Reply to reviewers' comments on "Oceanic and Atmospheric forcing of Larsen C Ice Shelf thinning"

We are very grateful to both referees for their support and constructive reviews, which have been invaluable in clarifying the paper. Their comments are re-printed in blue text below, with responses wherever needed. We note also that we have changed the title of the paper in response to the reviewers' comments. We have uploaded a revised version of the paper with 'tracked changes' highlighted, and the line numbers below refer to lines in this uploaded document.

In addition to responding to these reviewer comments, we have modified the paper in response to three new relevant studies. Jansen et al. (2015) detail the propagation of a large rift in LCIS that may proceed to threaten its compressive arch, Paolo et al. (in press) show that the surface lowering of LCIS has recently focussed on Bawden Ice Rise, and Khazendar et al. (in press) show that the remnant Larsen B Ice Shelf is showing clear signs of instability.

Anonymous Referee #1

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 surface 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.

The spatial variations since 1994 are regarded as a real signal in all studies. Some have used the northward intensification of lowering as evidence of a surface melting influence, since surface melting is known to be strongly northward-intensified. This argument is only indicative, however, because we have little knowledge of the spatial distribution of ocean melting. We have strengthened the sentence announcing the variation (line 55) and draw the reviewer's attention to the sentence outlining the melting argument (line 99).

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.

We considered this at some length, but couldn’t come up with a better title that succinctly captures the focus of the study. We feel that something like ‘An assessment of oceanic and atmospheric forcing of Larsen C Ice Shelf thinning’ contains too many sub-clauses. The abstract faithfully describes that we are assessing these forcings, and that our reported results are a primary estimate subject to considerable uncertainties. In the title, we have swapped ‘Atmospheric and oceanic forcing…’ to ‘Oceanic and atmospheric forcing…’ to reflect our primary finding of a greater role for ice loss than air loss (see response to reviewer 2).

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

Boreholes drilled in LCIS have yielded no evidence of a perennial aquifer. Specifically, 6 holes drilled by hot water and instrumented with thermistors in 3 widely dispersed locations produced no evidence of a borehole pressure drop, reduced drilling progress, or thermal signature that would be expected from an aquifer (personal communications with Keith Nicholls and Bryn Hubbard, 2015). The sentence has been modified to state that there is no evidence for a perennial aquifer (line 144), and this is now fully described in section 4.3 (line 568).

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, 2011)?

A quantitative comparison is not justified because radar penetration is affected only by the top few metres of firn, while the study of Holland et al. (2011) derives the total column-integrated firn air content, and there are many reasons why the two might not co-vary in detail. Nevertheless, we feel it is worthwhile noting that the southward decrease in elevation difference is at least consistent with less compact firn (greater penetration) in the south. The sentence has been modified to emphasise the qualitative nature of this agreement. (line 299).

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

The satellite data are a timeseries of quasi-monthly data from the 5 merged crossovers (section 2.5), and to add them to the plot would make it extremely dense. We never derive ice and air thickness anomalies for the individual surveys; equations (5) and (6) show how ice and air trends are derived directly from elevation and TWTT trends (i.e. the blue and black dashed lines in Figure 4 are derived directly from the green and red dashed lines). This has the advantage that uncertain quantities that are steady in time, such as the geoid and mean dynamic topography, are explicitly excluded from the calculation. This is now fully explained (line 178).

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’.

Changed to ‘Figure 7a suggests that…’ (line 419).

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.

The preceding sentence says that the lowering is caused by ice loss of ~0.3 m/y and air loss of ~0.03 m/y. The ice loss is larger. We have changed the sentence to ‘The ice loss is much larger than the air loss, but both contribute approximately equally to the lowering because the ice is floating’ (line 22).

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

Changed to ‘However, longer-term processes such as ice thinning and firn compaction must first have driven these ice shelves into a state liable to collapse by weakening the ice and enabling meltwater to pool on the ice surface’ (line 49).

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

Changed to ‘… suggests that atmospheric warming may have pushed some ice shelves beyond a thermal limit of viability (Morris and Vaughan, 2003); the northern edge of LCIS is at this limit’ (line 96).

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

Changed to ‘higher’ (line 98).

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

Changed to ‘albeit with a high uncertainty’ (line 102).

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

The trends are strongly negative and so we prefer the sentence as it is (line 106).

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

All numbers are given with 3 significant figures; there is no difference (line 184).

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

Changed to ‘lower’ (line 260).

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

Changed to ‘This is important because studies of LCIS that include these early data (Fricker and Padman, 2012; Shepherd et al., 2003) derive very rapid lowering in the 1990s that is not found if the early data are neglected’ (line 328).

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?

The dot for 2004 elevation anomalies relative to 2004 is indeed hidden behind the TWTT anomaly dot, since both are zero. The 2004 data have no error because the error bars refer to the standard error of the differences between the individual data points in each survey and their 2004 counterparts. Both points are now clarified in the text and figure caption. (lines 385 and 1157).

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.

We have enclosed the legends for figures 5a and 7a in boxes.

Review comments by A. Khazendar

Overview

The main objective of the manuscript is to describe a technique that partitions observed ice-shelf surface elevation changes into components of ice and air content changes. The technique combines measurements of surface elevation changes with contemporaneous travel times through the ice shelf of a radar signal. The method is applied to 8 surveys of a transect in the central part of the Larsen C Ice Shelf. The authors conclude that the observed surface lowering was probably due to both air and ice loss, with air loss more likely to be the more prevalent of the two. Possible implications of these findings for the stability of Larsen C are then discussed.

We are concerned that the reviewer has concluded that air loss is more likely to be the prevalent cause of the lowering. Our primary estimate is that ice loss is an order of magnitude larger than air loss and so we would argue that ice loss is the dominant change implied by our results. This is a complex issue, however, for two reasons: 1) since the ice is floating, these rates of ice loss and air loss contribute approximately equally to the lowering (within error bars they have the same effect on lowering, though the central estimate is that the air loss has a slightly larger effect); 2) if one arbitrarily neglects individual surveys, it is possible to render insignficant the conclusion of ice loss, but the conclusion of air loss is robust (Table 2).

We have addressed this issue by re-ordering the title to 'Oceanic and atmospheric forcing…' rather than 'Atmospheric and oceanic forcing…', emphasising in the abstract that the ice loss is much larger than the air loss (line 22), and emphasising in the conclusions that we argue ice loss to be the dominant change affecting LCIS (line 864).

The work addresses an important question. Attributing observed thinning in peninsular ice shelves to oceanic or atmospheric causes has been long debated as part of the effort to understand the destabilization of these ice shelves. Ice shelves on the eastern peninsula generally have lower basal melting rates compared with elsewhere in Antarctica, hence atmospheric warming could be as important a factor in observed ice shelf thinning as enhanced basal melting, if not more so.

The method devised is highly innovative and promising. One of the main challenges in implementing it is the high uncertainty of the observations, especially in a situation where observed thinning rates are relatively low. The authors address this issue with an extensive discussion of the errors involved and by using different combinations of the data sets in performing their calculations. The manuscript could probably benefit from review by someone with more knowledge of statistical error analysis than I do. Apart from the uncertainties, one aspect of the theory remains unclear as discussed below.

See response below.

The manuscript is mostly very well written and presented, if somewhat sprawling. In particular, parts of section 5.2 on ice-shelf stability read like a review paper with little relevance to the current work and can benefit from some abridgement.

Section 5.2 reviews the future prognosis for LCIS. Since our results allow us to assess ice-loss and air-loss timescales for the first time, it was not previously possible to speculate with any certainty upon the possible mechanisms for imminent LCIS collapse. We regard this as a crucial exposition of the implications of this work, which is all the more important in light of the Jansen et al. (2015), Paolo et al. (in press), and (Khazendar et al., in press) papers and the possibility that LCIS collapse is now imminent. We have shortened section 5.2 wherever possible.

Main remarks

P. 256, equations 1 and 2: neither equation has information about the relative vertical distributions of ice and air in the ice shelf. The method as I understand it would work, however, because it combines the observed surface elevation with the observed change in TWTT. The combination constrains the possible partitioning scenarios and is able to attribute the observed change to ice and/or air change. This approach, however, seems to have an underlying assumption. Namely, that signal propagation in, and the dielectric properties of, an ice/air mixed medium will change linearly with the change of ratio of air to ice. Is this the case?

Equation (2) states that the total delay of the radar wave passing through the ice shelf is the linear sum of the delay due to the total solid ice thickness and the delay due to the total thickness of air inclusions in the firn. This is also known as the 'Complex Refractive Index Method' and has been used in quite a few studies of glaciological radar data. The method is introduced briefly and equation (2) is now provided with a reference to the CRIM (Arcone, 2002) (line 157).

P. 268 L. 5: I believe that instrument and processing specifications and errors deserve more discussion, especially given the relatively small thinning rates in this study. For example, what is the time resolution and bandwidths of the instruments used, and are they sufficient to distinguish unambiguously the changes in TWTT?

This is an extremely good question, to which the answer is complex. In the text below, we use 'precision' to refer to the length of time between samples of the radar return echo power. We understand the reviewer's question to be whether or not the precision in the data is fine enough to capture the observed thinning signal.

The TWTT data are recorded with a wide variety of instruments and subject to different processing techniques to optimise the signal prior to picking (Table 1 and Section 2.3). When considering the TWTT changes, the important measure is not the precision of the instrument, it is the precision of the processed data from which the TWTT is picked. This is usually lower than that of the instrument, since processing to reduce the noise in the data also reduces the resolution. The precision in the echograms picked varies between surveys, with a mean of ~4 m ice thickness equivalent (~24 nanoseconds) and a range of 0.125—8.8 m ice equivalent. The mean TWTT trend is ~3.5 m ice equivalent over 15 years (Figure 4), and so at face value this trend may seem indistinguishable by the data. There are many factors to be taken into consideration, but there are two primary reasons why this is not the case.

Firstly, the position of the 'first break' in the return echoes can be estimated at a higher precision than the TWTT data. The waveform of the echoes are at least 10 times the length of the TWTT precision, and it is the position of a gradient at the leading edge of this waveform that we seek to determine. In our processing, the leading edge is fitted using high-order interpolation, and the position of the first break is determined at a nominal precision one tenth that of the original data (i.e. a mean of ~0.4 m).

Secondly, and most importantly, each difference plotted in Figure 4, from which ice and air trends are calculated, is actually the mean of a population of thousands of point differences, with standard deviations of ~10 m (Table 3) and ranges of ±40 m (Figure 2). These populations are well-resolved by the TWTT precision, and so we are able to detect the mean difference statistically to a precision much finer than that of the individual data. As an extremely crude illustration, imagine if precision is 1 m ice-equivalent in two surveys and we have observed 1000 TWTT differences between them; if 300 points show a first-break 1 m shorter and 700 show no change, the mean change estimated would be a reduction of 0.3 m; this is less than the 1 m precision of an individual data point but a validly precise estimate of the mean difference between surveys.

We have responded to this point by adding an abridged version of the above discussion to section 2.3 (line 245).

P. 264 L. 19-24: the radar elevation trends were considered unreliable and replaced with satellite elevation trends. I assume that the same TWTT were then used in the calculation of ice and air losses. But, if the radar surface elevations were judged unreliable, wouldn't that mean that the corresponding TWTT should also be considered suspicious, given that TWTT are obtained from the signal travel time between the (unreliable) surface and bottom of the ice shelf?

For airborne surveys, the TWTT and elevation data are independent datasets derived from separate instruments. TWTT through the ice is derived from picking and then differencing the surface and basal echoes from an ice-sounding radar, while elevation is derived separately from an altimeter. The surface pick from the ice-sounding radar is not coincident with the altimeter elevation, and we are free to discard one set of measurements and retain the other, if justified.

In the text referred to, we consider the satellite-derived elevations to be more reliable than the surveyed elevations due to a concern over radar altimeter penetration in the 1998 survey. This could affect the surveyed elevation trend because later surveys use laser altimeters or GPS, which have no surface penetration. That concern does not apply to the TWTT, since the surface pick of the ice-penetrating radars has similar penetration in all surveys. This is now noted (line 412).

Figure 3 and caption: I find these confusing. North of latitude -67.8, the differences plotted in the figure are positive, implying that the (lower due to penetration) values from 2011 BAS survey were subtracted from the (higher) 2010 IceBridge laser altimetry measurements. South of -67.8, the caption explains, the 2011 data become progressively lower due to increased radar penetration of the firn, which means that their difference from the 2010 data laser altimetry data should increase, yet the opposite is shown in the figure.

As described in section 2.4, the 2011 radar altimeter elevation data are subject to two problems; first, they require general calibration, and second, they are subject to firn penetration in the south that needs to be removed to make them comparable to the laser altimeters and GPS used in the other surveys.

The differences shown in Figure 3 are 2011 minus 2010. For the uncorrected 2011 data (blue dots) this is generally positive, implying that the 2011 radar altimeter is apparently recording the surface higher than the 2010 laser altimeter, which sampled the surface only 10 weeks earlier and was precisely calibrated. Therefore, we regard this difference as a calibration error and correct the 2011 data by subtracting from them everywhere the mean offset north of 67.85S, 1.59 metres.

After this correction, the blue dots would be shifted downwards by 1.59 metres everywhere. This then produces a negative 2011-2010 difference in the south of the section, i.e. the 2011 radar altimeter is recording the surface lower than the 2010 laser altimeter. We attribute this to radar firn penetration, and correct it by adding a linear fit to the mismatch south of 67.85S. The corrected 2011 data minus the 2010 data are shown by the green dots.

To address this point, we have substantially rewritten the relevant text (line 289 onwards) and also rewritten the figure caption to reflect the above logic.

P. 268 L. 7-8: How does a spatial offset from the reference line introduce an error? Doesn't each data point come with its own spatial coordinate?

LCIS gets progressively thinner from west to east, with a progressively greater firn air content. If one survey were systematically to the west of the others it would sample thicker ice with less air, and this would appear as a temporal change in ice and air; an inter-survey error. Since none of the surveys is systematically offset this is not a problem, but the measurements do not precisely follow the same line, weaving slightly to the east and west, and this introduces intra-survey error. The sentence has been modified to clarify this (line 513).

Other remarks

P. 253 L. 6: here or elsewhere in the manuscript, please consider citing earlier work that investigated meltwater-induced ice fracture (e.g., Weertman, 1973; van der Veen, 1998), in addition to the work cited here already.

There is a large body of literature on meltwater-induced ice fracture so in the interest of brevity we have only added the van der Veen reference (line 45).

P. 259 L. 27 and Table 1: if the 2009 IceBridge TWTT data were not included, were any other data from this campaign used in the analyses leading to the final conclusions of the work? If no, why keep referring to 8 surveys instead of 7?

The surveyed elevation data from the 2009 IceBridge survey were used throughout, so the paper does consider 8 surveys. The retention of the elevation data from 2009 is now explicitly stated (line 264).

P. 253 L. 25-26: ocean water at or below sea-surface freezing temperature could still melt ice at depth. Replacing "sea-surface" with "in situ" would probably be more accurate.

Changed to '…found the ocean to be at or below the surface freezing temperature, suggesting that it is only capable of slow melting' (line 71).

P. 253 L. 27-29: even if marine ice presence were widespread it does not necessarily mean that cooler ocean temperatures are spatially and temporally prevalent. Existing marine ice could have accumulated mostly under past conditions.

Changed to 'widespread marine ice in LCIS suggests that these temperatures are spatially and historically prevalent' (line 74).

P. 254 L. 5: consider showing the location of the sonar measurements on the map of Fig. 1.

Sentence changed to ‘sonar measurements near Kenyon Peninsula in the south of LCIS’ (line 79).

P. 252 L. 16: “in [the] future”.

We prefer the original wording, which is valid English usage (line 28).

References

Arcone, S. A.: Airborne-radar stratigraphy and electrical structure of temperate firn: Bagley Ice Field, Alaska, USA, Journal of Glaciology, 48, 317-334, Doi 10.3189/172756502781831412, 2002.

Holland, P. R., Corr, H. F. J., Pritchard, H. D., Vaughan, D. G., Arthern, R. J., Jenkins, A., and Tedesco, M.: The air content of Larsen Ice Shelf, Geophysical Research Letters, 38, L10503, 10.1029/2011gl047245, 2011.

Jansen, D., Luckman, A. J., Cook, A., Bevan, S., Kulessa, B., Hubbard, B., and Holland, P. R.: Newly developing rift in Larsen C Ice Shelf presents significant risk to stability, The Cryosphere Discussions, 9, 861-872, 10.5194/tcd-9-861-2015, 2015.

Khazendar, A., Borstad, C. P., Scheuchl, B., Rignot, E., and Seroussi, H.: The evolving instability of the remnant Larsen B Ice Shelf and its tributary glaciers, Earth and Planetary Science Letters, 10.1016/j.epsl.2015.03.014, in press.

Paolo, F. S., Fricker, H. A., and Padman, L.: Volume loss from Antarctic ice shelves is accelerating, Science, in press.