TALDICE-1 age scale of the Talos Dome deep ice core, East Antarctica" by D. Buiron et al. (CP2010-59)

❖ Response to referee 2:

General Comments:

1. This manuscript presents an age scale for the Talos Dome ice core produced via an inverse method constrained by a range of tie-points. As this is the first age scale for this core it represents a valid contribution to Climate of the Past. This age scale, along with future refinements will contribute to our understanding of past glacial cycles. The manuscript will be suitable for publication once the corrections and alterations suggested here and elsewhere have been made to the satisfaction of the editor.

2. It is strongly recommended the authors work on improving the clarity of the language. The grammar and word selection throughout the manuscript is somewhat idiosyncratic. I suspect this may be due to English not being the first language of the lead author. This is not a criticism as I have the utmost respect for any author not writing in their native language, however I did find sections of the manuscript difficult to read. This may be a matter of personal taste, but I think the manuscript would be greatly improved by some finessing of the grammar.
   Many of the sentences are far too long and could be broken into several shorter sentences. Removal of redundant words will reduce sentence length and increase clarity. Improvements to the punctuation will also assist the reader. A number of specific suggestions have been made in this review, but many opportunities for further improvement remain and are left to the authors.

   We thank the reviewer for his indulgence regarding our English writing. We tried to improve it in many places, and the paper has now been read by an American researcher who provided additional corrections.

3. In section 5.2.2 regarding the thinning function the literature review is incomplete. A broader consideration of the literature dealing with the development of polycrystalline anisotropy and its influence on ice rheology is essential and should be included prior to publication. Further detailed comments relating to changes or improvements required in this section are presented below.

Detailed Comments:

1734, line 19: Replace '...to the one observed...' with '...to that observed...'.ok

1735, line 18: The statement 'at 5km from the peripheral dome of Talos Dome' is confusing. I think the intended meaning here is that the core was drilled 5 km from the Talos Dome summit? Yes . If so please rewrite as currently there is a suggestion the core was drilled on some secondary dome feature that is distinct from Talos Dome summit. OK

1735, line 20: A figure indicating the location of the Talos Dome drilling site would provide some

regional context for the reader.

There are already 8 figures in the paper and all papers relating to ice cores do not necessarily show a corresponding map of the location of the core. Besides some maps showing Talos Dome location have already been published in several papers, such as Frezzotti et al. 2004, Stenni et al. 2010 (Nature Geoscience of 5 december).

1736, line 1: The bibliographic entry for Stenni et al. (2010) should be updated if this article is now in press, otherwise the citation is not valid. This comment applies to all instances of this citation.

it is published now. We changed the reference.

1736, line 4: East Antarctic does not require hyphenation.ok

1736, line 5: Remove 'directly'. ok

1736, line 15: Remove the opening parenthesis before 'Kawamura...'. ok

1736, line 16: An example of a sentence that could be greatly improved by editing (there are many others).

'Air trapped in polar ice cores have the unique property of containing global tracers of the atmosphere such as CH4 and δ18Oatm that globally display the same temporal variations on the different drilling sites' could (for example) be changed to 'Air trapped within polar ice contains global atmospheric tracers such as CH4 and δ18Oatm that display temporal variability between different drilling sites'. Some references supporting this statement should be provided. ok

1736, line 24: This sentence needs to rewritten. It is unclear how something that is missing is incorrectly identified. ok

1736, line 27: This sentence is very long and should be rewritten to improve clarity and explain why the GICC05 and EDC3 age scales were chosen, or at least mention that the reason for choosing these scales will be discussed later. ok

1737, line 9: This sentence needs to be rewritten. At the very least replace 'underlined' with 'illustrated'. ok

1737, line 13: Replace 'carrying' with 'conducting'. ok

1738, line 7: Replace 'of both hemispheres' with 'from either hemisphere'. ok

1738, lines 11-13: References supporting this statement should be provided. ok

1738, line 23: Replace 'will be' with 'are'. ok

1739, line 5: Insert a full stop after the citation. ok

1739, lines 5-6: I'm not familiar with methane measurement techniques; is there a justification for increasing the LGGE values by 6 ppbv?

The main reason lies in the difficulty to estimate the contamination introduced by the gas extraction protocol. 6 ppbv represents the average difference over hundreds of contiguous measurements performed by both laboratories on EPICA ice cores. It is explained in Spahni et al. (2005).

1739, line 9: If the mean sampling interval is 2m and the minimum is 0.5 m, what is the maximum sampling interval? Or is it that the samples are largely obtained at regular 2m intervals.

We add the maximum resolution value.

1739, lines 24-25: It is not clear what is meant here regarding duplicate/repeat analyses; please clarify. Done

1740, line 3: In the discussion of CH4 measurements 1σ errors are reported, whilst here for δ18 Oatm measurement errors of 2σ are reported. I would suggest selecting a single bound for error estimates and applying it consistently for all measurements throughout the manuscript, unless there are clear reasons to not do so. ok

1740, line 26: Replace 'forty-four' with '44' here and elsewhere. ok

1741, line 1: Replace 'fourteen' with '14' here and elsewhere. ok

1741, line 12: What is the uncertainty in visual matching and how is it assessed or quantified?

The visual matching uncertainty is determined by shifting the x-axis of the TALDICE dataset with respect to the x-axis of the reference dataset until there is no more match within the error bars of the measurements and taking into account a possible interpolar gradient during the CH_4 transitions. Such way of determining the uncertainty covers both sources of errors due to measurement uncertainties, interpolar gradient and resolution. For instance if the resolution is loose over a rapid Dansgaard/Oeschger transition, a more important shift between the two x-axes will be possible. Based on Huber et al. (EPSL, 2006), the methane transition associated with each D/O event during MIS3 has a typical duration of 200 to 350 yr. The typical time resolution of our TALDICE CH_4 measurements over these same transitions lies between 60 and 300 yr. Therefore we are confident that we pick up well the main structure of each D/O transition in the methane signal, and that the uncertainty mostly reflects the tolerance in the visual matching, which is limited by measurement errors and our poor knowledge of the interpolar gradient. We added in parenthesis a short explanation on the procedure.

1741, line 23: Replace 'fifty-eight' with '58'. ok

1742, line 2: Remove '(ie)', this abbreviation for ice equivalent has been defined previously. ok

1742, line 7: Replace 'air bubbles' with 'discrete air bubbles'. ok

1743, line 6: What recent studies? We modified this section

1743, line 7: Where does the unpublished data come from? This section would be more convincing if the data were included or it was some way able to be cited. We changed the reference.

1743, line 12: Replace '(cm of ice equivalent per year)' with (cm ie/yr) as defined previously. ok

1743, line 19: The quantities p and ΔH are defined in Table 4 but should also be defined here. ok

1744, line 10: No capitals required in 'East'. ok

1744, line 12: Replace 'was favoured by' with 'resulted from' and rewrite the remainder? ok

1744, line 25: What is the counterpart of Talos Dome on the East Antarctic plateau? If the point of this sentence is that recent evolution of Talos Dome elevation is distinctly different from neighbouring regions of East Antarctic plateau it should be rewritten as the suggestion that there is a counterpart to Talos Dome on the East Antarctic plateau is misleading. We rewrote this misleading sentence.

1744, line 27: Replace 'Glacial geology observation' with something like 'Glaciogeomorphology' or 'Observations of glacial geology' ok

1745, line 1: This sentence is rather clunky. Something like 'the present dome elevation is similar to that 30 ka BP and passed through an intermediate maximum around 16 ka BP' would be better. ok

1745, line 8: Replace '30 000 yr' with '30 kyr'. ok

1745, line 10: This sentence is difficult to understand. I suggest something like: 'Due to numerous poorly-defined parameters and the simple description of ice flow, which does not account for the effect of anisotropy on rheology, the 1-D model is not able to describe past migrations of the dome summit or changes to its lateral limits which induces errors in the thinning function scenario. A couple of shorter sentences would be even better. No new paragraph is required here. ok

1745, line l3: Replace 'Besides' with 'The model'. ok

1745, line 23: Remove '(expressed in ice equivalent (ie))'. This has already been defined.

Ok

1745, line 25: Should there some reference to the past or previous CODIE, rather than just the CODIE? This is a minor point, but I would have thought there is only one current CODIE at any

location where an ice core is recovered. As this sentence describes parameters which vary with depth, estimates of the CODIE along the length of the core are thus estimates of the past CODIE (at an age related to the depth). Perhaps I missed something? The reviewer is correct. We changed it with "that vary in the past".

1746, line 8: Remove parentheses around the citation. ok

1746, line 16: Remove parentheses around the citation. ok

1747, line 13: There is a reference to a 'D/O' event here and on 1748, line 14 a 'DO' event. I assume these are same? If so please use a consistent abbreviation. ok

1747, line 20: This sentence should be rewritten to provide more detail and clarity. ok

1748, line 26: The sentence beginning with 'The inverse ...' could be much clearer. ok

1749, line 12: The sentence beginning with 'As the dust source ...' would be easier to understand if it was rewritten so that Patagonia was not in parentheses. We suppressed the full section, as requested by reviewer 1.

1749, line 19: The discussion of sulphate peak matching is unclear and needs revision. Perhaps I've misunderstood something but how can one of three peaks (all from the same depth of 766.09 m) be from a different depth to the other two? If the other two peaks are from different depths are there corresponding peaks in the EDC record and how well do they match those in TALDICE? We suppressed the full section, as requested by reviewer 1.

1750, line 15: An additional cross reference to Figure 5 in this sentence would be useful. ok

1752, line 6: The citation 'Scarcilli et al., 2010', should be updated if this article is now in press. ok

1753, lines 3-9: Two possibilities regarding the Holocene/LGM accumuation rate ratio are presented with the suggestion that either the LGM accumulation rate determined from the inverse method is incorrect or other factors have influenced the TALDICE LGM/Holocene [10 Be] ratio. Some discussion of which of these situations is more likely would be useful as it relates directly to assessing the quality of glaciological parameters derived via the inverse method.

At the moment, we do not have a firm argument in favor of one or the other possibility. It is up to the future TALDICE-2 chronology to bring new constraints allowing us to better evaluate the relative adjustments required to both parameters.

1753, line 10: Section 5.2.2 Thinning Function: The discussion of the current and past deformation regime at Talos Dome, the influence of polycrystalline anisotropy on dynamics, presentation and interpretation of crystal orientation fabric and its connection to the thinning function all require attention. The review of literature relevant to these processes is also inadequate.

As an introductive response to reviewer 2 on the thinning function discussion, we want to stress here that the objective of the TALDICE-1 chronological paper is not to fully discuss the glaciological implication of changes in fabric evolution with depth along the TALDICE core. These changes are only shown here to illustrate the coincidence between thinning function changes required by the solution from the inverse model on the one hand, and fabric evolution on the other hand. We agree with him that many aspects require in depth discussion. It will be the purpose of a separate paper led by M. Montagnat, being currently finalized.

As a consequence, several responses to the reviewer's remarks below will refer to this separate paper.

1753, lines 13-17: Discussion of the location of the dome and possible variations in the flow regime over time would be assisted by a figure indicating the surface strain rates measured at the drill site. Presentation of the surface strain rates will also assist interpretation of the crystal orientation fabric data. What is the surface slope at the coring site? From the differences in ice thickness at the Talos Dome summit and ID1 coring site, which is on something of a divide (Urbini et al., 2006), I would expect the deformation regime to vary from uniaxial compression. This is an aspect which is discussed in the paper by M. Montagnat et al. currently being finalized.

Castelnau et al. (1998) indicate that significant horizontal shear strain rates can exist beneath an ice divide. A more complete presentation of the crystal orientation fabric data would assist here (more on this later. This is an aspect which is discussed in more detail in the paper by M. Montagnat et al. currently being finalized. But proof of shear is already appearing on the eigenvalues represented here and is discussed in the text.

1753, lines 22-24: There is considerable earlier work from both field and laboratory studies that illustrate the links between crystal orientation fabrics and deformation which should be discussed. Some examples include (there are many others) : Kamb (1972); Budd (1972); Gow and Williamson (1976); Russell-Head and Budd (1979); Bouchez and Duval (1982); Thwaites et al. (1984); Dahl-Jensen and Gundestrup (1987); Gao and Jacka (1987); Thorsteinsson et al. (1997); Gow and Engelhardt (2000); Diprinzio et al. (2005). Also see the reviews in Budd and Jacka (1989) and Cuffey and Paterson (2010). I would have thought reference to Durand et al. (2007) rather than Durand et al. (2006) would be more appropriate here? We added several references to give better credit to previous studies. Reference to Durand et al. 2006 is linked with the interest of the representation of fabrics through eigenvalue of orientation tensor. The 2007 paper for the analyses by itself.

1753, line 24: This is minor item. Rewrite this sentence so that it does not begin with 'C-axes'. The crystallographic c-axis is by convention indicated by a lower case c. Also see Budd (1972) and Alley (1992) for a discussion of grain (c-axis) rotation relative to applied stress directions. Ok (ref)

1753, line 26: Given that the links between polycrystalline anisotropy and ice flow are discussed at 1753, lines 22-24 is this sentence necessary? Yes it is, as it insists on the reverse relationship, i.e. that the development of anisotropy affects the ice flow.

1754, line 2: How were the crystal orientation fabrics measured? What instrument was used? From where in cores were the thin sections obtained? Were they vertical or horizontal thin sections or a mixture?). A paragraph was added on the methodology of fabric measurements.

Unless the status of Montagnat (in preparation) has changed this is not an appropriate citation. Similar comments apply to 1754, line 6: Montagnat et al, 2010 and 1754, line 18: Montagnat, n.d.....I think these are all the same manuscript in preparation? Yes it is. To our knowledge, it is correct with Climate of the Past and many other journals to make reference to a manuscript being currently finalized.

1754, lines 3-9: I think **Woodcock (1977) should be cited in this discussion** of the second order orientation tensor and interpretation of its eigenvalues. The discussion of eigenvalues is incomplete in its current form.

In Figure 7 the range of values for a1 is $0 \rightarrow 1$ which suggests these are normalised eigenvalues so that $a1 + a2 + a3 = 1$. If this is case the **comments on 1754, line 8 are incorrect** and should be rewritten. Ok

As noted in Woodcock (1977) and Durand et al. (2006), for a strong single maximum fabric a1

≥ a2 ≈ a3 and for an isotropic fabric ai = 1/3. The fabric data presented in **Figure 7 is incomplete without a2 and a3**. I suggest that it is included. This could make Figure 7 quite cluttered so the authors may like to reconsider how the fabric data and thinning functions are presented. Perhaps two figures are required? Figure 7 could also benefit from an **additional vertical scale on the right hand side indicating age**.

This is an aspect which is discussed in more detail in the paper by M. Montagnat et al. currently being finalized. Again here, the aim is only to illustrate that changes in the thinning function required by the solution from the inverse model find similarities with changes in the a1 slope.

Whilst fabrics below ~ 900 m are clearly very strongly clustered presentation of a2 and a3 would indicate the level of transverse isotropy in the fabrics. Presentation of all ai will be even more useful in the interpretation of fabric data and deformation regime from above 900 m where a1 values are lower. The fabric patterns, as indicated by ai (not just a1)) give a direct indication of the flow regime and therefore what level of thinning may be expected. In general I would say that the current presentation of fabric data and its interpretation could be improved. Same response as above

1754, lines 12-14: How were changes in the slope of the fabric evolution curve determined? To my eye the rate of fabric evolution (from only a1) looks similar, but noisy from 700 m down to almost 950 m. Higher resolution fabric data (i.e. samples every 5-10 m) would really assist in determining where the real transitions in fabric strength occur and what is just noise. Again, presentation of all ai , not just a1 would greatly assist interpreting trends in fabric development and their relation to the thinning function which are discussed from 1754, line 12 to 1755, line 3.

We revised the whole discussion about changes in a1 slope changes. Broader changes are considered now. Higher resolution fabric data are envisaged in a short future.

1754, lines 15-23: See comment above regarding changes in fabric evolution from 700 m to 950 m. I don't see a significant change in fabric development over the depth range where there is supposed to be a higher dust concentration. Perhaps the orientation tensor values as a function of depth could be plotted along with dust concentration and/or measured grain sizes. Some references to the effect of particles on pinning grain boundaries are required (e.g. Alley et al. (1986a,b) - there are others). In addition to Durand et al. (2007) there are many examples from Antarctic and Greenland ice cores where dust concentrations are linked to grain size control (e.g. Gow and Williamson (1976); Li et al. (1998); Gow and Meese (2007) etc). If it was intended to make the point that there are regions of the EDC and Talos Dome cores where dust has exerted some control over grain size and these regions occur at corresponding ages in both cores it should be made more clearly. The statement 'Smaller grains lead to a change in ice rheology and ice viscosity (Cuffey et al., 2000)' is perhaps stronger than Cuffey et al. (2000) intended. My impression from this article is that the jury is still out. Cuffey et al. (2000) describe grain size as a residual lower-order effect on strain rates that is smaller than that of polycrystalline anisotropy. The statement 'Smaller grains lead to a change in ice rheology and ice viscosity...' is also slightly tautological as a change in viscosity is implied by a change in rheology (or vice versa). Some Schmid plots of c-axes and some grain size data would really assist the authors in illustrating changes in fabric strength and

grain size that are suggested to occur in the 800 m to 900 m depth range.

We rewrote entirely the section discussing the a1 tensor evolution. The new version does not include any discussion of grain size and its possible association with a pinning effect due to dust concentration. We leave this specific discussion on what could cause the a1 slope changes to the article of M. Montagnat et al. currently being finalized. Again here, our aim is to show a co-variation with depth between an indicator related with the ice deformation and the thinning function provided by the inverse model, indicating that slow changes in the latter are plausible. The mechanistic link between both is clearly beyond the scope of our paper.

1754, lines 24-25: Is there a depth range missing here? This sentence should be rewritten to improve clarity. ok

1754, line 26-27: To me it looks like the change occurs between 1150 m and 1200 m, but there is a gap in fabric data between these depths. Higher resolution fabric data would be useful to indicate whether this is a gradual or step change.

Yes, this is the range of depths where the change occurs. The two steps of stable fabric evolution are clearly separated by this depth range of changing slope, although as stated by the reviewer, a higher resolution data set would reinforce this observation.

1755, lines 1-3: The statement 'A maximum of fabric concentration could be reached at this point' is vague. Are these the strongest measured fabrics? If so, I think such a statement is fine, however to me it looks like the strongest fabrics occur from 1250 m to 1400 m. This part has been rewritten.

As noted previously I would be more convinced by higher resolution fabric data. This would give the reader greater confidence in statement regarding the occurrence of maximum fabric concentrations.

Between 1200 m and 1400 m the fabrics are very concentrated, and stable enough to think that a higher resolution data set would not show it better... And yes, the a1 values measured are particularly high for fabric evolution along ice cores.

1755, lines 5-8: If a 3-D ice flow model is required to adequately link anisotropy and thinning why not present all eigenvalues of the orientation tensor as this would assist in illustrating the complexity of the flow regime and its variation with depth?

The aim of the comparison between fabric measurements and the thinning function was mainly to show that both signals, resulting from two very different approaches, show similar trends. Presenting the full fabric dataset would require deeper explanations, clearly beyond the scope of the paper. Such a detailed work is being carried on in the paper led by M. Montagnat, currently being finalized, discussing in depth the fabric evolution and modeling along TD ice core.

In addition to Durand et al. (2006) (do the authors actually mean Durand et al. (2007) here?) and Gillet-Chaulet et al. (2006) there are other examples of 3-D ice flow relations incorporating the effects of anisotropy that could be applied to examining the links between fabrics and thinning (e.g. Azuma and Goto-Azuma (1996); Thorsteinsson (2002); Seddik et al. (2008); Greve et al.

(2009)). Also see the reviews of Placidi et al. (2006) and Gagliardini et al. (2009).

This section has been entirely removed in the revised manuscript.

1755, lines 11-13: This sentence is very long and hard to follow. It could be broken into 2 or 3 shorter sentences. ok

1755, line 20: Is Buiron et al., 2010 still in preparation? If so I don't think this is a valid citation. ok