

Interactive comment on “Dynamic changes on Wilkins Ice Shelf during the 2006–2009 retreat derived from satellite observations” by Melanie Rankl et al.

RD Drews (Referee)

rdrews@benicetoice.eu

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Summary

Ice shelves buttress ice flowing off the Antarctic continent, and ice-shelf disintegration causes a rapid increase in ice discharge. To predict the future of ice shelves, it is important to derive metrics assessing ice-shelf stability from observations. The Wilkins Ice Shelf (Antarctic Peninsula) has shown significant dynamic changes over the last decades, including thinning and frontal retreat, making it a suitable test case for previously published metrics such as the 'compressive arch' (Doake et al. 1998) or the 'stress-flow angles' (Kulesa et al., 2014).

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With this motivation in mind, Rankl et al. present 8 time-slices of surface velocities for the Wilkins Ice Shelf starting in 1994 and ending in 2010. The velocities were derived using intensity/speckle tracking on scenes from various radar satellite sensors. Based on the surface velocities, they derive strain rate fields and the corresponding magnitude/direction of the principal strain rates. Using a simplified rheology (which assumes a spatially/temporally constant rate factor), they also derive the corresponding stress fields with the direction/magnitude of the principal stresses.

The surface velocities quantify temporal changes, in particular an acceleration in 2008 as response to a partial disintegration of an ice bridge between Latady and Charcot Island. Crevasse formation and frontal retreat are tracked in the underlying backscatter images. The derived strain rate and stress fields provide temporally resolved data for testing ice-shelf stability criteria. The authors find that the first principle strain rate (direction of maximum extension) describes observed crevasse opening and the corresponding principal stresses give some indication at which threshold this may occur (subject to the simplified rheology). Change of sign in the second principal stress are independent of the rate factor and help during the interpretation. In particular, positive second principal stresses (marking a purely extensive flow regime) seem to mark areas which are prone to disintegrate. Other measures such as the second strain-rate invariant of the stress-flow angles remain ambiguous.

General Impression

The time series of surface velocities provide a strong observational dataset quantifying the dynamic changes of the Wilkins Ice Shelf in the past. I appreciate the derivation of strain rates and stresses (together with their principal components) to understand the underlying mechanisms of ice-shelf stability which is a timely and important topic. Overall I enjoyed reading this manuscript and from my point of view, this paper will fulfill the standards of The Cryosphere. Below I do suggest some revisions which should be addressed and which hopefully will help to improve the final version of the paper. I hope the authors persevere to go through my comments, and to turn this already well-

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developed TCD paper into a nice TC publication.

Reinhard Drews

General Comments

1. The paper is in many places unnecessarily descriptive where it could be rigorous. This is manifested in expressions such as "some of the measures [...] emerge to be more or less applicable" p.7/l.29 (is it applicable or not, and why?), "..acceleration is somewhat lower" (how much?), "..showed very small velocities in March.." (how slow and slow compared to what?). Although the individual examples are all minor, the repeated occurrence of these type of descriptions makes the paper imprecise and speculative in places. Below I mention a detailed list and suggest improvements.

2. Error estimates of the velocities are derived in the supplements, but I wonder if these errors are complete. From my experience with intensity tracking, it is (at least sometimes) required to calibrate the data (e.g. offset it at rock outcrops) and even the calibrated fields may show non-zero, spatially varying values in the difference fields of mosaics (due to errors in coregistration, orbital information, atmospheric contribution,...). In Fig. 2b,c,d,e cutting edges are visible, but the authors do not report their magnitude and attribute them to monthly flow variations. With the information available in the paper, this appears overinterpreted and should be better justified (how were the data calibrated, what is the magnitude of the cutting edges and what other evidence exists for monthly flow variations?). Along those lines, I suggest to show the full difference field and not restrict it to the western part only.

3. The wave-like pattern which appears in the strain rates (e.g. Fig. 2 c,k,o) and elsewhere is quite prominent but is only briefly mentioned in an isolated sentence (p. 6/l. 12) when describing the stresses. This must be mentioned earlier (e.g. section 3.1) and the explanation should be expanded. How is it possible that the tracking algorithm creates such a pattern, and if so, why only in the scenes from 2008 and 2009? Ionospheric path delays are possible but may not be the only option. Somewhat

immodestly I refer to Drews et al. 2009 (IEEE Geosc. Rem. Sens. Fig. 4) where a similar wave-like pattern has been linked to variations in tropospheric water vapor content. However, this was using differential SAR interferometry and I don't know how this would appear in flow fields based on intensity/speckle tracking.

4. I have no prior experience in applying the ice-shelf stability criteria mentioned in the manuscript and I enjoyed learning more about them. However, in the end I was left with a foggy impression which criteria are applicable where and under which conditions. A good example for this confusion is the paragraph p.7/l. 38f stating that the authors "want to follow Doak et al. (1998)" in saying that "when the ice front retreats beyond a certain isoline [in the second principal strain rates] destabilization is expected". However, two sentences later it is stated that the observed "second principal strain rates are [...] insensitive during the retreat of WIS" which makes me think that the Doak-criteria does not fit because WIS has retreated. However, the authors go on stating that the "negative values suggest general recession" which again is what has been observed. I am sure that I misunderstand something here, but even after reading this paragraph multiple times I am still left in the dark whether or not the second principal strain rates are a good or a bad metric for assessing the stability in this case. Similar ambiguities also occur at other places mentioned below. Summarizing, I am not questioning the analysis, but I suggest that the authors put more effort into clearly stating their findings which are currently partially hidden. This also includes sharpening the conclusions. Detailed comments are mentioned below.

Specific Comments

p1 | 12 how about "A total area of $2135 \pm 75 \text{ km}^2$ (XX percent of the total ice shelf area)

p1 | 12 "multi-temporal": how about "in order to derive temporal variability of surface flow

p1 | 14 "which were forwarded" –> "which were forwarded previously"

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p1 | 21 "pins down" → "defines"

p1 | 22 remove "general"

p1 | 23 ice-shelf "disintegration" "collapse" "break-up" are used interchangeably. Maybe better stick to one term if you mean the same thing to avoid confusion for the reader. Same for ice-front "retreat" "recession"

p1 | 26 I have difficulties imagining a tongues of a tributary glacier feeding into an ice shelf. Can you clarify?

p1 | 33 I supposed "within only one week" refers to the time period of ice-shelf disintegration. It could also mean that the criterion was published on week after the breakup. Rephrase.

p2 | 9: "under" → "during"

p2 | 26: "multi-temporal" → "time series"

p2 | 35: I suggest to mention also normalized values of the area (e.g. relative to the entire ice-shelf extent in year XX), because it is hard to get a feel for these numbers otherwise.

p2 | 38: Surface melt ponding may still play a role in ice-shelf disintegration. Stick a "exclusively" in there or restrict this sentence to the WIS. Otherwise you neglect the role of surface-melt ponding in general, which I think is not what you intend.

p3 | 19: Compared to which reference direction are flow vectors filtered? Where does this direction come from?

p3 | 16: specify what you mean with "relative orbits" as opposed to just "orbits".

p3 | 21f: I get the feeling that "relative" refers to uncalibrated data where as "absolute" refers to calibrated data. Please specify how the calibration was done (on rock outcrops?) and be more clear about using the words relative/absolute

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p3 | 5 : What is the final gridding of the velocities and did you apply any calibration?

p3 | 25: I am not yet convinced that the deviations in the overlapping parts of the mosaicked surface flow are solely due to monthly ice-flow variations. Are there any independent data showing that ice-flow varies on monthly time scales and by how much? How can you be sure that the observed offsets are not due to the processing artefacts (i.e. unresolved coregistration offsets, imprecise orbital information or maybe even atmospheric contributions)? I think this point should be discussed in more detail, because the difference field is a major result of this paper. Currently I have the feeling that the overall data accuracy of the velocities is overestimated (although Fig. 2g is and will remain significant). I am happy to be shown otherwise.

p3 | 25: Given that TC has no rigid page limit, I don't see the advantage of having the error analysis (which I find important) in the supplements.

p4 | 11: Pattyn 2003 is a paper about including higher-order mechanics in ice-flow models. However, the use of deviatoric stresses is more general and already occurs in the shallow ice approximation. Therefore, I find this reference a little misleading and suggest a textbook reference (Greve Blatter, Cuffey Patterson,...)

p4 | 12 "data are acquired" (data are plural)

p4 | 20 remove "In addition,"

p4 | 20 "necessarily linear" → "is non-linear" (I consider this as the default in glaciology. The "not necessarily linear" somehow implies otherwise.)

p4 | 25 Remove "generally". The rate factor always depends on temperature.

p4 | 30 Not sure if it is fully correct to state the "stress tensor" is symmetric by construction. The symmetry reflects conservation of angular momentum. I don't want to be witty here, just point out small imprecise language. Maybe rephrase.

p4 | 34 E-5 (also at other places) is not TC style

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p5 | 5f: I am more used to have velocities in meters per year but certainly don't insist on it.

p5 | 11: Quantify "very small" because velocities are a main result of this paper there is no need to be descriptive here. Same holds for terms like "slightly higher" etc.

p5 | 13 "large double fracture" how large?

p5 | 17 "the consecutive acceleration was most expressed" how about "the consecutive acceleration was largest at.."

p5 | 18 "0.8 meters per day" is a velocity, not an acceleration. Do you mean velocity increase? Again, mentioning relative values (e.g. and XX percent increase) would help to better grasp the meaning of these numbers.

p5 | 19 terms like "somewhat lower" are unnecessarily descriptive for primary results. Same for "rather stagnant".

p5 | 26: How about: "We calculate strain rates at each point in a local coordinate system with the two axis aligned parallel and perpendicular to the local flow direction."

p5 | 26: Mention and check that the invariants (principal strain rates etc.) are indeed invariant compared to strain rates which are calculated in a global coordinate system.

p5 | 30: Which threshold was chosen and why? Fig. 3 mentions 0.05 m/d.

p5 | 33: "slight inflow"?

p6 | 2: a "certain direction" is imprecise. It is along the eigenvector corresponding to the eigenvalue.

p6 | 2: As a non-sailor I constantly mix up "lee" and "luv". Also people may understand this term from a perspective of atmospheric circulation. I think it is better to talk about "upstream" and "downstream" in these cases (and elsewhere).

p6 | 6: quantify "less negative"

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p6 | 14: This must be mentioned earlier and discussed in more detail (see general comment above).

p6 | 33 remove "more or less"

p6 | 35 when interpreting strain fields for fracture formation it is important to explicitly state the resolution of velocities and strain rates.

p7 | 16 "Even if zero is not passed, "-> "Even if values remain negative, ..."

p7 | 29 "more or less" is not helpful here. Better write a clear sentence stating that no single stress/strain field can fully explain the observations (or something like that).

p7 | 30 I suggest avoiding "might be" (and "could be" later on) and instead clearly stating the uncertainties that are indicated with these formulations.

p7 | 36 I do not grasp the meaning of this paragraph and suggest rephrasing (see general comment above).

p7 | 38 "certain" isoline? Is it not the 0 isoline specifying pure extension in all directions (as stated in intro)?

p7 | 38 "retreats/recedes" what is the difference between retreating and receding and why are both verbs mentioned here?

p8 | 9 This paragraph of fractures fits to the one started in p7 | 29. I also suggest to move the last paragraph of the conclusions to this section to have all fracture-related things together.

p 8 | 29: This paragraph brings up a observations which have not been discussed previously in the paper. I suggest not to introduce novel things in the conclusions (see previous comment).

p8 | 16 High variability or high uncertain (because flow direction is ill-constrained)?

Figure 1

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I don't see the red nor the cyan line. On the other hand I see an orange ice-front position not mentioned in legend. Something went wrong here.

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