Earth Surf. Dynam. Discuss., 2, C494–C500, 2014
www.earth-surf-dynam-discuss.net/2/C494/2014/

Earth Surface Dynamics Discussions

Interactive comment on "Are longitudinal ice-surface structures on the Antarctic Ice Sheet indicators of long-term ice-flow configuration?" *by* N. F. Glasser et al.

Anonymous Referee #2

Received and published: 13 November 2014

The authors present an interesting discussion of longitudinal ice-surface structures, and their mapping across Antarctica is a new contribution – this mapping facilitates new analyses that lead to their conclusions about ice-flow configuration. While the discussion is interesting and some arguments are compelling, based on the information provided in the manuscript I was not convinced that they have answered their title question: "Are longitudinal ice-surface structures on the Antarctic Ice Sheet indicators of long-term ice-flow configuration?" – more information is necessary to evaluate whether or not this is a question that can even be answered. The manuscript did not provide many details of the methods on which the conclusions are based, and in general did not present a clear case to address the title question. I hope that my comments

help to improve the manuscript so that other readers can better understand how the authors have addressed this important title question, what the take away messages are from this work and where to go next.

Major comments:

1. Since the main conclusion (hypothesis?) depends on the "ice-residence time" calculation, a much better justification of why this calculation is valid and holds over long timescales needs to be given. Some major comments:

- Why are the calculations that you made stated as minimum estimates – variations in ice flow speed over time will alter the time it takes for ice originating in the interior to reach the margin, and this uncertainty will likely be larger than uncertainties in where the paths of ice started.

- This calculation assumes that the ice sheet is in steady state – why can you then use the timescales calculated to support your case for stable ice flow?

- And, the modern velocity field isn't in steady state so using only one compilation is also an assumption. How sensitive are the residence times for the four chosen glaciers on surface velocity within range of modern behavior?

- Why are these four glaciers representative of the entire ice sheet(s)?

- The term "ice-sheet residence time" (and more often used "ice-residence time") does not seem like the best term for what you have calculated. It does not at all get across the idea of ice flowing on a specific path from inland to the margin. Has this term been used elsewhere? I suggest using a descriptive statement instead of a catch phrase.

- Page 921, Lines 11-: More discussion of uncertainties in the modeling and evidence from blue-ice areas needs to be given before these statements may meaningfully relate to your points. Also, since you cannot determine anything about the interior from the ice-surface structures why are these points given to strengthen your argument for a stable ice-flow configuration? No discussion of coupling between margin changes and

interior changes is given, and I think that this is something that is not well understood (and likely differs greatly depending on margin conditions and distance to the interior).

There seems to me to be two different conclusions wrapped into one. This statement needs to be better supported (based on concerns above): "...then it is possible that ice velocities have been stable both within flow units and between adjacent (tributary-trunk) flow units, over centennial to millennial timescales". But, why do ice-surface structures inform about flow configuration and flow velocity? Couldn't the locations of outlet glaciers and ice streams remain largely the same while the velocities of individual glaciers changed in the past thousands to tens of thousands of years? This is currently discussed only in the context of modern changes, but past change is surely important.

Page 921, Line 18: This statement about "Earth surface processes" seems too broad. While the statements are true, it seems too strong to extend the conclusions in this way. This is a good motivation, but I don't think the conclusions based on analysis of four select Antarctic outlet glaciers warrant this as the final statement in the paper.

2. Are these ice-surface structures across ice streams and outlet glaciers really tracers? The situation with flow features in the ice shelf, especially streaklines as in the Ross Ice Shelf, is different because the flow of the shelf must adjust to volume flux of incoming ice and the streaklines originate in the grounded ice sheet and get advected downstream, then they are "locked in" as passive tracers in the ice shelf. The ice-surface structures mapped here may have different possible formation mechanisms and can be altered anywhere along the length of the structure by ice-sheet flow. This is not discussed in the text. Why would the ice-surface structures record events that occurred anywhere along the path and at all times? For example, if there was a transition from one flow state to another, and then back to the original state, for how long would the ice-surface structure have an imprint of that change? The features can likely respond quickly in fast-flow regions where they are located. It seems like some analysis of the internal structure is necessary. Why were radar-observed internal structures not considered here? Internal layers are a challenge to interpret, but their value (and

necessity) related to this problem needs to be addressed.

Why can't the observation that there are few regions (maybe only two) where longitudinal ice-surface structures were not aligned parallel to flow velocity be interpreted that ice-surface structures only record changes for a few hundred years?

Assuming that ice-surface structures are long-term tracers, if they can be formed in multiple ways then why can the areas near Kamb Ice Stream and near Ronne Ice Shelf – Thiel trough be used to establish an expectation about structure in all other regions? Have structures in all other regions really been stable compared to these two areas, or have they just evolved differently? Again, it seems that the conclusion that all other regions have been stable would be a surprise so this needs to be much better justified.

3. I think that the strong conclusion, "... we infer that the major ice-flow and ice-velocity configuration of the ice sheet may have remained largely unchanged for several thousand years, and possibly even since the end of the last glacial cycle" needs to be better qualified. This conclusion is based on ice-residence time estimates (see points above) that give a range of residence times from a few thousand years for West Antarctica to more than ten thousand years for East Antarctica. Conclusions including time information need to be given explicitly for East Antarctica and for West Antarctica; I do not understand why it is appropriate to state these conclusions generally for all of Antarctica. Regional evidence addressing ice-flow history on different timescales (as given in the Discussion) is difficult to bring together to make single statements about all of Antarctica. The final paragraph of the paper would be better as motivation for this work, and does not seem justified by the ice-structure mapping alone.

What are the next steps? The comprehensive map is available, initial analyses have been done using four outlet glaciers, and strong hypotheses have been presented. Should more glaciers be analyzed, or should these hypotheses be tested against models or radar data? What can the community do with understanding of the "basic ice-dynamical organisation" of the ice sheet when many studies are focused on regional

changes in the ice sheet over time, and especially since the last glaciation. Are the steps following on from this work the same for West and East Antarctica? If the focus is on continent-scale change, how do these conclusions mesh with sea-level records?

If they can be justified, I assume that the conclusions imply that if the overall ice-flow configuration has not changed then the controls on fast ice flow have not changed much either. Since there are many different controls on flow speed, does this imply that the basal topography is most important to where ice flows fast and that other bed conditions control how fast the ice flows in these areas. As is, not enough discussion is given regarding the relationship between the locations of fast- flowing ice and the controls on fast-flowing ice.

4. While it is necessary to describe the different possible explanations for the formation of the ice-surface features, it was not clear if we expect that all of these mechanisms are (or could be) active or if it is an outstanding problem that we need to isolate one explanation. How does this state of the research affect your conclusions? It would help the reader to know if this work will support specific formation mechanism(s). When the issue of formation mechanism is discussed again in Section 4 (Discussion and Conclusions) it is not related back to the list given earlier in the Introduction. What is given in the Discussion seems like it should be mentioned in previous work, or that it is already given there. With regards to formation mechanisms across Antarctica, would we actually expect one mechanism to act over such different flow environments and spatial scales? Also, the reader should be told explicitly what is new from Glasser and Gudmundsson (2012) and what questions this work answers that could not be answered in the previous studies, presumably because the mapping was not available.

5. The methods section is very abbreviated. More detail should be given, addressing: What percentage of the mapped structures came from higher-resolution imagery? How did you account for different resolution of the imagery (assuming that it mattered, or if most of the data are lower resolution)? What resolution was necessary to confidently map these features? Also, was quantitative analysis done to compare the mapped ice-

surface structures to the measured modern velocity field? Since most of the results follow from this comparison it should be detailed how it was done, and if it was only qualitative (as in Figure 4 visual comparison) then does this affect any of the details of the interpretation? It would be really interesting to see this difference map, but I am not sure how it would be presented given that the ice-surface mapping is not a gridded product. Also, is the improvement from Glasser and Scambos (2008) due to improved resolution, greater satellite coverage, or both?

6. The authors should consider separating the Discussion and Conclusion sections. To me it seems that it would be better to provide specific take away messages backed up by results as given in the Results section, and not just open discussion. The organization is a bit confusing in that the calculations of residence time that are central to the conclusions are not given in the Results section.

Minor comments:

Page 912, Line 2: Need to provide acronym for Antarctic Ice Sheet (AIS)

Page 912, Line 10: I am not sure what is meant by “ice residence times”

Page 912, Line 13: Originating in the interior is an overstatement, or at least for East Antarctica. Some of these structures approach the divide in West Antarctica but in the description it might be worth stating more explicitly their extent (or referencing Figure 1)

Page 913, Line 20: I am not sure what you are referring to by “. . .are commonly developed parallel to the margins of individual ice-flow units. . .” – what is an ice-flow unit, an ice stream?

Page 913, Line 24: Is there a word missing: “. . .structure, longitudinal foliation. . .”

Page 914, Line 26: Only part of this sentence is italicized – why? Also, “vertical sheets of changed ice fabric” is hard to understand, more description could provided so that the reader cannot confuse what is meant here

Page 917, Line 9: What do you mean by "...not smoothly aligned parallel..."?

Page 919, Line 5: I am not sure what you are referring to with "downstream overprinting"

Page 919-920, Line 28-: Was the glaciology community unsure that the overall organization of Antarctica "...is one of rapid-flowing, warm-based ice streams, separated from slow-flowing frozen-bed zones by abrupt shear margins". This is a really general statement, applies much more to West Antarctica, and to me it isn't something new based on the ice-structure mapping. It is interesting to note, but stated using "confirm" makes it sound like we did not really know this until now.

Page 920, Line 27: The statement about recent and rapid changes in velocity and surface elevation for Antarctic outlet glaciers is very under-referenced. The reference given to Johnson et al. (2014) seems strange when referring to recent changes (presumed to be from the measurement era?). A more complete / appropriate list of references should be given.

Page 921, Line 12: Why is modeling of the central part of the ice sheet related to the conclusions here? The ice-surface structures are not mapped back to the divide and central interior, especially for East Antarctica.

Interactive comment on Earth Surf. Dynam. Discuss., 2, 911, 2014.