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Interactive Comment

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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This is a valuable paper, showing the kinds of features that can be resolved, even at low accumulation rate sites. It indeed shows that features of an annual nature may be resolved. I do not propose to carry out a full review but would like to make two comments.

The first comment concerns the "peculiar event". This is indeed strange. My first thought was that the high concentrations in the adjacent volcanic peak had induced movement of chemistry out of the sides of the peak. There are several documented examples of acidic anions such as nitrate and fluoride being "pushed out" of volcanic peaks, leaving a "hole" under the volcanic peak, and higher concentrations on the shoulders. Presumably this occurs in firn. By analogy, one might imagine ammonium being "sucked" into the acidic volcanic peak, causing a depletion on the side, and a peak (as observed) under the volcanic peak. However, this would not explain the situation for Na, the absence of any effect on the deep side of the volcano, nor the lack of effect in other such events. I therefore do not believe this is the explanation, but present it here just for completeness. The authors toy with the idea that the homogeneity is the result of a large sastrugi being formed: however the flat section is 20 cm thick, which would require a 50 cm surface feature, much larger than the sastrugi typically observed at sites on the plateau.

I therefore cannot explain the event, but I think the authors need to absolutely establish that it is real before they publish it. It has something of the look of an analytical issue, with sensors losing sensitivity for what would be about 10 minutes, after the acidic melt has passed. I am sure the authors would think this very unlikely but it can easily be checked. The authors must have core sections remaining. They could simply cut 20 samples at 1 cm resolution across this section and analyse them by ion chromatography. If the resulting depth profile is still flat (as I hope), they have proven that the event is real. I would strongly recommend such a check before putting such a mystery into the literature for us all to worry about.

My second comment concerns the implications of seeing some annual signals. I agree that some annual features can be seen, and in this sense, annual layers (perhaps even in the Eemian) may be resolved. However, the paper takes the extra step, in its very last sentence, of suggesting that a "counted time scale can be established" at Dome F. I think this is wildly optimistic, and perhaps points to some questions we should revisit about the philosophy of annual layer counting. In this case, it is accepted that a significant number of annual layers are missing. In addition, Figs 3 and 4 make it obvious that layer counting in the traditional sense has not been achieved throughout the sequence. Taking for example the section from 304.2-304.4 m, I would count maybe 3 peaks, while the figure shows 7 certain and one uncertain. Like the authors, I would know already that the accumulation rate is about 3 cm, so I would insert extra year marks to achieve

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about the right spacing. However this is not layer counting - it is assigning of year dividers in a section where we think we already know the number of years. The authors assign "certain" years to sections with no chemical indication of a year, and thus end up with a 10% uncertainty, which seems unrealistically low if based only on the chemistry. It has only been achieved because the prior assumption of layer thickness leads to a tight condition on the allowable gap between counted layer marks. To me, this becomes circular, and it is not clear if the counting itself improves the chronology that would already be estimated based on the presumed layer thickness.

I don't want to give the wrong impression. I think that layer counting is an ideal way to establish a chronology when the layers are sufficiently clear and generally present. This is the case for example in most of the counted GICC05 age model, and in the counted section of the WAIS Divide core. However as soon as that is not the case layer counting becomes layer marking, and I do not expect it to improve our chronological uncertainty. I think this issue, even if the authors disagree, needs to be acknowledged. I would personally recommend removing that last sentence from the paper, as I think it raises false expectations, perhaps even for the authors themselves.

Interactive comment on Clim. Past Discuss., 11, 805, 2015.