

# ***Interactive comment on “Atmospheric and oceanic forcing of Larsen C Ice Shelf thinning” by P. R. Holland et al.***

## **Anonymous Referee #1**

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Review of

Atmospheric and oceanic forcing of Larsen C Ice Shelf thinning

by Holland and others

General

This paper sets out to resolve a long-standing issue on the causes of Larsen C ice shelf thinning. While earlier studies ascribe the surface lowering/thinning to enhanced basal ice melt, later studies suggested that firn compaction, notably its northern regions, could also have a major impact. By quantifying both terms separately along a survey line in the central ice shelf area, that has been revisited multiple times, the authors conclude that it is likely that both processes explain a similar amount of sur-

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face lowering along this line. However, the uncertainties remain large because of the heterogeneous datasets used, which contain significant noise. The extensive error analysis does justice to these uncertainties and provides the right context to interpret the results.

### Recommendation

The paper is well and clearly written, albeit somewhat long, and the figures are of good quality. It is certainly an original and important contribution to an important research topic, and the science, including an extensive uncertainty estimate, appears careful and robust. That is why my assessment is that relatively minor revisions are needed for this paper to become publishable in *The Cryosphere*, see below.

### General comments

In the introduction, previous studies on the possible reasons for the surface lowering of LCIS are discussed, but no introductory discussion is dedicated to the spatial variability in observed elevation changes (Figs. 1a and 1b). In previous studies, were these significant variations thought to represent measurement uncertainty or real signals, or both? Please elaborate.

In spite of (or owing to!) the careful consideration of all potential sources of error, the uncertainties in the final results are large, and that is why I feel the title could be somewhat less 'definitive', for instance by starting with the wording 'A primary estimate of...', as is used in the first sentence of the discussion.

p. 256, l. 8: Can the presence of liquid water really be ruled out, given the recent finding of perennial firn aquifers in Greenland?

p. 260: Can the assumption that the southward decrease in surface elevation between surveys, which in the paper is now simply ascribed to increasing radar penetration to the south, where firn air content increases, be corroborated for instance by a quantitative comparison with firn air content (e.g. using the data of Holland and others,

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2011)?

Fig. 4: Why are no data points provided for ice and air thickness anomalies and the satellite data?

p. 264, l. 21: Replacing the surveyed elevation trend with the satellite elevation trend (Fig. 5a vs. Fig. 7a) completely changes the interpretation of the air loss signal, from one that is monotonically increasing in magnitude from north to south, to one that has a maximum magnitude in center of the survey line. In view of this rather arbitrary swapping of data, the word 'conclude' (p. 264, l. 26) is too strong to my taste, and should be replaced by something like 'hypothesise'.

Specific comments

p. 252, l. 11: "Though the ice loss is much larger, ice and air loss contribute approximately equally to the lowering." This is ambiguous; the word 'larger' has no explicit meaning here (mass, vertical motion?). Please reformulate in terms of contributions to ice shelf thinning or surface lowering.

p. 253, l. 7: Please explain how firn compaction could -indirectly- have led to ice shelf weakening.

p. 254, l. 21: and THAT the northern edge of LCIS is at this limit...(?)

p. 254, l. 23: lower firn air THICKNESS.

p. 254, l. 23: high -> significant.

p. 254, l. 26: Modelled firn compaction entirely offset the lowering in one study of 2003–2008 (Pritchard et al., 2012), BUT WITH A LARGE UNCERTAINTY

p. 255, l. 3: suggest to remove 'strongly'

p. 257: In expressions 7 and 8, is the different in significant numbers in the factors real, or should 1.06 be 1.060?

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p. 259, l. 24: thinner -> smaller

p. 261, l. 24: "... that is not supported by the remaining data." What remaining data?

p. 263, l. 19: If anomalies relative to 2004 are presented, should then 2004 not have a zero point for elevation, or are these hidden behind the red dot? Why no uncertainty for that point?

p. 294, Fig. 5a: the blue point in the legend appears to be a point in the graph, consider moving the legend to upper part of graph.

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Interactive comment on The Cryosphere Discuss., 9, 251, 2015.

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