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

❖ Response to referee 1:

The paper presents an age model for the Talos Dome ice core based on inverse modelling along the lines of Lemieux-Dudon et al., 2009. Roughly speaking, the inverse model seeks to adjust the flow, accumulation and Δage scenarios derived from simpler a priori model results so that the resulting gas and ice age scales agree optimally well with a set of chronological marker points that can represent ties between several cores being age-modelled at the same time (depth-depth ties) or independently dated horizons that are applied to the cores in question (age ties).

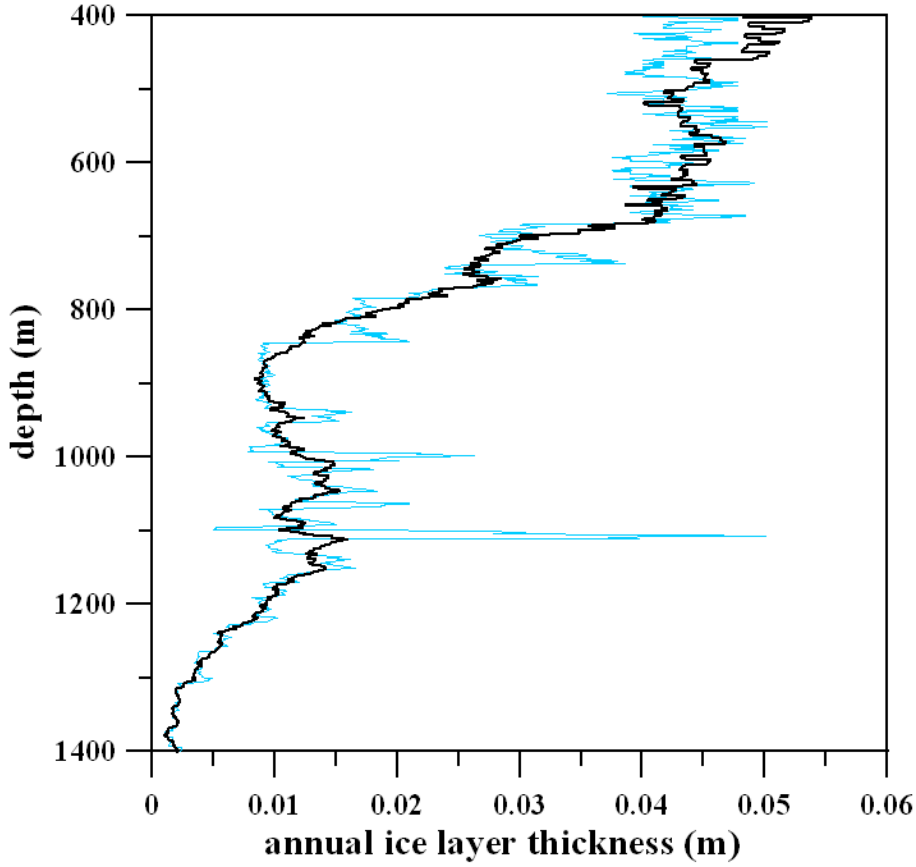
In my opinion, there are three main potentials for added value by the inverse modelling method: 1. The model allows parallel and consistent dating of several cores (if the tie points are correct). 2. Both gas and ice tie points can be used, also in the case where the tie points are not fully internally consistent due to e.g. uncertainties. 3. On top of the age model itself, the model produces a consistent set of Δage, accumulation and thinning functions.

The application of the inverse model in this work is a simple version of the Lemieux-Dudon work, because only one core is being modelled (i.e. there are only age ties and no depth-depth ties), and because only gas-age ties are used for the young part and only ice age ties age used for the old part of the record. The advantage of using the model in this case is therefore almost entirely reduced to point (3) above.

The statement above from the reviewer minors the importance of point (3). Indeed point (3) is a major advantage of the inverse model to produce a suitable chronology. Contrary to simple interpolations which have been widely used in previous work, the chronological solution provided by the model bears a glaciological meaning and avoids unrealistic stretching-squeezing of the depth-age relationship both in the gas and ice phases. Point (1) is clearly a long-term goal when applying the inverse model, and beyond the scope of our paper.

I know this will sound provocative, but I would like to see a calculation / graph of how much the inverse model differs from a simple interpolation between the tie points (either linear interpolation depth vs. depth or linear interpolation of annual layer thicknesses), i.e. inverse model age minus interpolated age vs. depth. I have the feeling that the whole thing relies almost entirely on the tie points, and that one would get almost the same age model without applying the inverse model. If the differences (also in between tie points) are small compared to the uncertainties of the tie points, the inverse model does not add much value in this regard.

We thank the reviewer for this useful suggestion. The following figure A shows the added-value of using the inverse method: it represents the evolution with depth of the annual layer thickness along the core using both the inverse method (purple line) and a linear interpolation between gas stratigraphic markers (blue line). It is obvious from this graph that the linear interpolation method provides unrealistic features which disappear with the current optimal run provided by the inverse method.



With regard to point (3) above, the consistent set of Δage, accumulation and thinning functions are valuable and may in themselves justify the use of the inverse model, but I'm not sure how well-constrained they are when only one core is being modelled and only gas age ties are used in the top and only ice age ties are used in the old part.

The fact that the inverse model is able to find a consistent set of glaciological parameters able to match the stratigraphic markers, whereas the use of the 1D glaciological model combined with the densification model does not, speaks for itself. An alternative would be to use the second solution in a trial-and-error mode, testing hundreds of possible sets until eventually a suitable solution is found. The beauty of the inverse model is to provide an efficient alternative to such cumbersome approach. In addition it provides quantified uncertainties. The inverse solution presented with TALDICE-1 may not be the only combination of Δage, accumulation and thinning functions able to match the stratigraphic markers. Improvement will come in the future with more tie points such as the use of the Laschamp 10Be event. But at the moment, TALDICE-1 is clearly superior to both a direct approach with a 1D glaciological model and a simple interpolation between tie points.

To summarize, I think the inverse model is almost overkill in this simple case. Provided that the differences between the inverse model and a simple interpolation are indeed small, the result of whole setup is an advanced transferral of the selected GICC05 and EDC time scales to the Talos Dome core using the visually selected tie points. There is nothing wrong with this, but the manuscript should reflect this and focus on the value of point (3) above.

The new graph shown above and now incorporated in the manuscript hopefully convinces the reviewer that we are not using an overkill approach here.

All this being said, I am confident that the presented age model is robust, and in this regard, the conclusions are justified.

Thank you!

I acknowledge that the manuscript documents the dating methodology carefully, which is an important contribution, albeit it to some degree has the character of a technical report. I therefore recommend publication after the authors have satisfactorily dealt with the issues raised in the review. In line with the comments above, I encourage the authors to focus on point (3) above and if possible include more discussion of the results of the age model and the dated Talos Dome record and it's relation to other records.

In the revised manuscript, we insist now more on the benefit of point (3) regarding the use of the inverse method. On the other hand, we do not consider that the scope of this paper should also be to discuss the relation between the Talos Dome climate record and those from other ice cores for instance. Other papers are focusing on this important aspect of the TALDICE project, such as Stenni et al. which just appeared in Nature Geoscience, discussing the phase relationship between Talos Dome and other sites during the last deglaciation.

Detailed comments –– 1734. 16: Bølling and Allerød should be spelled with ø.
16: The abrupt warming is at 14.6 ka BP (14692 b2k is 14642 bp1950).
16-17: The text should reflect that the dates given are for the onsets of the named periods, not the periods themselves. Also, the onset at 14.6 ka BP is the onset of Bølling, not Bølling-Allerød, as Allerød only starts some 700 years later (Lowe et al, INTIMATE protocol QSR 2008).
18: Severinghaus et al., 1998, sets the onset of the Holocene to 11.6 ka BP1950.

All points above done.

15-18: Consistency would increase if the dates of Greenland transitions where taken from one source, e.g. the GICC05 time scale.

We now refer to GICC05 for all dates

22: Only a few hundred years? Which record is that?

It was a wrong statement. We changed the numbers.

–– 1735. First paragraph: mention that some of these differences could be related to dating. Done.
–– 1736. 24: "Due to incorrect identification of missing seasonal signals and absence of absolute volcanic chronology before 1000 AD". Please clarify how one incorrectly identifies missing seasonal signals . . . or rewrite.

The sentence was misleading; we rewrote it.

–– 1738. 11: The authors may want to address the implications (and if possible, the magnitude) on this study of the concerns raised by Köhler recently (Clim. Past Discuss., 6, 1453-1471,2010).

We added a paragraph related to Köhler's study.

16: Add reference to Rasmussen et al, 2006 (JGR), which describes the dating in the interval 8-15 ka. The Vinther et al., 2006 (JGR), paper describes dating of the section above 8 ka, which is not used much here, but could be added for completeness. Done

20: Which EDC3 age model is used? The one with modifications by Lemieux-Dudon et al., 2009, or the original EDC3?

The original EDC 3 age scale is used because the one improved by Lemieux-Dudon et al. 2009 is relevant only for ages younger than 50 kyr BP.

And if not the first of these, why, given that this time scale is expected to be more consistent with GICC05 and the modelling approach applied? In general, the paper would benefit from consistent use of one or the other EDC time scale and a discussion of which one is considered superior.

See previous response.

—- 1741. 12: It is not described how the authors estimate the uncertainty of the visual matching. This is an important shortcoming given that the result depends critically on these ties. Some kind of statistical modelling would be welcome to support the estimates, and the authors should at least describe in detail how they evaluate the synchronization uncertainty and how this value compares to the (variable) CH4 data resolution. The different uncertainty contributions (esp. for the CH4 tie points) should be listed individually as well as combined for each tie point.

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.

14: The GICC05 uncertainties are not included in the error estimates of table 1, and I doubt they are for the EDC-derived tie points, either. This could be a perfectly reasonable approach (especially if the EDC age model used is the Lemieux-Dudon et al. version), with the resulting TALDICE model being a trade-off between a match to GICC05, a match to the used EDC age model, and the glaciological constraints. This would, I believe, follow the approach by Lemieux-Dudon, and imply that the TALDICE age model inherits any possible errors in the GICC05 time scale and EDC age model. This is also suggested in line 21 on page 1755, but contradicts the text here.

Yes, the GICC05 uncertainties are not included in the error estimates, as the TALDICE-1 chronology has not the pretention to be an absolute one. It is a relative chronology with respect to GICC05 and to EDC3. We therefore changed the sentence which was misleading, and we added another sentence.

—- 1743. The authors could discuss here whether the chosen accumulation rate and temperature parameterization realistically can capture both variations in A and T on short timescales and across glacial-interglacial transitions. I am aware that the accumulation rate is a free parameter of the more advanced inverse model of section 4, but deviations from the simple (background) model scenario are penalized (or at least, I guess they are, as described in Lemieux-Dudon et al., 2009), and it is thus essential that the a priori estimate is essentially correct.

The deviation from the simple model scenario depends on the correlation length and error parameters chosen for the inverse simulation and associated with each of the three glaciological parameters: the correlation length parameter represents the maximum interval on which the variations of the glaciological parameters can be smoothed. In absolute, there is no limit in the model for changing the reconstructions of these parameters from the direct scenario, but the probability of a suitable solution very different from the 1-D model parameters becomes lower when large corrections are required. Sensitivity tests investigating and quantifying all possible scenarios for the past evolution of the glaciological parameters would be very useful but will be the subject of further studies.

19: The parameters p and ΔH have not been introduced at this point. Elevation changes are discussed in section 3.2, but are not referred to as ΔH in the text.

We modified the text and introduced these parameters earlier in.

—- 1745. 3: Explain how/if the elevation changes are used in the full model and if the δD values are corrected accordingly before used for determination of past accumulation and/or temperatures.

Yes they are taken into account; we added a sentence to explain this.

—- 1746. 10: "(further studies are necessary to test this latter assumption)" is a strong understatement that would benefit from some qualified comments by the authors.

The relationship between the CODIE and climatic parameters is complex : a lower temperature leads to a deeper CODIE whereas a lower accumulation rate leads to a shallower one (see for instance Landais et al., QSR 2006). Therefore we cannot state at first glance that the CODIE and accumulation rate should be related, in a linear or non-linear way, as the accumulation rate also depends on temperature which has the opposite effect on the CODIE. A realistic estimate of this relationship would require to perform many tests with the model, which was not the objective here. We had to make assumptions in order to reduce the range of scenarios evaluated with the inverse model. The sentence has been slightly modified to specify this point.

Besides the a posteriori control presented in the paper, where the inverse accumulation rate is used as input parameter for a direct simulation with the firn densification model of Goujon et al (2003), provides a Δage and thus a CODIE estimate in very good agreement with the one calculated by the inverse model. This shows that the CODIE estimated by the inverse method is realistic even if it is not assumed to be directly correlated with changes in the accumulation rate.

Sec. 4.2: It seems to me that these rather crude assumptions about error magnitude and correlation significantly reduce the value of the model. For example, the choice of constant correlation length parameters of 4000 yr / 50 m seems to be a stretch . . . I would estimate that the errors are likely to have weaker (auto)correlation in periods of changeable climate conditions. I know that good estimates are not easy to obtain, but a more full discussion of the implications of these simplifications and – preferably – a sensitivity experiment that allows some quantification of the influence of these error magnitude and correlation estimates would greatly increase the reader's ability to assess the robustness and quality of the new age model. This is especially true if the model results are not very different from those of a simple interpolation (see general comments above). Why should we prefer a modelled time scale based on these simplifications more than a simple interpolation if the influence of the simplifications cannot be assessed?

We agree with the reviewer that it would be beneficial to vary the correlation length and error magnitude parameters in the simulations, but this requires further development of the model. With TALDICE-1, the inverse method provides a suitable solution, bearing a glaciological sense and a realistic isotope/accumulation rate relationship. Moreover the amplitudes of error and correlation length parameters used in the simulation are taken into account in the ice age uncertainty calculation.

A tentative age scale based on a trial-and-error application of the direct model would not provide a better chronology, nor quantified uncertainties.

—- 1747. 14: The line staring with "By comparison . . ." is unclear. What is being compared, and what is "their" after the comma? We rewrote the corresponding section, to make it clearer.

17: Strictly speaking, "The relatively small uncertainty" applies only to the MIS3 part, which is not clear from this line. See above.

20: Clumsy sentence in the beginning. We rewrote the sentence.

22: "largely"? We deleted it

—- 1748. 15: "The tie point assignment becomes more uncertain during this time interval and leads to larger changes in the thinning function deduced by the inverse method." If the tie point uncertainties appropriately reflected the larger uncertainties here, wouldn't one expect the model to produce a less varying thinning function at the cost of a less tight fit to the (more uncertain) age tie in question?

The sentence was misleading. We modified it.

—- 1749. 7-24: These cases of good agreement are direct consequences of the tie point at 14 680 ± 100 yr BP used for the inverse model and a similar tie point used by Lemieux-Dudon et al., 2010. As both TALDICE-1 and the new EDC scales are tied very closely to GICC05, anything else than good correspondence so close to a tie point would be a sign of a huge problem

. . . my point is that this comparison cannot be used as an independent support of the validity of the inverse model, but is a direct consequence of a tie point with a low uncertainty assigned to it. Unless there is another (hidden) point with this comparison, I suggest that the section is removed.

We agree with the reviewer that the use of such tie point already in the match between EDC and GICC05 reduces its weight as an a posteriori control of the chronology. We thus removed the corresponding section.

—- 1750. As the resolution of the δ18Oatm profile in general is lower than the resolution of the data used for deriving tie points (maybe except for the last glacial transition), the agreement does not add much information about the age model . . . you can say that the agreement confirms that the tie points are overall correct (e.g. that the right stadials and interstadials have been matched together), but apart from that, this agreement cannot be used to validate the model. In summary, section 5.1.3 is very weak. The data presented are perfectly reasonable but cannot be used for a confident evaluation of the quality of the age model as the authors claim.

We rewrote the section to better highlight the usefulness of δ18Oatm as an a posteriori evaluation of gas age markers instead of TALDICE-1 overall.

—- 1751. 15: The authors may want to note that the biggest difference between the resulting acc. rates and the simple model (purple) occurs over the 18-30 ka interval, in part of which also the simple Greenland δ18O – accumulation relation- ship breaks down (Svensson et al., 2006, QSR). We added a sentence for this point

—- 1754. Whole section and Fig. 7: I would prefer the consistent use of inverse thinning function or just thinning function, not a mix. Done.

Also, I would suggest that the authors decide whether they trust the a priori thinning function from the ice-flow model that does or does not take into account altitudinal changes (and argue why), and consistently compare the fabric curve with that curve only. We kept only the one that takes into account elevation changes

7: I’m definitely not an expert on this, but the range is 1-3 in the text and 0.2-1 in the figure. Please correct / explain / make consistent. Thanks, it was an error

12: "Around 700–750m (11.5–12.4 kyr BP) the fabric evolution shows an increasing slope (yes) at the time when the ice-flow and inverse thinning functions start to diverge in their main trend (only for the– unrealistic? – case of no altitudinal changes – the inverse model fits well with the solid grey line)" We modified this section

15: "a clear increase in the rate of fabric clustering appears" . . . could this "clear increase" not be caused by one low value (ca. 825 m)? Given that there is quite some noise in the record at this depth, I find the correlations between the fabric orientation and thinning function curves to be very bold. We modified this section

24-25: I simply have no clue what this line means. We modified this section

26: "Around 1100–1150m (42.8–46.6 kyr) both the fabric and the thinning function evolution record an increasing rate of change". I would say that the fabric curve is almost constant . . . the changes happens below 1150 m. We modified this section

—- 1755. 3: "could"? Do the data show this or not? Yes they do.

In summary, my impression as a non-specialist in fabric analysis is that this section is based on rather bold interpretations that rely on the determination of changes of slopes of very short curve segments where error on just one point can change the picture. I am not convinced about the validity of the conclusions, and recommend that the editor seeks expert advice.

We agree and have changed the frame of this section (the response to reviewer 2 focusing more on the fabric analysis). 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.

5: rephrase . . . especially ". . . come now to . . ." The full section has been changed.
—- 1756. 2: Rephrase "and makes a clear bonus to use the" Done.
- Table 5. Rephrase "bibliographic descriptions". Done.
—- Figures. The clarity of figures would benefit from a homogenous font size, consistent use of yr/kyr, and consistent use of labels A, B, etc. on both (sub-)figures and in captions. A use of a more diverse colour scheme (rather than shades of blue) would ease interpretation, as would legends on the figures, so that the reader can see what is presented by the different curves without having to read the caption for colour definitions. Done.
—- Fig. 1: Make figure full width. Done.
—- Fig. 4: Is there an explanation for the offset of the peaks at ∼250 kyr BP? It seems related with changes in the thinning function from the inverse model calculation. But it is not critical for TALDICE-1.
—- Fig. 5: Mention if elevation change corrections have been applied. Fig. 5 relates to the oxygen isotopes in molecular oxygen, not to those of ice. Therefore elevation changes are irrelevant.
Write "atm" in subscript in label. Done.
—- Fig. 6: Mention if elevation change corrections have been applied in any of the three cases presented. Done. It is not clear how the 10Be stars relate to the explanation and accumulation rate ratios on page 1752-53. Made clearer.
—- Fig.7: a1 has a strange font size/subscript in label. There is no mentioning of the 950 m grey-shaded section in the text. Done.