The Cryosphere Discuss.,
doi:10.5194/tc-2016-27-RC1, 2016

The Cryosphere Discussions

Interactive comment on "Ice core evidence for a recent increase in snow accumulation in coastal Dronning Maud Land, East Antarctica" *by* M. Philippe et al.

Anonymous Referee #1

Received and published: 23 March 2016

Review: Philippe et al., Ice core evidence for a recent increase in snow accumulation in coastal Dronning Maud Land, East Antarctica

This paper reconstructs the accumulation-rate history for a site in coastal East Antarctica using a 120m ice/firn core. The authors find an increase in accumulation rate in the past few decades compared to the previous ~200 years. The authors suggest the increase is due to recent warming, supporting model projections of increased accumulation in Antarctica as the world warms. Unfortunately, flaws in the layer correction and timescale development raise major questions about the inferred increase in accumulation. This paper should be rejected.

The paper has much potential but was not yet ready to be submitted. In particular, the underdeveloped timescale and inaccurate correction for layer thinning raise questions about the inferred increase in accumulation. The ice-core records presented here are potentially very interesting, but to become publishable, this paper needs considerably more analysis, discussion, and possibly even more lab measurements.

Major Issues This paper has three major issues: 1) the timescale is not convincing, 2) the description of the layer thinning correction is too small and 3) the climate implications are underdeveloped.

1 - Timescale The development of the timescale is the crux of this study and it is clear that considerable effort has gone into the timescale. The paper makes it clear, without explicitly stating so, that the timescales was quite difficult to develop. I have great sympathy for anyone who develops ice-core timescales. However, the timescale as presented is not convincing for two reasons. First, the isotope sampling is too low to resolve annual layers for much of the core. At a sampling interval of 10cm (above 80m), this yields only 5 or 6 samples per year for much of the timescale given the accumulation rates. You need about twice that to resolve clear annual layers, especially on a proxy such as oxygen isotopes that have relatively noisy seasonal cycles. Statements like “no ambiguity in layer counting is detectable above 62.38 m (i.e. 1933 AD)” are in direct contradiction with the need to perform major ion analysis “for sections of unclear isotopic seasonality” and I can see ambiguity in Figure 2 (near 20 and 29 m depths). It seems odd to me that for a relatively short core, the whole thing wasn’t sampled at much finer resolution (water isotope analyses are cheap and don’t need much ice) and that aerosol analysis wasn’t performed on the full core.

Second, the volcanic matches are not convincing. In Figure 4 it appears that any small peak that rises past the 2sigma level is considered a volcano if it happens to be of the correct age. This may be because the data are normalized before identifying volcanic peaks. Regardless of the normalization issue, using ECM (or sulfur) at coastal sites to identify volcanic events is very difficult because the volcanic signal gets overwhelmed by marine inputs.

The timescale is the crux of this paper. This means considerably more effort needs to be made to describe it convincingly, for both the annual layers and volcanic matches. Items that this paper needs:

1) A clear description of what measurements were made at what depths (i.e. where were aerosols measured and show the ambiguities and how they were interpreted)

2) An analysis of the impact of the low sampling resolution on the ability to resolve annual layers (often, low resolution leads to picking false peaks)

3) A realistic assessment of annual-layer interpretation uncertainty

4) A critical assessment of volcanic matches. I.e. why is Cerro Azul 1932 not one of the bigger, yet unmatched, peaks about a meter above or below. (this same question applies to pretty much every match, except for possibly Tambora).

5) A description of why ECM loses the annual signal yet preserves the volcanic signal

6) Why Krakatau isn't observable in the ECM record and what the distinctive characteristics in the aerosol record are that allow it to be identified. Also a description of why the technique to identify Kratatau wasn't applied for the full core.

It sounds like the only truly identifiable volcanic event was Tambora. The authors need to make use of Tambora, and pattern with the unknown 1809 eruption, to make a strong case that this is indeed properly matched (Figure 4 does not do this). Plot it against high resolution ECM/Sulfur/Sulfate records of this event. If the authors can demonstrate that this is a clearly identifiable match, then it would strongly support their annual layer interpretation.

2- The corrections for flow-induced layer thinning reveal a lack of understanding of how ice flow and are clearly underestimated. In particular, it is disappointing that the authors don't make use of the detailed ice-flow modeling that's been done on Derwael Ice Rise (Drews et al., 2015) to develop the vertical thinning function. It is clear from phase sensitive radar measurements (Kingslake et al., 2014) that the simple approximations for vertical thinning have trouble replicating the vertical strain pattern under ice divides. The Nye assumption is so obviously not applicable to Derwael Ice Rise, which has a distinctive Raymond Bump, that it should not even be considered. The authors' don't supply the kink height value of the D-J model. Using a kink height of nearly 1, Kingslake et al. could still not match the pattern under Roosevelt Island. Since the authors say the kink height is below the zone of interest, I can infer that they didn't use anything greater than ∼0.7. This will lead to an underestimation of the amount of strain experienced by the ice in the core. Thus the older accumulation rates will be underestimated, and it will appear that there has been an increase in modern accumulation rates. The underestimation is likely exacerbated by the preferred thinning rate of 3cm per year for the ice rise (Drews et al., 2015). Getting the thinning right is critical to primary conclusion of this paper.

3- The discussion of atmospheric and sea-ice patterns seems like an after thought. I'm not sure why the authors choose to analyze only anomalously high and low years. I also wonder why the authors don't compare the inferred accumulation rate history to the climate reanalyses. Other cores (e.g. Medley et al., 2013, GRL; Morris et al., 2015, Nature Geosciences) find good correlation of annual accumulation.

Below are a few additional points that caught my attention.

P1, L27-29 – your data do not actually support this because your thinning correction is much too small.

P2, L10 – you should mention timescales. Frieler et al only address glacial-interglacial changes. The most directly comparable ice-core record to yours is from Law Dome, which does not show a consistent relationship between accumulation and temperature in the Holocene.

P3L1 – in the ice-core community, continuous is generally used to mean a melting system where discrete samples are not cut. Using continuous to mean that all of the core has been sampled discreetly is confusing.

P3,L13 – Be specific about what you mean by local ice flow. You should really mention that it's an ice rise with a well developed Raymond Bump that has likely been stable for thousands of years.

P4,L25 – DC-ECM does not depend on the impurity content at the crystal boundaries. It depends on the acidity.

P4,L28 – a ∼30cm smoothing window seems really large to me.

P5,L1-3 – Did you normalize the data before identifying the volcanic peaks? If so, you can no longer reliably identify volcanic events with a threshold because the increased conductance of volcanic events would impact the normalization. Is the 2sigma threshold then for the entire data set. I'm confused, but I think this is a major problem.

P5,L5-22 – This section should be entirely redone. Get a thinning function from the ice-flow modelers working on Derwael Ice Rise.

P6,L17 – explain what changes in the ECM and why

P6,L27 – explain how Krakatau was identified

P10,L11 – why are the uncertainties being presented after the results

P10,L30 – there is a lot complexity in the position of the divide, the Raymond Bump, and the minimum accumulation (which is offset from the divide). There needs to be a much more detailed discussion of whether small (i.e. one ice thickness)

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