

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

P. Vallelonga et al.

ptravis@nbi.ku.dk

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Response to reviews are shown by ">" here and changes are shown in red in the attached file.

Referee #1 (www.clim-past-discuss.net/8/C1293/2012/) The referee makes a number of valid points which improved the manuscript for accuracy and clarity - we appreciate their attention to detail. Our responses are specified below.

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 C1611

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.

>The abstract has been rewritten to clearly describe the aims and relevance of the manuscript. Some general statements, which should be common knowledge to the readers of CPD, have been removed. The majority of the introduction has been maintained because we feel that the material is relevant to the topic.

Section 4.1: The authors state that the GS-22 duration (2894 \pm 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.

>We have edited the text so the duration of GS-22 is expressed as a range rather than a value-and-uncertainty. This approach has also been used at p.2590 lines 17 and 22.

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

>'lateral strain' changed to 'vertical strain'

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

>'1060' changed to '1010'.

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.

>We have rewritten the second paragraph of section 4.1. We now state our findings, and their implications for GICC05modelext, more clearly.

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?

>We have extended section 4.2 to discuss the likely sources of discrepancy between GICC05modelext, EDML-synchronized NGRIP chronology and our new evaluation of GS-22 duration. The limited detail provided by Capron et al. (2010) reduce the extent to which we can pinpoint the sources of error, although we note that the tie points for the GS-22 onset are poorly constrained whereas the tie point for the GI-21 onset is tightly constrained. This points at least to an underestimated uncertainty in the EDML1 chronology at the GI-21 onset (83578 y BP).

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

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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 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). 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.

>The abstract has been updated to include the ranges of both Dage estimates. The term 'slightly longer' has been removed from the text - we agree with the referee that the two independent Dage calculations are consistent within their uncertainties. We do not assume that the two warming events produced similar firn closeoff conditions, for this reason we avoid pooling the results of the two independent calculations. Instead we are content to demonstrate the maximum ranges, for the benefit of the community members who may wish to use the data for their models.

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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?

>Table 1 has been updated so that 1-sigma uncertainties are shown consistently. We indicate in the footnotes where we have adapted the 2-sigma uncertainties reported for NALPS. We have updated the column headers to '+/- 1-sigma' and renamed 'GI-22 end' to 'GS-22 onset'. The first column has been retitled 'chronology' and the data sets have been labelled more accurately.

Fig 4 caption: Horizontal numbers indicate [age] differences

>The figure caption has been updated.

>Also, we have identified an error in fig.2 (now fig.3) - the uncertainty of the NGRIP-EDML duration of GS-22 was mistakenly written as +/- 300 years. This has now been corrected to +/-325 years.

Referee #2 (<http://www.clim-past-discuss.net/8/C1486/2012>) We welcome the favorable comments from the referee. Our responses are described below:

1. I would recommend moving figure 3 up to the introduction section. It fits very well with the discussion that starts on page 2586, and would provide further clarity for the background. 2. All climatic events should be better identified for clarity in figure 3.

>We have referred to Figure 3 in the introduction (hence Figure 3 is now Figure 1), and

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included shaded areas and text to better identify the climate events presented.

3. page 2590. I would recommend some more discussion on why the sodium and ammonium signals have been smoothed by diffusion, while the conductivity signal apparently was not.

>We have expanded our discussion of the smoothing of sodium and ammonium signals considered in respect to annual cycles in electrolytic conductivity. 4. Page 2592, lines 6–8. Give citations for these assertions.

>An appropriate citation (Capron et al, 2010b) has been added.

5. page 2592 line 13. Modify to: “reported that impurity content also plays” 6. page 2593, line 5. Modify to: “the start of the d18Oice increase”

>Both changed as suggested

7. It appears to me that the EDML-based estimate of the duration of Greenland Stadial 22 has many more sources of uncertainty than the estimate presented in this work. I would recommend more discussion of this and a more forceful statement in the manuscript to the effect that the presented estimate is more reliable than the estimate of Capron et al.

>Following similar comments from referee 1, section 4.2 has been rewritten to discuss the assumptions and sources of error in the chronology reported by Capron et al. 8. Figure 4. Horizontal depth differences on the figure should be put into the units of meters, to be consistent with the x-axis. It should also be clarified on the figure that the peaks represent the onset of GI-21

>The figure has been updated: GI-21 and GS-22 are labelled and the caption has been corrected to refer to horizontal text as ages, not depths.

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/8/C1611/2012/cpd-8-C1611-2012-supplement.pdf>

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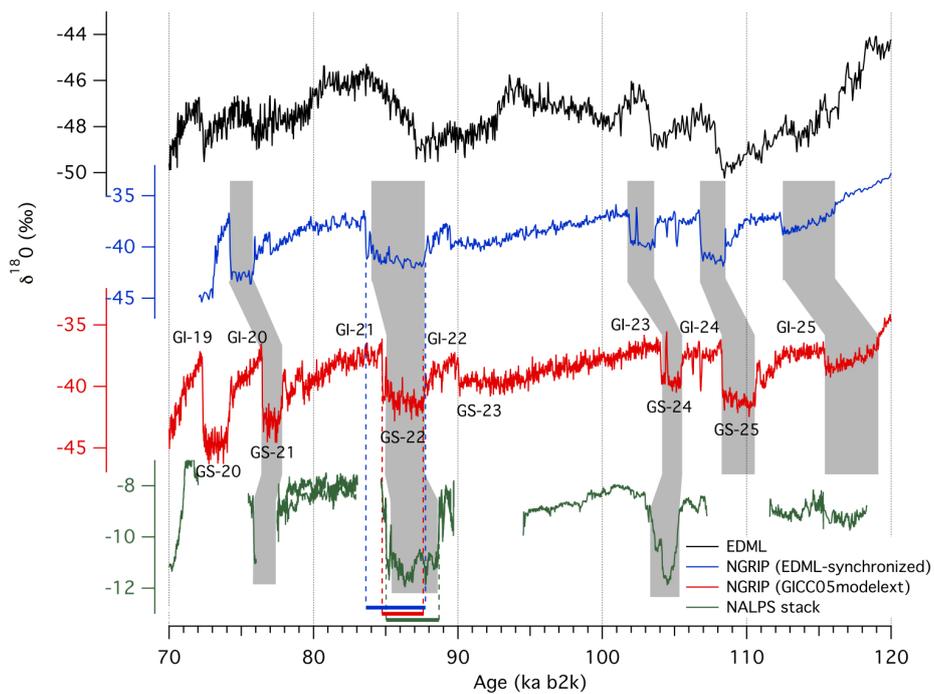


Fig. 1. Comparison of temperature proxy records from Greenland, Europe and Antarctica. The Antarctic EPICA Dronning Maud Land (EDML) ice core $\delta^{18}\text{O}_{\text{ice}}$ record is shown in black (EPICA community members, 2006).

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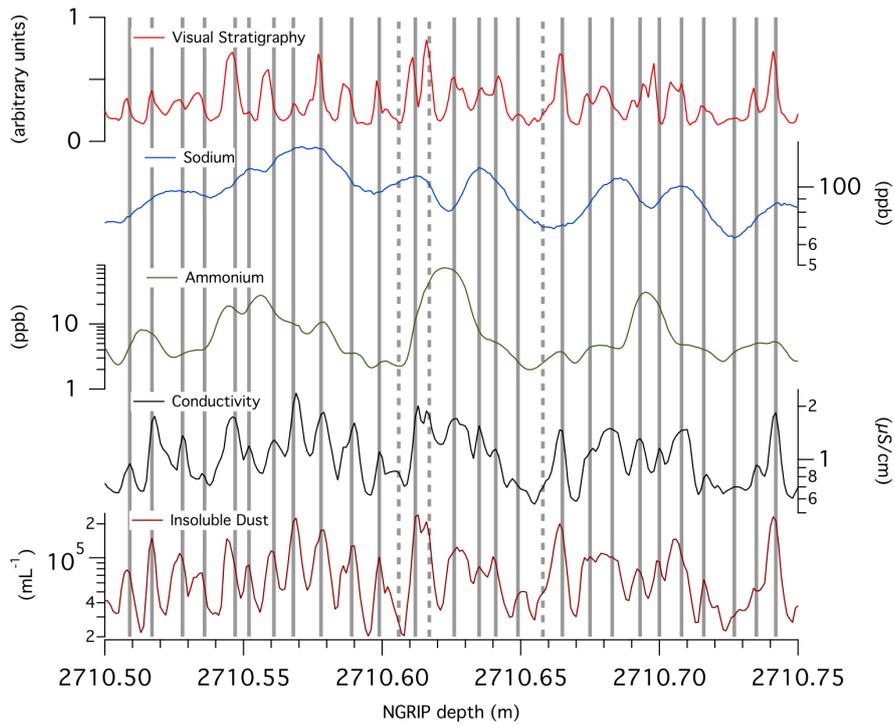


Fig. 2. Example of Continuous Flow Analysis (CFA) measurements of NorthGRIP glacial ice and assignment of annual layers. Visual stratigraphy was measured by optical scanning at the drilling site, not by CFA.

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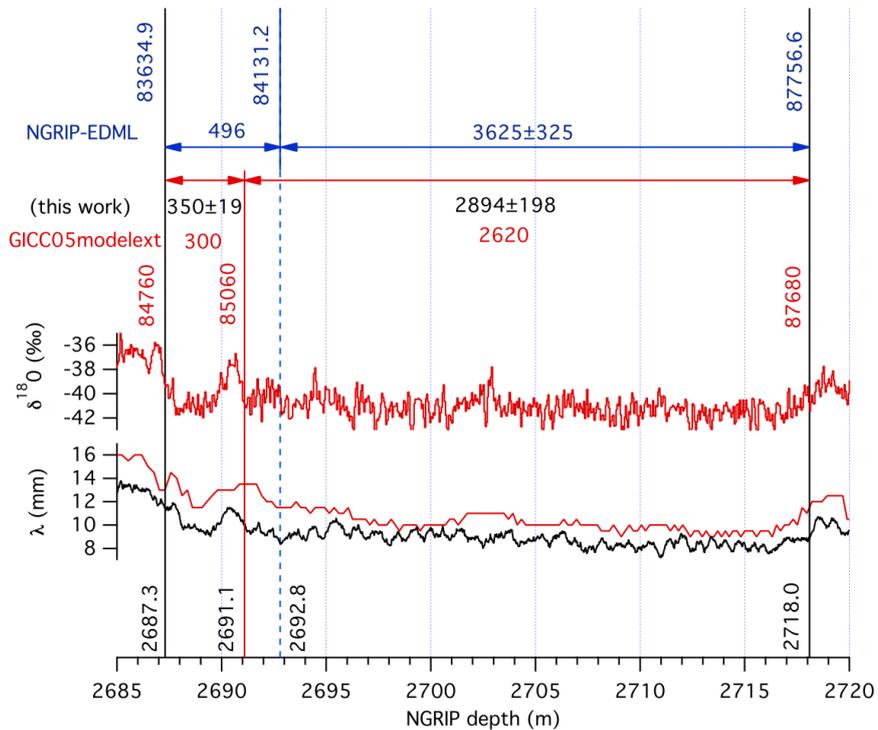


Fig. 3. Comparison of NorthGRIP ice core chronologies over the interval between GI-21 and GI-22. Also shown are NGRIP $\delta^{18}\text{O}_{\text{ice}}$ and annual layer thicknesses (λ) from the ss09sea06bm ice-flow model and from ann

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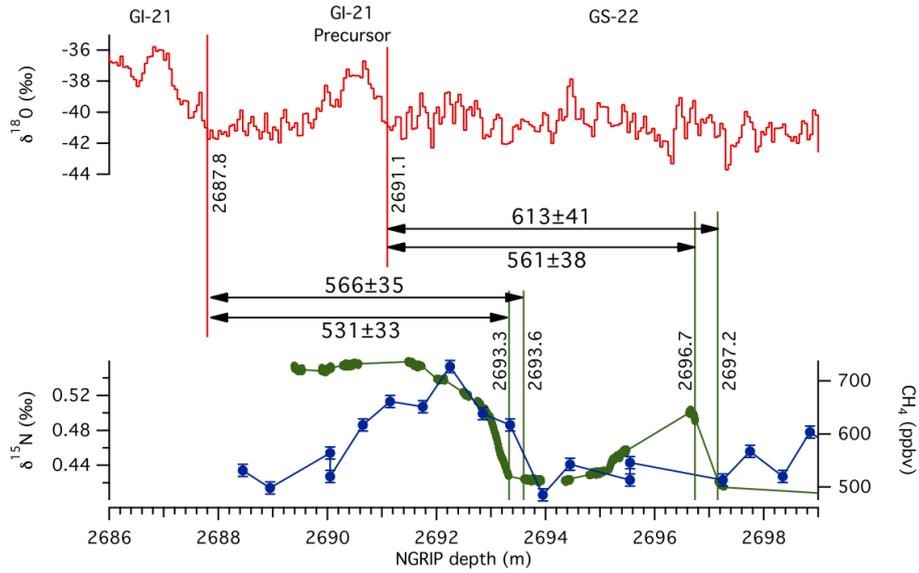


Fig. 4. Comparison of NorthGRIP ice core $\delta^{18}\text{O}_{\text{ice}}$ and trapped gas species for the determination of Δage . $\delta^{18}\text{O}_{\text{ice}}$ is shown in red [GICC05modelext chronology, Wolff et al. (2010)] while $\delta^{15}\text{N}$ is shown in blue (Ca

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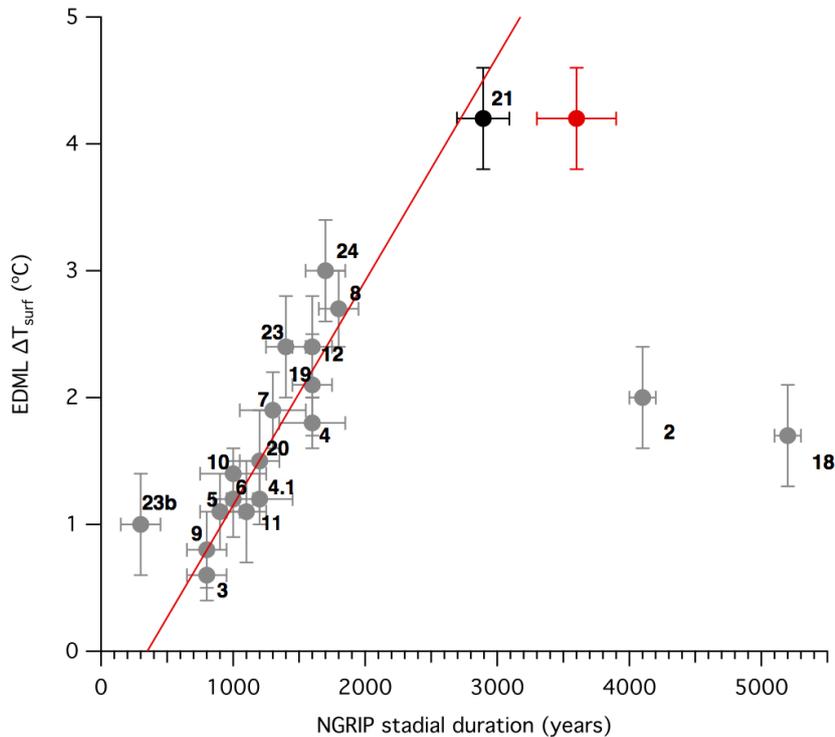


Fig. 5. Comparison of NGRIP stadal duration and EDML temperature change, modified from Capron et al. (2010a). Numbers indicate corresponding GI and AIM events, where the stadal duration is determined from t

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