Below we provide point-to-point answers to the comments of the two reviewers and to the short comment by Eric Wolff. We thank the reviewers and Eric Wolff for their insightful comments that have helped to improve the manuscript. Reviewer comments are in black and our responses are in red.

Review by F. Parrenin (Referee)

Received and published: 4 May 2015

This manuscript presents an analysis of annual layers in a 5 m thick section of the Dome Fuji ice core during the Early Holocene period, as well as a synchronization of this section to the EDML and NGRIP ice cores by the mean of three common volcanic events.

The main conclusions are that:
1) it is possible to count annual layers in this section with a typical uncertainty of _10%
2) there are three common volcanic spikes with the EDML and NGRIP ice cores in the age interval
3) the results of the layer counting are compatible with the EDML and NGRIP layer counted chronologies
4) among the three volcanic spikes, 2 have corresponding high dust concentrations (tephra layers)
5) there is a so-called ’peculiar event’ in the bottom part of the section, where the variations of the various proxies are very small. This event is suggested to be due either to snow redeposition by wind (sastrugi) or to a high precipitation event (blocking event).
6) the accumulation rate (3.0+-0.3) is slightly higher than what can be deduced from ice isotopes

This manuscript is useful for two reasons. First, it informs us on the snow accumulation process in central East Antarctica. In my opinion this is the most important aspect of the manuscript but it is not emphasized in the abstract. Second, it opens an interesting perspective on the annual counting of the Eemian section of the Dome Fuji ice core, a period for which we currently don’t know the duration better than a few kyr. For the second point, I have a question that the authors might want to discuss in the manuscript.

What is the best location in Antarctica for counting the Eemian? There is a compromise to be found between a high accumulation rate and a high thinning factor, so this question is not completely trivial. Is Dome Fuji really among the best locations?

Based on the present study alone, we cannot judge which Antarctic ice core would be most suited for resolving the Eemian in annual resolution, but we can of course list the approximate thickness of the Eemian section in the Antarctic ice cores:
EDC: Eem covers ca 250m depth interval (1510-1760m)
DF: Eem covers ca 200m depth interval (1610-1810m)
Vostok: Eem covers ca 300m depth interval (1600-1900m)
This information is now given at the end of the manuscript.

The manuscript is clear and concise so I have very few technical comments.

- abstract: the conclusions regarding the accumulation process at DF should be emphasized here.

Included.

- p. 812, l. 9: Note that Parrenin et al. (CP, 2007) also found that the accumulation at DC during the Early Holocene period was larger than what can be inferred from the ice isotopes. I therefore suggest to add to this sentence: '..., in agreement with what was inferred at EPICA Dome C (Parrenin et al., 2007)."

Included.

- p. 815, l. 2: the 'peculiar event' is now called 'P3 tail event'. Please use a consistent denomination.

Changed.

- p. 815, l. 3-13: I suggest to start this paragraph with 'Another possible explanation for the event is related to unusual meteorological conditions'. Then, the fact that it might be related to the volcanic event is only a sub-hypothesis inside this 'meteorological' hypothesis.

Changed.

- p. 815, l. 19-21: remove this sentence this the 'peculiar event' is already described in the following paragraph.

Removed.

- figure 1: there are strange grey lines in this figure that are not described in the legend.

The grey lines are now described as traverse tracks.

Short comment by EW Wolff

Received and published: 21 May 2015

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.

Indeed, this is a strange event and we admit to not being able of giving a satisfactory explanation for it. We have, however, considered some of the possibilities mentioned above:

- The core was melted and analyzed from top to bottom. Therefore, the peculiar event was analyzed before the large acidity peak (P3) was melted. Furthermore, there is a bag separation just above the P3 peak (at 306.40m depth), so the peculiar event has been physically separated from the acidity peak during measurement.

- The DEP curve shown in Figure 2 and now also in Figure 4 was performed in Japan after the CFA analysis on a parallel section of the core. Across the peculiar event, the DEP curve has a very similar shape to that of the meltwater conductivity. We take this as a sign (but not a proof) that the chemistry records are those of the ice and not an analytical artefact.

- Unfortunately, there is not sufficient ice core sample left in the granted ice core section to perform high resolution IC analyses. It is questionable that we would learn much more from such IC analysis than what we already see in the DEP profile. Neither of these techniques are able to resolve the high frequency (annual) variability.

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 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.

*Maybe we have been too much on the optimistic side concerning the establishment of a counted time scale for the Eemian section of Dome F. We now modified the last sentence to ‘*The present study suggests that annual layer counting in the Antarctic Eemian period may help to constrain the chronology of that section, if annual layers are preserved*’. Bearing in mind that the duration of the Eemian section of the Antarctic ice cores currently is fairly poorly constrained.*

On the other hand, some of the not-so-young authors of the present manuscript remember when we published the GICC05 time scale about a decade ago. At the time, we were given many warnings that annual layer counting in glacial ice in Greenland would not produce a useable time scale. Given the success of the GICC05 time scale, we may keep part of the optimism concerning layer counting in the Antarctic Eemian at least in our minds.

Review by R Mulvaney (Referee)
Received and published: 22 May 2015

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.

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.

The final line of the manuscript has been modified as follows: 'The present study suggests that annual layer counting in the Antarctic Eemian period may help to constrain the chronology of that section, if annual layers are preserved.'

There are many uncertain layers because the annual signal is not very prominent due to the low accumulation. Most likely, not two persons would pick layers in exactly the same way, but the many uncertain layers increase the chance that different layer counting attempts agree within error estimates. If the layer counting results in a 'time scale' is of course a matter of debate. Maybe 'event duration estimate' would be a better phrase.

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 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.

With an annual layer thickness close to 3 cm in the present study, we are not close to the resolution limit of the CFA technique, but, indeed, it is relevant to mention other high-resolution techniques and we now refer to those techniques in the introduction.

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, 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?

Unfortunately, we are not able to obtain sufficient ice for a high-resolution re-analysis of the peculiar event. The DEP curve shown in Figures 2 and 4 is, however, obtained independently from the chemistry profiles on a parallel ice core 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 thickness of the Eemian section in the EDC, DF, and Vostok ice cores is about 250 m, 200 m, and 300 m, respectively. This may provide sufficient depth resolution to resolve annual layers by CFA. Other high-resolution techniques will also be able to resolve such layers, but they may not cover the entire Eemian depth intervals.

Annual layers have been detected in 100 kyr old ice in Greenland, where the ice is much warmer and grain growth is much more active than in Antarctica (Svensson et al., 2011). Annual layers may therefore also be identified in Antarctic Eemian ice. We now mention this at the end of the manuscript.

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

Reference:

Svensson, A., Bigler, M., Kettner, E., Dahl-Jensen, D., Johnsen, S., Kipfstuhl, S., Nielsen, M., and Steffensen, J. P.: Annual layering in the NGRIP ice core during the Eemian, Clim. Past., 7, 1427-1437, 2011.