REFERENCES

Doake, C. S. M., Corr, H. F. J., Rott, H., Skvarca, P., and Young, N. W.: 1998, ’Breakup and conditions for stability of the northern Larsen Ice Shelf, Antarctica’, Nature 391, 778-780.

Doake, C. S. M., and Vaughan, D. G.: 1991, 'Rapid disintegration of Wordie Ice Shelf in response to atmospheric warming', Nature 350, 328-330.

MacAyeal, D. R., Scambos, T. A., Hulbe, C. L., and Fahnestock, M. A.: 2003, 'Catastrophic ice-shelf break-up by an ice-shelf-fragment- capsize mechanism', J. Glaciol. 49, 22-36.

Nicholls, K. W., Pudsey, C. J., and Morris, P.: 2004, 'Summertime water masses off the northern Larsen C Ice Shelf, Antarctica', Geophys. Res. Let. 31, doi:10.1029/2004GL019924.

Scambos, T. A., Hulbe, C., Fahnestock, M., and Bohlander, J.: 2000, 'The link between climate warming and break-up of ice shelves in the Antarctic Peninsula', J. Glaciol. 46, 516-530.

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