Response to reviewers for “Are longitudinal ice-surface structures on the Antarctic Ice Sheet indicators of long-term ice-flow configuration?” by N. F. Glasser et al.

We thank all three reviewers for their comments on the paper. Their comments have been very useful to us in revising the manuscript and making our message clearer to the readers.

Overall summary of changes made

In order to provide some sort of overview, we thought it worthwhile to provide an overall summary of the changes made to the paper in the light of the Reviewers’ comment. Here we identify and respond collectively to 12 key points made by the reviewers. More detailed responses are given later in the document.

1. Change the title of the paper from a question to a statement.

We originally thought it was a good idea to pose this as a question but can see that reviewers did not like this approach. We have rewritten the paper and given it a new title to avoid the 'question' approach. Hopefully this has helped to alleviate some of the criticisms about the title and our approach to answering that question. The new paper title is “Origin and dynamic significance of longitudinal structures (“flow stripes”) in the Antarctic Ice Sheet”.

2. Methods used in the mapping

We have added to the methods to include the information requested.

3. Focus of the paper

As per the comment above we have rewritten the paper to focus more on what the longitudinal features can tell us about the formation mechanism rather than the history of the ice sheet (although please note these two issues are inter-related to a certain extent). This is achieved via a hypothesis-testing approach, using the hypotheses listed in the Introduction, and moving to the evaluation of these in the “results” part. Please note that previous papers have looked at these features mainly on ice shelves. Our paper deals with the fast-flowing parts of the ice sheet (e.g. ice streams).

4. How do the longitudinal features form and how are they preserved down-ice?

Please see #3 above about the focus of the paper.

5. Add references to other studies including radar stratigraphic studies

Several of these references are cited already, but we have added the others to enhance this element of the discussion.

6. Need for more quantitative analysis

We agree that a more quantitative analysis is a good idea. To this end, we undertook an analysis of the entire population of mapped flow-stripe lengths and we now present their frequency distribution as histograms in a new Figure 6. We note that while “more quantitative analysis” is suggested, the reviewers did not provide any concrete suggestions for which aspects of our work they would like us to quantify. We hope that the addition of the frequency distribution histograms helps in this respect.

7. Ice-residence time calculations

We now explain how the four glaciers studied in detail are representative of the wider ice sheet. Although the original term “ice-residence time calculation” is perfectly self-explanatory we now explain these calculations in terms of parcels of ice where the term is first introduced in the Methods. In the Discussion, we have now also divided the glaciers into representative “steady-state” ice streams, and areas of perturbed ice-flow history to make this clearer.

8. Are the ice-surface structures across ice streams and outlet glaciers really tracers?

This is a very important question that requires us to discuss formation mechanisms (see above, where we are asked to sharpen the focus of the paper). It is also why the original paper was framed as a question (but see above, where we were asked to remove this ambiguity). It appears that we can't keep all three reviewers happy at once on this point because they all want a different focus in the paper. The key question is whether or not “flow-lines” are really indicators of flow direction. In the majority of cases, we believe that they are. The fact that these features persist through crevassed areas and through areas of surface ablation (blue-ice areas) demonstrates that they are a three-dimensional structure. We discuss this via the hypothesis-testing approach. We also added two new Figures to demonstrate this; one Figure showing the survival of these features through surface ablation on the Lambert Glacier and one Figure showing conceptually why this implies they are three-dimensional in nature.

9. The conclusion that the ice sheet may have remained largely unchanged for several thousand years, and possibly even since the end of the last glacial cycle.

The reviewers clearly did not like this conclusion, perhaps because we did not explain it clearly enough, so we have re-worded it and added a caveat. However, we still believe that the overall pattern of ice-sheet flow (i.e. major flow-path directions) has remained unchanged except for in a few areas e.g. Kamb Ice Stream.

10. What are the next steps?

We have added some suggestions here for the next steps related to testing the hypotheses. For example, further comparison of longitudinal structures and radar stratigraphic studies could be a fruitful area of investigation.

11. Consider separating the Discussion and Conclusion sections.

We have done this.

12. Minor comments.

We have attended to all of these suggestions.

Detailed response to Reviewers' comments

Please note that we have made a large number of changes to the original document. This means that some of the reviewers' comments no longer apply (we have noted this where it is the case). Our responses are in red text.

Anonymous Referee #1

This paper presents interesting observations of the distribution of longitudinal surface structures on Antarctica, provides some useful discussion of how these features might form and insight into the potential history of the Antarctic Ice Sheet. Thank you. The authors come to the significant conclusion that most of the Antarctic Ice Sheet has had broadly the same flow configuration since the end of the last glacial cycle. The main inadequacy in this paper is in the methods section, which requires detail in how the mapping was conducted (outlined below). We now explain these methods in more detail. Caveats related to these methods need to be discussed in the paper. Alternative interpretations of these features should be considered in the manuscript, and the implications these have for the interpretation of ice sheet history. We now do this via the hypothesis testing approach.

The focus of this paper is also confusing, and should be narrowed or defined more clearly. It is unclear whether the aim is to look at ice dynamics, how longitudinal surface structures form, the implications for long-term landscape evolution, or all of these. If the aim is to cover all these topics, the paper should be restructured accordingly, and contain a conclusion section which documents these topics separately to clarify the conclusions drawn in these areas. We have re-focused the manuscript. Overall, this is a worthwhile, well presented manuscript, but the following changes and points need to be addressed. Thank you. The mapping methods used by the authors of this paper are unclear, and poorly described. We now explain these methods in more detail.

Three sources of satellite images are used, all of which have different resolutions and record different spectral wavelengths. A comparison of the differences for mapping structures between these datasets does not occur, which is especially important considering the range of resolution between the MODIS imagery (150 m) and the Landsat imagery (15/30 m). The authors do not state in which regions the coarser imagery was used, where smaller features may be missed in the MODIS imagery or be amalgamated by the coarser resolution. We now explain these methods in more detail. Furthermore, whilst a commendable amount of mapping is shown, the claim of the authors that continent wide mapping (page 912, line 2; page 913, line 9) was achieved is unjustified. As Figure 1 shows, no mapping was done on the West-Antarctic Peninsula and surrounding regions. This has been presented elsewhere so we did not wish to repeat it here. Consideration of the implications of not studying the longitudinal surface structures here means (i.e. not in an ice stream context) should be made. Crucially to the interpretations drawn by paper, the authors map longitudinal surface structures as smooth continuous lines, in a fashion which may not represent these features correctly. The authors claim that longitudinal surface structures can be followed "without interruption folding or buckling over their entire length" (Page 920, Lines 21-22), meaning they can be traced along entire flow-lines. However, in many instances longitudinal surface structures are actually interrupted by other features on the ice surface (e.g. transverse waves or crevassing), and also appear to fade out when examined close up. Often, a similar feature will become visible downstream of an interrupted or faded longitudinal surface structure. If the authors interpolate across these interruptions in a sensible way, this may be a valid approach, but this is should be documented, perhaps with an additional figure, in the methods section and the caveats this has for the results presented in Figure 6 should be discussed. Caveat added. Beyond just the mapping, the interpretation and conclusions presented relies on longitudinal surface structures being created as "point-source" features at the start of the ice stream which then propagate down-stream, allowing the residence time of the ice to be calculated. This may be the case, but alterative interpretations should be considered. For instance, if flow structures are born everywhere on the ice stream, they may eventually propagate, collide and merge, allowing them to achieve their great length in a much shorter time-frame. It may be the case that these surface features are able to adapt rapidly to changes in ice sheet configuration, especially in fast flow regions as studied here. Furthermore, any potential thinning or thickening of the ice in these regions may not have an effect upon these surface morphological features. This is now addressed in more detail.

Therefore, further considerations are required within the manuscript. Although the authors recognise regions of palaeo ice configuration in their description (the Kamb and Institute regions), no consideration is given for previously identified palaeo ice regions of the Carlson Inlet (a deep subglacial trough where fast flow is not occurring; King, 2011) and the Siple Ice Stream (Catiania et al., 2012). In addition to the above, I have the following further points:

For context, reference should be made in the introduction to the geomorphological evidence that sectors of the Antarctic Ice Sheet were more extensive during the last glacial cycle. A useful summary of formation hypotheses for these features is given, but again it is unclear if and how the authors are testing these hypotheses. Page 916, line 1. The word magnitude implies that ice velocity can be calculated from these features, when they occur in a range of ice velocities. Page 916, line 18. "Dynamic" implies change, but the paper argues that the ice sheet configuration has remained stable. We also knew that rapid ice flow reached the interior of the ice sheet from the velocity measurements shown here. The subsequent observations (Page 917, line 26 to Page 918, line 7) are also shown in the velocity data. Comment no longer applies.

Page 920, line 2. What about topographic ice streams which are bounded by topography rather than slow flowing ice shear margins? This is now covered in the Introduction.

Page 921, lines 26-29. I disagree with this statement about subglacial bedforms. Crosscutting features have been inferred for a long time to demonstrate ice divide migration (Boulton and Clark, 1990) and can occur on flat areas without deep subglacial troughs (e.g. Ross et al., 2009). They can also form rapidly, during relatively short-lived events (e.g. the Dubawnt Lake Ice Stream (Stokes and Clark, 2003)) and land terminating ice streaming events which do not occur in Antarctica such as at the Southern Laurentide Lobes (e.g. Colgan and Mickelson, 1997). Statement removed.

References:
Boulton, G. S., and C. D. Clark. "A highly mobile Laurentide ice sheet revealed by satellite images of glacial lineations." Nature 346, no. 6287 (1990): 813-817. Statement removed.
Catania, G., Hulbe, C., Conway, H., Scambos, T. A., & Raymond, C. F. (2012). Variability in the mass flux of the Ross ice streams, West Antarctica, over the last millennium. Journal of Glaciology, 58(210), 741-752. Added
Colgan, P. M., & Mickelson, D. M. (1997). Genesis of streamlined landforms and flow history of the Green Bay Lobe, Wisconsin, USA. Sedimentary Geology, 111(1), 7-25. Statement removed.
King, E. C. (2011). Ice stream or not? Radio-echo sounding of Carlson Inlet, West Antarctica. The Cryosphere, 5(4), 907-916. Statement removed.
Ross, M., Campbell, J. E., Parent, M., &Adams, R. S. (2009). Palaeo-ice streams and the subglacial landscape mosaic of the North American mid-continental prairies. Boreas, 38(3), 421-439. Statement removed.

Stokes, C. R., & Clark, C. D. (2003). The Dubawnt Lake palaeo-ice stream: evidence for dynamic ice sheet behaviour on the Canadian Shield and insights regarding the controls on ice-stream location and vigour. Boreas, 32(1), 263-279. Statement removed.

Anonymous Referee #2

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. We have rewritten the paper and given it a new title to avoid the 'question' approach. 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. We now explain these methods in more detail. 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 message 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. Done.

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. Explained in text.
- 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? Removed.
- 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? Removed.
- Why are these four glaciers representative of the entire ice sheet(s)? We don't claim they are representative of the entire ice sheet.
- 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. We added 'parcels of ice' in conjunction with ice-residence time to explain this concept better.
- 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). We added two new Figures to address this.

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. We have removed this statement about "Earth surface processes".

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. This is now discussed in the text.

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? This is now explained in the text.

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? We didn't mean to imply this, so we have re-worded this section.

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. Again, we didn't mean to imply this, so we have re-worded this section.

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. We have toned this conclusion down. We also added a section on "Uncertainties and Further Work" where these types of issues are covered.

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. We added a section on "Uncertainties and Further Work" where these types of issues are covered. We have added some thoughts on the next steps.

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. Hopefully, the 'hypothesis testing' approach we now used has answered this comment.

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? More information has been added to the Methods.

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. Done.

Minor comments: All the following minor comments have been attended to

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 be 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.

H. Conway (Referee)
An interesting question – the authors make a case that the answer is YES. They argue that persistence of these features is evidence that, with a few exceptions, the dynamic configuration of the Antarctic Ice Sheet has remained unchanged for thousands of years, even back to the end of the last glacial maximum.

Of course exceptions are expected because there is good evidence that the West Antarctic Ice Sheet expanded during the LGM and has subsequently thinned and retreated dynamically (e.g. Stuiver et al., 1981; Whitehouse et al. 2012; RAISED Consortium, 2014). The pattern of streak lines in the Ross Ice Shelf have been used to infer century- to millennial-scale variations in the flow dynamics of not only Kamb Ice Stream but all of the Siple Coast and Gould Coast ice streams (Fahnestock et al. 2000; Hulbe and Fahnestock, 2004, 2007). Streaklines in the Ronne-Filchner Ice shelf have also been analyzed to infer histories of flow dynamics (e.g. Hulbe et al. 2010; Ross et al. 2010). The two exceptions are important. They contain histories of deglaciation of West Antarctica and together with those from the Antarctic Peninsula (not discussed in this paper), suggest the answer is NO. The goal of the paper is not clear and it is not at all clear that the question posed in the title has been answered. A more quantitative examination of the shape and continuity of streaklines is needed to address the question. We have rewritten the paper and given it a new title to avoid the 'question' approach.

Some detailed comments:
P912 line 18: What about work by Hulbe and Fahnestock, (2004, 2007); Ng and Conway, 2004; Siegert et al. 2013, which discuss century- to millennial-scale variations in ice-flow? Interpretations of thinning and divide migration from ice cores (e.g. Nereson et al. 1998; Conway et al. 1999; Waddington et al., 2005; Martin et al. 2006, 2009; Price et al 2007). Text deleted.
Page 913, lines 19 ff. The assessment of origin of streaklines does not need to be duplicated; it is repeated in the summary of Glasser and Gudmundsson, (2012) presented on p 914. Fixed.

P913 I do like Fig. 3, but I am not at all clear how it fits in this paper, unless the primary focus is the origin of flow stripes. The paper now discusses the origin of flow stripes in more detail so Figure 3 is retained.

P915 line 7: No mention of surface expression of subglacial channels such as those described by Le Brocq et al. (2013), Millgate et al, (2013). Both references added to the Introduction.

p. 916 line 1: How do these structures indicate the magnitude of ice-surface flow? Earlier the implication is that some (but not all) originate from basal sliding over a rough bed. And not all stripes are aligned with present-day flow direction. Many may represent past flow directions, and sub-surface channels are not always aligned with flow (Le Broq et al (2013). Added to Introduction.

3. Results p916 line13ff. Draws attention to comparisons of surface structures to SAR images, velocity maps, and subglacial topography (Fig. 4). The comparisons are not compelling- it is not possible to make quantitative comparisons from plots like these. The reader is asked to note co-location of streaklines with regions of fast flow and deep subglacial topography, but even a qualitative assessment is not possible. Some other metric is needed to make a convincing case. Also, most mapped streaklines are from the Ross Sea and the Ronne Filchner (Fig. 4b), which, as stated in the manuscript, are "exceptions" where streaklines do not follow present-day flow field and may not be continuous. The different data sets cannot be compared statistically so we have no choice other than to make this comparison visually.

p. 917 line 8 ff. As discussed above, the two regions (the Ross Sea and the Ronne Filchner) show evidence of major changes in flow dynamics. They cannot be dismissed. They are not dismissed.

P 917 line 29ff. Discussion of Fig. 5. Why would we logically expect rapid flow through the Thiel Trough? Presumably driving stresses are insufficient? If the focus of the manuscript is glacial history, it would be useful to constrain some point in the past (P918 line 9). This work has been reported and discussed by Siegert et al (2013), who also placed a constraint on the timing of the switch in flow. Text amended to explain this.

4. Discussion and Conclusions

P 920 line 15ff: Discussion of timescales and Fig. 6. This should be a key conclusion, but I do not understand the figure. Presumably the first blue dot is at the first location that the streakline is observed (since it is stated that streaklines are not evident when ice is not sliding). Assuming this is correct, the residence time for say the Byrd streakline would be about (18,500 – 11,000) or 7,500 yrs (rather than 18,500) IF the streaklines originated from a point and propagated down flow at the same velocity as present. However this is a big IF; alternative hypotheses relating to the origin and evolution of streaklines need to be considered. Possibly the reviewer has misunderstood the Figure. The point at which the streakline is first observed is at "0" and not the first blue dot. So for the Byrd Glacier, say, the streakline would indeed represent 18,500 years.