This paper describes the new WD2014 chronology for the WAIS Divide ice core as far as 31.2 ka, where annual layer counting was abandoned. Clearly, good documentation of a timescale for such an important ice core as WAIS Divide is crucial. Here it is of particular importance as this is the only layer counted age scale (at least extending beyond a few centuries) from Antarctica, and it is therefore likely it will become a standard, much as GICC05 has, to be transposed to other cores and to records from other palaeoclimate media. The paper presents clearly which data were used, and relatively clearly how the counting itself was done (though I do have some questions about that). The examples shown are, in the main, quite convincing that a good layer counted age model could be achieved in this core. I also appreciate that the authors stopped layer counting once they no longer believed in the viability of the method. The paper presents a discussion of uncertainty that is mature and philosophical, although again I have some comments about it in detail. It includes a good comparison with other age models although I think the assumptions behind that need to be explored a little further, and that the conclusion of the comparison should be tied up in the discussion of uncertainty. However in general the paper does what the title suggests and deserves to be published in CP with relatively minor changes.

I apologise to the authors that I am asking very picky questions but I think the assumptions made in layer counting need to be tested and improved. This age model is likely to be very important so I think it's worth documenting all the assumptions as carefully as possible. In practice this will involve quite small but important textual changes.

Detailed comments

Page 3428, line 13 and elsewhere. The Edwards et al paper seems to be in permanent review (it was also in review when the Buizert et al Part I of this paper was published). Please check its status before publication. However , more important, at section 3.2, the reader needs to be able to see the speleothem 18O data in order to judge whether the claimed synchronisation can really be achieved (some DO events don't give sharp transitions in some speleos). Since the deeper data were already published in Buizert et al, I am going to insist that the data for at least 27-31 ka are shown in an extra figure in this paper please.

Page 3430, para 1. You explain that resolution for most analytes was 1-2 cm which allows annual layers of 7 cm to be identified (sounds a bit marginal, but OK), and you state that layers of 2.5 cm can be identified using dust but you don't say what the resolution of the dust data was; please do so here.

Page 3433, line 13. I wonder why you say that manual interpretation is the "best" method. In what way is it best? I think this may be a hangover from previous layer counting philosophies. Of course humans will always see layers even when the automated system doesn't but does that make them better? What about the possibility that the manual counters are imagining layers (knowing the expected spacing) where the automated system correctly doesn't see them? Additionally I would argue that the automated system offers at least the possibility of giving an objective uncertainty estimate, as one can vary the parameters in the underlying model (peak shape and allowed spacing) within ranges estimated from modern seasonal cycles to get a range of layer counts. I don't think much needs changing here, but I just find that the statement that manual interpretation is in some cases "best" hides a lot of assumptions that should perhaps be explained to the reader who hasn't been exposed to the arguments.

Page 3434, line 20. 12:57????

Page 3435. I appreciate the honesty, that when there was uncertainty about layers, two investigators came to a consensus. But I think it would be more useful for the reader if you said what was the thinking behind that consensus. For example, in Fig 4, there is an obvious uncertain layer at about 1017.05 m: a clear extra peak in nitrate and DEP, as well as an extra peak in Na (albeit with a slightly unusual timing). What were the rules that led to the investigators deciding that this was not an extra year? Was it that the lack of peak in sulfate took precedence, or that the two layers would be too narrow compared to your assumptions about layer thickness? Did you always apply the same rules consistently?

Page 3435, line 19. Why was Straticounter not run? Based on Fig 4 it looks like a section in which the program should have worked well?

Page 3436, line 1 and 2. Again I am interested in the process when there was disagreement. Please provide (in supplement?) a couple of examples where the 3 interpretations disagreed, explaining how you reached a consensus, so that the reader can judge what you are taking as the rules in such a case.

Page 3436, line 15-18. Please redraft as this seems to be circular, appearing to say that since you didn't use Straticounter (for unexplained reasons), you couldn't use the algorithm.

Page 3436 and other places. It seems that (Table 2) when you used ECM alone you overcounted compared to other methods and your consensus by around 1%. But you did not then adjust your ECM counts below 2300 m for this apparently well-justified correction. Why not?

Page 3437-8. I am wondering exactly what decided where you stopped counting. Please in Fig 7 show a section below 2850 m so we can see what is worse about the ECM data there. There is already a lot of uncertainty in the sections shown so it would be helpful to see what a section you consider uncountable looks like.

Page 3437. You haven't mentioned here the additional difficulty that, once we reach glacial climate we can no longer be sure if the seasonality of inputs remained the same. This is a critically difficult issue for counting in Greenland where almost everything gets controlled by dust once glacial ice is reached. It is likely less severe for WAIS Divide but even so it is worth discussing (for example it is not so obvious whether nitrate would appear in summer or be associated with dust in the glacial period; I note that you don't show nitrate in the deeper ice however).

Page 3439-3440. I appreciate and even agree with your reasoning for not giving an uncertainty and claiming it has any particular statistical significance. However, it would still be interesting to understand how much variability there is between counters given the same rules for example. Can you give at least an idea whether individual counters had differences of order 1%, 3%, 10%?

This is just an aside and nothing to be done in this paper, but there is actually an issue I have never understood here that an unbiased person looking at the examples given in this paper and in GICC05 papers would give a huge number of uncertain layers (probably much larger than the quoted errors), and yet comparisons with other data suggest the errors are very small. It is sometimes claimed this is because counters are as likely to add as subtract a real year, but that assumes that all their assumptions and rules are correct and it is just as likely that they only add or only subtract years. I am therefore amazed that layer counts work out as well as they do.

Page 3441, line 6. Why do you assume that Straticounter missed layers rather than assuming that manual counters are adding in layers that don't exist? Can you look at specific examples where the methods agree and see what kind of features are being interpreted as years by manual counters but ignored by Straticounter. I see 3 options: 1) Straticounter is genuinely not tuned to see layers that exist; 2) manual counters are adding in layers that don't exist; 3) manual counters are adding in layers that are real years but that that don't have any real expression (in which case why are they doing that?). It would be interesting to consider which it is.

Page 3444, line 6 "currently being undertaken"

Page 3445, line 1. I think you are underplaying the assumption you are making here. Your assumption is that the jump in methane and the jump in speleo 18O are simultaneous within some uncertainty that is not being explained here. This may indeed turn out to be the case but it is far from being proved yet. And I think this really needs to be explained. Actually the assumption that Greenland 18O and methane are synchronous has been shown only for one DO jump by Rosen et al (it would be great to see more events so we can decide if this is a rule); the assumption that methane and speleo 18O are in synch rests on the idea that both are responding to rainfall in the same region and that the speleo has an instant response to rainfall 18O. This is a reasonable idea, but has really not been proved: there are numerous ways in which it could be wrong. It is certainly not yet clear that different speleos give the same dates for each DO event, and the European ice core groups have been holding off applying speleo dates to their cores until there is more evidence. The references given in line 2 of this page don't justify the assumption at all, they merely state it. I think this issue can be dealt with by spelling out in detail the reasoning behind the assumption and perhaps some estimate of the uncertainty in the assumption could be made.

(An alternative approach would actually be to turn this on its head: if you could find an alternative way to put an uncertainty on the counted age scale, then the close agreement of the counted age model to the Hulu ages actually supports the assumption. However, obviously you have to be careful with this: you can't use the same agreement in both directions, or it becomes circular.)

Page 3446, line 12-15. This is not really clear. Please rewrite. Also your statement that age errors tend to cancel out assumes the errors are based on something random whereas they could just as easily be systematic (e.g. if you always count shoulders as years when in fact they aren't).

Page 3448, line 26. I know it is often misused but the site is either Dome C or Concordia Station, but not Dome Concordia.

Page 3448. It should probably be mentioned that because of the many local volcanoes in West Antarctica, matching between WAIS Divide and East Antarctic cores is a little trickier than it sounds – but I agree it should be done.

Table 2. I am a little mystified that the consensus is sometimes (1940-2020 m, 2711-2800) higher than any of the individual methods. Please clarify in the text how this happen.

Fig 2. Please explain how you set month zero (is it based on Na at month 6, or what?)

Fig 3: is the lack on annual cycle in Na due to resolution issues? If so, I am not sure this plot makes much sense, but perhaps I missed its purpose.

And just to emphasise again that I think we should see Hulu 18O and methane for 27-31 ka please in a new figure.