

Atmospheric and oceanic forcing of Larsen C Ice Shelf thinning

By P. R. Holland et al., submitted to The Cryosphere, 2015

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.

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.

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.

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?

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?

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?

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 firm, which means that their difference from the 2010 data laser altimetry data should increase, yet the opposite is shown in the figure.

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?

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.

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?

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.

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.

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

P. 252 L. 16: "in [the] future".