Dear O. Rybak,

Thank you very much for reviewing our manuscript.

Response to the comments:

The original reviewer comments are in italics

Neither the semi-Lagrangian method itself nor its applications in glaciological and paleoclimatic studies are new. The authors acknowledge this fact themselves (p. 1139, lines 18-21) and refer to a series of studies published in 2002-2005 (Clarke and Marshall, 2002; etc.). Among the previous similar studies referenced in the manuscript, one very important paper is missed, which is crucial for understanding of the semi- Lagrangian approach in glaciological modelling and where several numerical issues are examined (Tarasov, L. and W.R. Peltier, 2003. 'Greenland glacial history, borehole constraints, and Eemian extent', Journal of Geophysical Research, 108, B3, 2143, doi:10.1029/2001JB001731). In this paper Section 2.4 is dedicated to semi-Lagrangian tracking in an ice sheet model.

Response: The Tarasov and Peltier (TP) paper applies semi-Lagrangian transport directly to δ^{18}O while the approach based on Clarke and Marshall (2002) uses an indirect approach (see section 2.2 in the discussion paper). In the TP paper the semi-Lagrangian transport was only implemented on a sub-grid which does not cover the whole ice sheet and with a high number of vertical layers (1025 and up to 4096 as opposed to 100 over the whole ice sheet in our study). Since the goal of our work is to close the hydrological cycle in an Earth System Model we require to know the tracer value at the margin. Therefore we need an ice sheet model which is sufficiently computationally efficient to run over long time scales and simulates the δ^{18}O value inside the whole ice sheet. Hence we based our work on the Clark and Marshall approach.

This paper is a model description paper which describes how we implemented the indirect semi-Lagrangian transport into the two-layer polythermal model SICOPOLIS and extended it with a second order backtracking scheme (see section below.).

The conclusions in the TP paper are very good if the passive tracer is δ^{18}O directly but not necessarily for "depositional provenance labels" t, x, y.

We will mention the TP paper in the introduction and emphasise the different approaches of TP and Clark and Marshall.

The authors apply an advanced semi-Lagrangian algorithm (a second order back- tracking scheme) elaborated by de Almeida et al. (2009). Implementation of this newly developed algorithm by de Alameida et al. (2009) seems to be the only one step forward made in the manuscript compared to several pioneering works published in 2002-2005 by Clarke and Marshall, Lhomme et al., etc. Unfortunately, this step is not supported by comparison with previously designed and tested first-order algorithm or with the analytical (i.e. Nye-Vialov) solution. That is why it is not possible to judge whether the modification implemented by the authors is really valuable. Without evaluation of the new method, this study seems to be just a repetition of what had been done before.

Response: We performed some additional experiments and compared the first-order scheme against the second-order scheme by de-Almeida et al. within the EISMINT framework. Enclosed we show some results with flat topography because the interpretation is easier without topographic effects. We also conducted experiments with the same mound topography as in the manuscript which where leading to the same conclusions.

The differences in the centre of the ice sheet are not big, as can be seen in Figures 1,2 and 4. Only near the surface there are some deviations which are linked to the two-level time scheme and the introduction of new ice at the surface. However in total, the differences between first and second order at the ice core location C1 and C2 are negligible.

Nevertheless, the differences near the ice sheet margin are substantial as can seen be seen in Figures 3 and 4. Figure 3 shows a virtual ice core in the ablation zone whereas ice core C1 and C2 lay in the accumulation zone. In order to close the δ^{18}O cycle in Earth System Models the values at the margin are of interest and therefore the difference between second and first order matters. Previous studies of tracer transport in ice sheets were mostly focussed on the ice sheet interior and for that purpose still first order backtracking method may be sufficient.

This deviations between first and second order in the ablation zone are likely associated to the greater velocity gradients near the margin. In addition during ice sheet build up the velocities vary more in the ablation zone, which is better handled by the two-level time scheme with second order accuracy.

The differences between first and second order are substantial near the margin but we can not proof if second order is really better in the scope of this paper. Therefore, a detailed comparison of different semi-Lagrangian, Lagrangian and Eulerian schemes in ice sheet models is currently ongoing and will be condensed into a subsequent manuscript. Nevertheless for now, we assume that the second order scheme delivers better results near the margin and therefore the higher numerical cost of about 5 times compared to the first order one are acceptable.

In addition a comparisons of first order and second order semi-Lagrangian schemes in atmospheric models can be found in McGregor (1993) and in Staniforth and Pudykiewicz (1985). Staniforth and Pudykiewicz (1985) found that first order schemes are inaccurate for large Courant numbers and exhibit poor conservation properties. McGregor (1993) observed that a first order scheme with straight lines and velocities taken at the end point produces an error of 4% each time-step for trajectories in a solid-body rotation problem.

We will include a comparison of first and second-oder schemes in the revised version of the manuscript.

The fact, that the SICOPOLIS model is polythermal and models employed in previous similar studies are not, does not bring any new experience. Or, at least, this new experience is not discussed.

Response: The polythermal model provides a more complex rheology treatment (temperate versus cold ice) in modelling ice dynamics and hence improve tracer transport. It also allows some additional information which could aid ice core interpretation:

If the particle at any time of its history would cross the cold-temperate surface (CTS) the tracer signal may be corrupted. It would be possible to include a marker if the ice ever crossed the CTS or not. Or in an alternative way if the particle was in a region where the water content was above a certain threshold and the passive tracer assumption does not hold anymore.

We will not implement this method at this time but will include it in the discussion.

The aim of the paper is to ". . . to simulate the δ 18O distribution in ice sheets . . ." (p. 1139, lines 27-28). In my view, the authors fail to demonstrate feasibility and advantages of their new method. At least, it does not follow from the examples demonstrated in the paper. For instance, mismatch between curves in fig. 14b can be attributed either to SICOPOLIS' errors in dating or to the wrong performance of the tracking algorithm or to any other reason. Nevertheless, the authors mention "The comparison between the simulated cores and observational data shows in general a good agreement of the isotope records." (p. 1152, lines 24-25). This is just a qualitative evaluation, which is not supported by any quantitative consideration.

Response: We aim at demonstrating the general performance of the method and do not intend to detailed reconstruct tracers at specific core locations. Therefore we only use SICOPOLIS in a standard setup without special tuning of boundary conditions which is of high relevance in the low precipitation area of Vostok station. We address the mismatch in depth between simulated and observed δ^{18}O to model deficiencies rather than tracking algorithm errors. The relative signal agreement in a qualitative evaluation is rather good. More care will be taken in future to this problem when applying the method in a quantitative way.

Comparisons of simulated and observed δ18O curves in the papers by Clarke et al. (2005) and in Tarasov and Peltier (2003)look more convincing. Experiments with the schematic EISMINT-type model (Section 3.1.1) seem to me totally uninformative.

Response: In general the EISMINT figures show that the results are symmetrical and are easier to interpret than the Greenland and Antarctic examples because of constant and symmetrical boundary conditions.

Authors cite the PhD thesis of Nicolas Lhomme (2004), which is dedicated to modelling of water isotopes in ice sheets and where the problem is thoroughly examined. Taking into account papers cited in the manuscript as well as the papers ignored (Tarasov and Peltier (2003) and some papers with Lhomme as a leading author or the co-author) one can see that the problem of simulation of isotope composition of ice sheets, isotope stratigraphy and related topics have been already intensively discussed in the past. So what is the scientific merit of the current study? Coupling of semi-Lagrangian tracking procedure developed by the authors with the ice sheet model SICOPOLIS is by no means a break-through because similar studies have been carried out long ago much more scrupulously.

Response: Indeed Lhomme et al. (2005) is also using the semi-Lagrangian scheme based on Clarke and Marshall and we include the citation on p.1139 line 18 and line 24. Nevertheless, Lhomme et al. (2005) uses the same semi-Lagrangian methods as the other papers already mentioned. The goal of the previous studies was mainly focused on the ice sheet interior and ice cores while in this study we are interested in the δ^{18}O at the ice sheet margin.

The only advantage of the manuscript is that it provides public access to a tested and working semi-Lagrangian procedures SICOTRACE and SICOSTRAT. In my view, this is *not enough. My opinion is that the paper, as it is presented now, cannot be accepted for publication in Geoscientific Model Development. Major revision is required.*

Response: We do not agree to the reviewer in this point. Besides the acknowledgment of the manuscript by the other reviewers (G. K.C. Clarke: "Adding a tracer tracking capability to a well-regarded. well-supported and readily available ice sheet model is a substantial contribution and one that certainly merits publication in GMD", F. Parrenin: "The manuscript perfectly fits into the scope of GMD. The tracers transport scheme is a semi-lagrangian one, like in the original work of Clarke and Marshal. But this time a higher (second) order scheme is used, which makes the scheme more accurate." we do think to substantially contribute to the aim of GMD, namely "... the description, development and evaluation of numerical models of the Earth System and its components". The current manuscript demonstrates the full implementation of tracer transport in an ice sheet model, using a semi-Lagrangian approach of second-order accuracy. We agree that a comparison to first-order accuracy (that is also requested by the other reviewers) will strengthen the paper and demonstrate the applicability and accuracy of the applied method. We will include a discussion on first vs. second order backtracking and a discussion on particles crossing the CTS in the revised version. In today's modelling efforts of fully coupled Earth system models the incorporation of traced transport is important to close the hydrological cycle and trace paleoclimate information in order to support paleoclimate interpretations of proxy data. The general performance of our approach can best be seen in simplified case study experiments as for example the EISMINT setup since model artefacts or forcing/boundary conditions can be simplified. The application to Greenland and Antarctica is only intended to demonstrate the general performance and applicability on "real settings" without major efforts of special tuning or regional focussing as done in the Tarasov and Peltier study.

Particular notes

1. Right spelling is Côté in the reference

Is now corrected.

2. Figures 7 and 9 are totally non-informative

Figure 7 has to be read together with Figure 8. It illustrates the ice is only transported a short distance in x direction while most of the transport is in y direction. While Figure 9 illustrates where the oldest ice can be found.

3. Wiggling of isolines in figures of 8 and 10 probably witness about the problems with the numerics.
The wiggling of the isolines are not present with a smaller time step.

4. Antarctica in fig. 13b looks strange.
That is because the ice shelves are not included in SICOPOLIS 2.9. We added this information to the image caption.

5. I do not think that the reference to Gornitz (2008) is a proper one for isotopic thermometry and related issues
We added some references to the first sentence in the introduction: "Oxygen isotopes are an important proxy for the reconstruction of temperatures of the past. Air temperature is related to stable isotopic composition of precipitation as indicated by observations (e.g. Daansgard, 1964; Gat, 1996; Jouzel et al., 1997; Gornitz, 2009)"

Sincerely,
T. Goelles et al.

References

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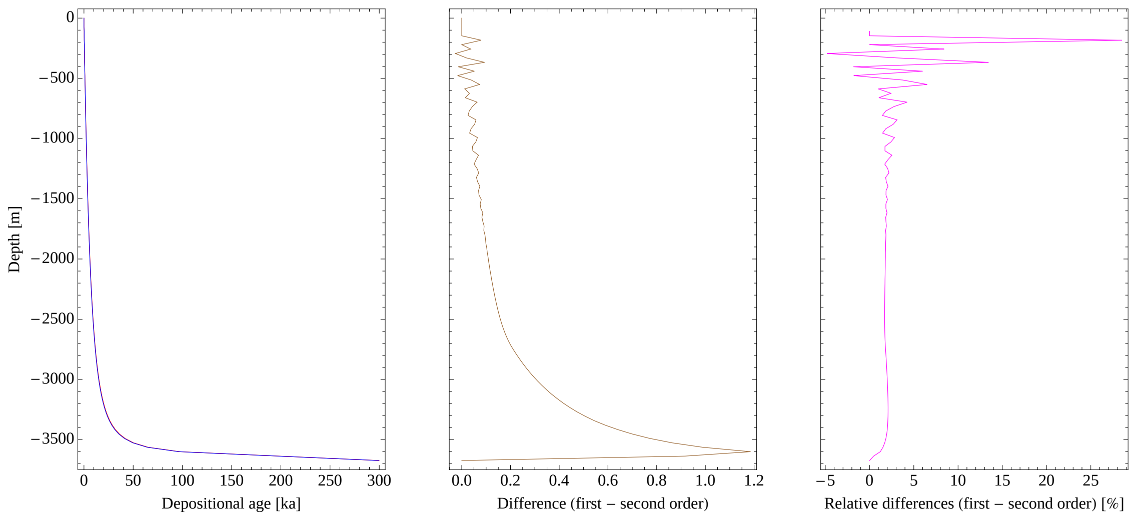

FIGURE 1. Simulated ice core C1 at x=y=750 km. The figure on the left shows depth vs depositional age in *ka* with first order in red and second order in blue. The middle figure shows the absolute difference (first - second order) and the right figure the relative difference in %.

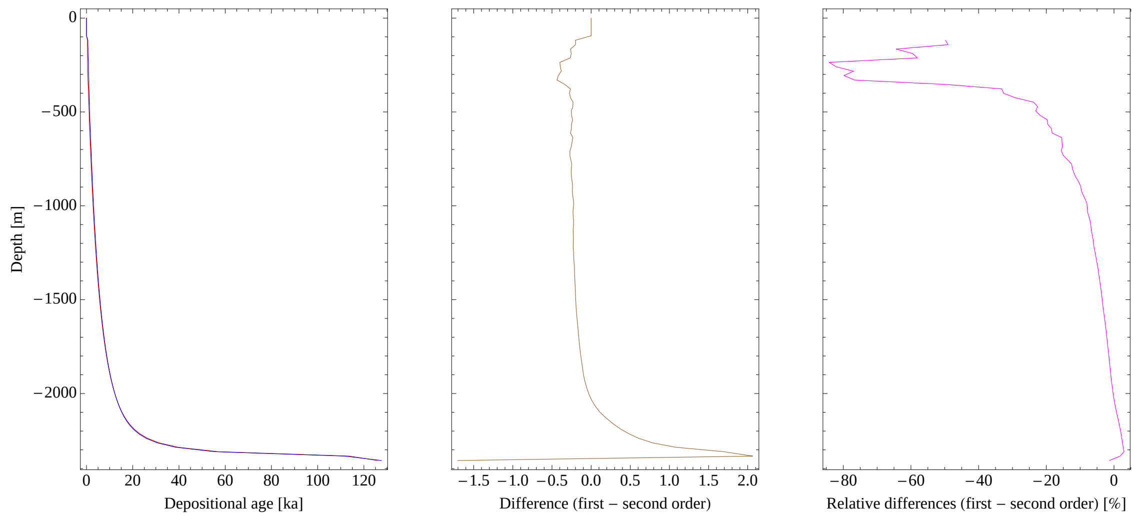

FIGURE 2. Ice core C2 with the same figure arrangement as in C1.

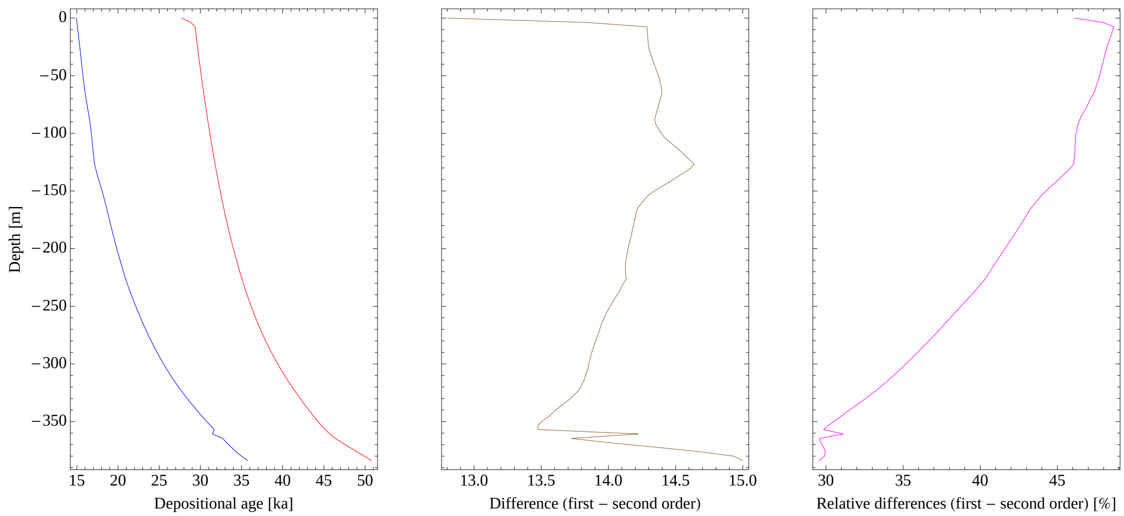

FIGURE 3. Ice core in the ablation area at x=y=1250 km. This core is not included in the discussion paper but illustrates the differences between first and second order in the ablation zone and will be included in the final version.

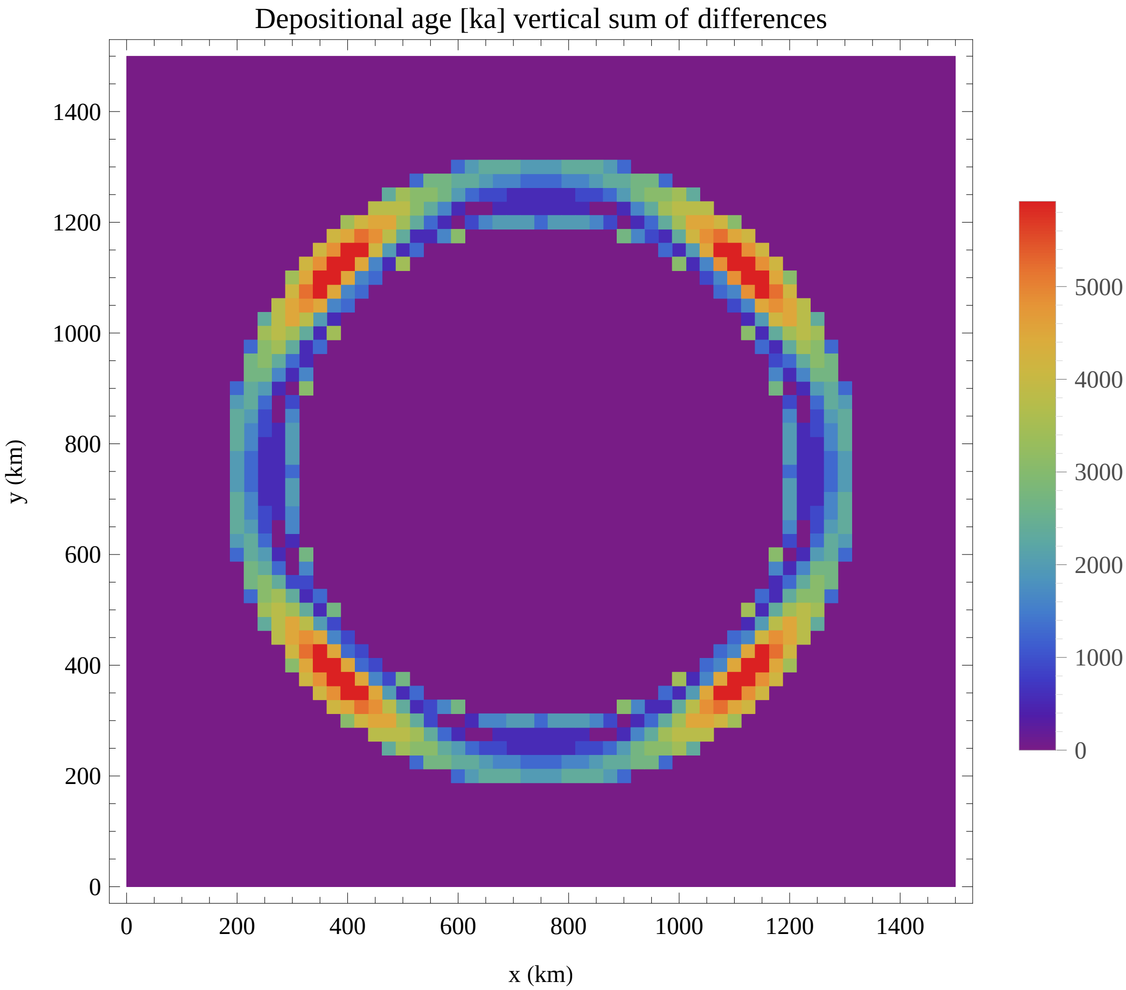

FIGURE 4. Vertical sum of the absolute value of the differences at each grid cell: $\sum_{\mathrm{ks}=1}^{\mathrm{ksmax}} \left| \frac{\mathrm{first(ks)} - \mathrm{second(ks)}}{\mathrm{first(ks)}} \right|$