Geosci. Model Dev. Discuss., 7, C121–C123, 2014
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Interactive Comment

Interactive comment on "Semi-Lagrangian transport of oxygen isotopes in polythermal ice sheets: implementation and first results" *by* T. Goelles et al.

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Received and published: 13 March 2014

The paper deals with the application of a Semi-Lagrangian particle tracking method in ice sheet models. From the formal point of view, the manuscript fits to the scope of the journal. But I see several serious reasons against its publication of this manuscript without serious revision.

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,

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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.

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. 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.

The aim of the paper is to "... to simulate the δ18O 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. Comparisons of simulated and observed δ18O curves in the papers by Clarke et al. (2005) and in Tarasov and Peltier (2003)

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look more convincing. Experiments with the schematic EISMINT-type model (Section 3.1.1) seem to me totally uninformative.

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.

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.

Particular notes: 1. Right spelling is Côté in the references. 2. Figures 7 and 9 are totally non-informative. 3. Wiggling of isolines in figures of 8 and 10 probably witness about the problems with the numerics. 4. Antarctica in fig. 13b looks strange. 5. I do not think that the reference to Gornitz (2008) is a proper one for isotopic thermometry and related issues

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Interactive comment on Geosci. Model Dev. Discuss., 7, 1137, 2014.