

Interactive comment on “On the occurrence of annual layers in Dome Fuji ice core early Holocene ice” by A. Svensson et al.

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In this paper, the authors use a continuous flow analysis (CFA) system to measure dust, ammonium, sodium and liquid conductivity on an early Holocene section of the Dome Fuji, and demonstrate seasonal cycles in the core which allow them to count annual layers and deduce a mean accumulation rate for this section.

The paper is an important contribution to the field because, although annual layers have been clearly demonstrated deep in for example relatively high accumulation Greenland ice cores, and in high accumulation West Antarctic ice cores, it is perhaps the first convincing attempt at layer counting in relatively low accumulation rate East Antarctic cores – Dome Fuji has an accumulation rate of around 27 mm water per annum.

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While diffusion has a tendency to quickly smear out the seasonal signal in stable water isotopes making them unsuitable for layer counting purposes, it is generally held view that if the flux of chemistry to a site has a clear seasonal signal at the surface then, for some species at least, this seasonal signal is likely to be maintained to considerable depth. Post depositional migration (methane sulphonic acid is a good example) may affect some species, while grain growth might sweep some species to grain boundaries. However, it has been demonstrated that for many analytes in ice cores, the seasonal cycle is maintained –this paper demonstrates the case for dust, sodium and ammonium. Thus, recovering the seasonal cycles and layer thickness at depth becomes only a matter of sample resolution, and the CFA technique has amply demonstrated that it can recover seasonal cycles in the ice, and at an acceptable analytical speed.

But, I'm not sure that they really achieve 'a counted time scale' as the final line of the conclusions claim (P816, L10). Was not the eye guided by an existing knowledge of the number of years between each of the three volcanic peaks already established in the NGRIP and EDML cores? There are many uncertain layers, and I think other observers might have produced quite different 'time scales' over this period had they not had the guidance of how many layers ought to lie between volcanic peaks.

In focussing on 'a counted time scale', I think the authors miss commenting on another significant benefit to their technique. Most deep ice cores use a model time scale rather than a layer counted time scale (though a layer counted time scale has been developed for Greenland ice to in excess of 60 kyrs). The models tend to use the stable water isotopes to infer temperature and from that accumulation rate, which is then integrated over the ice column, corrected for thinning, and trained on occasional reference horizons, to give the final time scale. Independent observation of the annual layer thickness at various depths through the ice column, particularly during periods of rapid climate change, would be extremely valuable in verifying the model time scale. Perhaps this is just a subtlety of wording, and perhaps it is implicit in the paper, but I feel the power of the high-resolution analytical technique is testing ice core time-scales

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has been missed and could be brought to the fore by the authors.

Even if the seasonal cycle in chemistry and dust is preserved at depth (and this does seem likely in the early Holocene ice here from a low accumulation site, and to at least 60 kyrs at high accumulation site in Greenland), then the CFA technique is perhaps only marginally capable of recovering the seasonal cycle from the ice where the layer thickness is small. The continuous melting technique generates some mixing of the melt-water directly at the melthead, while there is further dispersion in the tubing and reaction columns between the melthead and the detectors. This inevitably results in a more diffuse seasonal signal than might have been present in the ice, and likely limits the annual layer thickness that can be resolved to perhaps something around 10 mm or perhaps a little better. Other high resolution techniques have been developed that do not suffer this analytical dispersion of the original signal. Thomas (2008, doi:10.3189/172756408784700590) described mm-scale sub-sampling of ice sticks using a microtome for subsequent discrete analysis - laborious but effective in eliminating signal dispersion. Several groups have been developing laser ablation mass spectrometry (LA-ICP-MS) for in-situ and mostly non-destructive highly resolved analysis of ice (Reinhardt, 2001, doi:10.1007/s002160100853; Müller, 2011, doi: 10.1039/c1ja10242g; Sneed, 2014, doi: 10.3189/2015JoG14J139). Given the power to test age-scale model accumulation, and layer counting at depth, I would have liked to see the author's comment that other high-resolution techniques might be valuable and complementary to CFA.

I'm unsure about the interpretation of the 'peculiar event', and feel it has been given too much weight in the paper. A first impression was that we had observed something similar deep in the Dome C core where volcanic spikes were wider in depth (and therefore time) than was likely for a single eruption, and had clearly displaced other species such as nitrate to shoulders either side of the main sulphate peak, indicating that dispersion of the original volcanic peak had taken place, and that other acidic species had been excluded from the central event and migrated in the ice. For example, Barnes (2003,

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doi:10.1029/2002JD002538) described peak broadening of volcanic sulphate peaks at 350m in the Dome C core, though does not allude to the displacement of other species, and unfortunately I can't now remember if and where this was published. However, this doesn't appear to be the case in the DF results since the peculiar event is only present on the one (younger) side of the volcanic peak, and only occurs in one of the three events recorded in this section. My second thought was analytical error, and I'd really like to see this excluded as a possibility before this section is accepted in the literature. Is there any chance of re-analysis of a parallel section?

P816, L2: for sure you have shown layers exist for the early Holocene, but extending this to the Eemian as you do here is speculative. The additional grain growth over >100kys might have disturbed the clear seasonal cycle in chemistry; while even with higher accumulation, thinning might mean that the layers are just too thin for your CFA technique (though maybe not for the even higher resolution techniques mentioned here).

The manuscript is well-written, in excellent English and is laid out well and logical. I have no minor technical points of note.

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