Clim. Past Discuss., 8, C1293–C1296, 2012
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Climate of the Past Discussions

CPD

8, C1293–C1296, 2012

Interactive Comment

Interactive comment on "Duration of Greenland Stadial 22 and ice-gas △age from counting of annual layers in Greenland NGRIP ice core" *by* P. Vallelonga et al.

Anonymous Referee #1

Received and published: 20 August 2012

This paper uses high resolution continuous flow analysis (CFA) for multispecies to detect and count annual layers in a 50m section of the NGRIP ice core. This gives a revised duration for the GS22 event, and also provides a revised Dage value from observations of the warming events near the termination of GS22 (the full onset of GI21 and the earlier so-called 'GI21 precursor' warming). The results are interesting, the methodology is appropriate and the analysis appears well-conducted. The manuscript however is confusing at several points in its presentation, contains a number of errors, and fails to clearly articulate the significance of some of its key findings. I recommend that the paper be considered for publication after the shortcomings and errors have been addressed.

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The abstract does not clearly indicate the point of the results presented – partly because it contains extraneous matter (the first sentence is of limited value in the abstract) and partly because it doesn't clearly make the point that GS22 is a period of some interest, and why. This also applies to the introduction, which proceeds to page 2 line 17 before starting to lay out the case for such attention to GS22.

Section 4.1: The authors state that the GS-22 duration (2894+/-198yr) is inconsistent with the GICC05modelext duration (2620 yr). Even though the authors state that the errors are "maximum counting errors" in section 2.3, the casual reader is tempted to see the 275 year difference between model and counted as potentially a 1.4sigma difference, which isn't that significant. It would be clearer if the MCE values were specified as ranges, so it was clear that the duration (2696-3092 years) is indeed inconsistent with 2620 years.

P2592 line 8: 'lateral strain' – much better and more direct to refer to 'vertical strain' as the primary determinant.

P2592 line 23 (1060 yr) – I make the difference from Table 1 as 1010 yr.

P2592 lines23-26 – "...the ice-core based duration of GS-22 should be increased by approximately 350 yr". It isn't clear why this conclusion is reached. What is meant by the "ice-core based duration". I initially thought this meant the new counted duration (mean 2894yr) as this is 356 years. But the same sentence and next one refer to GICC05modelext and its errors. In fact, the discussion rather misses the point that the NALPS central value of 3250 years has an uncertainty of 526 years (2-sigma). This section needs to be clarified and rewritten.

The derivation of a new Dage figure is interesting, but it would be much more satisfactory if the authors set this in the context of other results and assumptions and drew some conclusions about the implications of their precisely counted values. The new Dage figures for NGRIP (ca. 550-590 yr +/- 70 y) are considerably lower than the estimates in Capron et al., 2010 through this period (around 740 yr +/- 80yr estimated from their Fig 5). Two points emerge, one of direct relevance to this paper, namely how would this affect the synchronisation with EDML, and hence the synchronised chronology that is referred to in this paper? Secondly, what does this say about the underlying Dage modelling that Capron et al., used? Is the model in question, or the assumptions? Presumably the lower Dage would imply a higher accumulation than Capron et al, but we know that the GICC05Modelext (or more correctly SS09sea06bm flow model) age now appears to overestimate accumulation. What do the authors suggest about these contrary implications?

Abstract, and delta age discussion – why settle upon the single value for ice age-gas age difference in the abstract, when determinations from two events were independently measured? If there is a reason why the value from the GI21 precursor is not regarded as reliable, then this should be addressed in the text.

Delta Age derivation and discussion in main text - This is confusing and the means for computation of errors is not specified, and only implied in the figure. The text states that of the two values, 550+/-52yr and 589+/-66yr, the second one is "slightly longer". The two results look completely consistent repeat measurements to me. Unless there is a reason to question one, the logical approach would be to pool the values and quote an estimated Dage of $\sim$570yr. The associated error is a little difficult to quantify without knowing what the quoted errors mean. This brings the matter of errors to the fore and raises questions.

It appears that the authors have simply taken the minimum and maximum layer counted ages (for GI21, 498 and 601 years), in computing the mean and range, so it would be useful to state this. It may even be preferable to simply state a range rather than a midpoint+/-semirange. In fact, the limits are dominated by the counting error rather than the sampling gap, although without depicting errors on the CH4 values (which are not given), it is unclear whether CH4 measurement errors would contribute if tested (eg through a monte carlo style lag determination).

CPD
8, C1293–C1296, 2012

As noted above, the second value for GI21 precursor actually gives a second 'experiment' to constrain the Dage value that the authors do not attempt to use. The larger sampling gap means that the upper bound of 613+41 years does not offer an additional constraint, but the lower bound of 561-38=523 years does. If the authors believe that the firn closeoff conditions are similar at the two epochs (an interesting point to consider) then they might reasonably combine the two events to come up with a Dage range of 523-601 years. While this is not necessary, it would add to the discussion.

Table 1: Personal preference "+/-" as a column header is imprecise, although as I read the paper it becomes clear given the widely ranging error bounds used (1 sigma and 2 sigma formal errors and maximum counting errors). The word uncertainty might be better.

Table 1: Errors: - it is unfortunate to use both 1-sigma and 2-sigma errors in the table. At the least, each should say what is adopted, but better still would be to standardise on one or the other. For footnote f – add that Boch is quoting 2-sigma errors.

Table 1: GI-22 end – given the text refers mostly to GS-22 start and duration, would it be better to head this column "GS-22 start"?

Table 1: (ss09sea) in parenthesis is confusing. Is this different to the ss09sea06bm that is used in GICC05modelext? And if it isn't, then would it be better to stick with GICC05modelext, as used elsewhere?

Fig 4 caption: Horizontal numbers indicate [age] differences

Interactive comment on Clim. Past Discuss., 8, 2583, 2012.

CPD

8, C1293–C1296, 2012

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