Final Author Comments on "A dynamic continental runoff routing model applied to the last Northern Hemisphere deglaciation" by H. Goelzer et al.

We would like to express our gratitude to both reviewers for their comments and suggestions. Please find below our response to the comments, indented and italic.

Reviewer 1

This is a nice paper looking at a key issue - modeling the routing of meltwater. It is an unusually clearly written paper that it is in essence a clear presentation of the modelling approach and, as such, is highly relevant for CMD. It should be published after the authors have addressed the following minor comments:

1. A slightly more detailed discussion of the routing ideas put forward by Clark et al and Broeker et al would be very useful to give more motivation for this work. Interestingly, a self-consistent routing/GIA model such as this could actually help constrain ice sheet reconstructions more fully if properly integrated with data.

> *We agree with the reviewer and will modify the text in the introduction and the results section to include a more detailed discussion of these references and their importance for the work.*

2. Page 5 line 2 - I suggest that 'use' rather than 'utilize' is less verbose.

> *OK, will be changed.*

3. There is some similarity between this problem and other topography-related problems: sills in the ocean, mountain peak altitude for atmospheric models. Can the authors compare their solution to these problems with parallel problems for other cases?

> *If the mentioned problem is how to deal with the smoothing of topographic features in coarse resolution models, we are happy to include a discussion on that.*

4. Page 10, line 14: are these really the most accurate/appropriate resources? Are there not more 'scientific' resources in the hydrography literature?

> *The second reviewer also raised this point. We will follow his advice and compare against the HYDRO1k basin divide with side-by-side figures.*

5. Page 10, lines 15-31: this raises some issues for me. As I understand it, all transport is only from one grid box to the lowest neighbouring grid box. These complexities indicate that the transport could be proportionally distributed to all neighbouring grid boxes and not just to the lowest one. Or am I missing something?

In principle it is possible to distribute proportionally to neighbouring grid boxes, but the benefit is not evident. It certainly makes separation of different drainage basins difficult and prevents the application of our offline water storage module.

6. Page 10, lines 15-31: is the ETOPO5 map really the best resource for this application? Inevitable smoothing is a very serious issue when this problem requires clear delineation of very narrow minima of the scale of rivers (<1km). Certainly, much better resolution is available from region-specific cartography. I think that a careful discussion of why relatively low resolution mapping is being used and what could be done to improve this is important.

*The other reviewer also raised this point so we respond to both comments here. At the time of model development, we were well aware of the existence of HYDRO1k but rejected it for the following reasons. The ocean bathymetry, Greenland and the larger lakes are not represented in HYDRO1k and the continents are represented in 3 different maps and coordinate systems. For our application, which covers a continuous area over a large section of the Northern Hemisphere, we wanted to prevent the risk of creating artificial discontinuities at the map boundaries, and therefore finally opted for using ETOPO5. The ultimate goal of developing the CRM is to use it inside the framework of a fully coupled global atmosphere-ocean-ice sheet model with other components competing for computational resources. It is therefore crucial to be able to run the model at low resolutions with a reasonable representation of the runoff routing on a scale that the ocean model is sensitive to. In fact, a comparison between our original results with ETOPO5 for the present day configuration and the same experiment using HYDRO1k (combined with ETOPO1 for undefined regions) at the same resolution shows very similar results (Figure **1**). This strongly suggests that using ETOPO5 instead of HYDRO1k does not decisively compromise our results. However, in the final version we will derive lower resolution versions from the HYDRO1k DEM. This facilitates a direct comparison with results for higher resolutions and validation against the HYDRO1k basins data set as suggested by the reviewers. We will also extend the discussion about the ultimate goal of the model development and the necessity of running the model in low resolution in the manuscript.*

7. Conclusions/further work - What data could the model be compared to/improved against?

At this stage it is difficult to separate the influence of the ice sheet and isostatic models, which adds uncertainty to simulations and makes it difficult to evaluate the model during past periods. There is however a growing body of geomorphological data that can eventually be used to constrain the model (e.g. Toucanne et al., 2010). A comment will be added to the validation section which points in that direction.

Reviewer 2

This submission describes a newly developed surface drainage solver with postprocessing lake storage. The submission briefly includes sample application to the last deglaciation and sensitivity to grid resolution of the drainage solver. The apparent intent is to validate this

solver for glacial cycle (or at least deglacial) drainage modelling. The solver offers nothing new over previous solvers (eg Tarasov and Peltier, 2005/6).
Furthermore, surface water storage is only implemented in post-processing form (unlike Ibid which has dynamic surface water storage). As I understand, the purpose of this journal is to document models, and there is no need for the models to be "better" than previous models. What I do find misleading is that this submission tries to sell a trival innovation that is in fact not innovative (as detailed below), and it fails to clearly compare the features of the described solver to previous ones in the literature and to document sources of error resulting from model approximations.

> *Our intention is to explain that our model is readily set up to be coupled with an ocean model, not that the model is "better" than the ones cited in the references. We will modify the manuscript as below and apologize for any misunderstanding in this regard.*
> *P4,L8 "While the routing of freshwater for a given topography is a standard procedure, and an example exists for the time period of the last deglaciation (Tarasov and Peltier, 2005), our model is specifically aimed for and ready to be coupled to an ocean model. We therefore address the problems that arise from the combination of a temporally changing topography …"*

My major concerns are:
1) The hydrologically-self-consistent HYDRO1k topographic data-set should be used, and not ETOPO5. One also needs to verify that critical sill elevations are preserved under coarse-graining.

> *As detailed above, we have rerun the present day configuration with the HYDRO1k DEM (in combination with ETOPO1 for regions where HYDRO1k is undefined) with notably very similar results on 6.25 km resolution compared to ETOPO5. We will nevertheless rerun all experiments with the more authoritative HYDRO1k DEM. For coarse resolution versions of CRM, we will manually edit the topography to reproduce the present day basin map as has been done for ETOPO5 before.*

2) There needs to be clearer (ie quantitative) validation of the derived drainage basins with this solver. How do drainage basins areas compare to that of the HYDRO1k dataset? Could consider including side by side plots of present-day modeled and HYDRO1K drainage basins.

> *We will compare our results for the present day topography to the HYDRO1k basins map and will follow the suggestion to give side by side plots of the drainage basins in the validation section of the manuscript.*

3) Relatedly, given the size of major drainage sills/chokepoints, an accurate deglacial drainage calculation with a 50km grid resolution ice sheet model has to take into account changes in past sill elevations and sub-grid ice margin position for certain critical choke points (cf Tarasov and Peltier, 2006). None of these issues are even mentioned in this submission.

> *We will add a discussion on that point in the manuscript with reference to the*

reviewer's prior work. However, we would like to stress that an accurate deglacial drainage calculation is not within the scope of our paper with focus on the CRM description. Being well aware of the complexity of such an experiment, we merely use the deglaciation as a test case to show the sensitivity of the CRM to grid resolution and isostatic parameters. We assume that even for a non-perfect ice loading evolution, a wide enough range of topographic configurations is represented in this experiment to distinguish different CRM versions.

4) As detailed below, I know from my own work that the coarsened (in this case via subsampling of a Gaussian filtered higher res version) 50 km version of the ETOPO5 DEM with the stated hand edits is inadequate for accurate deglacial drainage modelling, contrary to the stated conclusion.

The conclusion is based on the fact that changing the resolution by a factor 8 does not result in considerable differences for the large-scale routing patterns with ETOPO5. We will rerun the experiments with HYDRO1K, but do not expect considerably different results.
We will however reformulate the conclusion to put more emphasis on the envisioned application of the CRM (fully coupled atmosphere-ocean-ice sheet model), in which accurate representation of the runoff on the fine scale is not crucial.

5) It should be mentioned that the lack of dynamic surface water storage (lakes), means that computed surface elevations will have an extra source of error whenever proximate to pro-glacial lakes (since the surface load for isostatic adjustment determination is missing the water load). Furthermore, this deficiency precludes modelling of lake calving.

A comment will be added accordingly covering both the additional source of error and the absence of lake calving:
"As opposed to other routing algorithms that include dynamic lake storage (Tarasov and Peltier 2005; 2006), our lake module operates in post-processing mode only. This introduces a source of inaccuracy to the computed elevation changes since the water load is missing in the isostatic calculation. At the same time lake calving cannot be included."

Specific comments

abstract: should be more precise with "considers changes in water storage and lake drainage", as the water storage is only implemented in post-processing form

OK, will be changed accordingly.

" 2005), our approach is novel in the following aspect: it specifically addresses the problems that arise from the combination of a temporally changing topography (as a func- tion of evolving ice thickness and isostatic adjustment) with an ocean model grid that uses a fixed land-sea mask. This represents an important step towards coupled ice-.. ... ocean model grids has to be taken into account, and the changing topography makes it necessary to regularly update the routing matrix automatically and in a computationally efficient way."

##This is a trivial feature to highlight and is not novel. Ie one simply drains downslope, filling depressions along the way, to whatever elevation threshold or other condition (overlap with ocean mask) you want along with some imposed settings on drainage pointers to deal with grid boundaries and such. I already did this back in 2005 (Tarasov and Peltier 2005), with a chosen threshold at the 600 m bathymetric depth (and with topography and drainage mask updated every 100 years, though could have easily been more often).

> *We agree with the reviewer that the applied concepts are not new. We would like to stress however that our model is readily set up to be coupled with an ocean model. We have reformulated this part accordingly.*

##What I do find irksome, is that this submission cites Tarasov and Peltier 2005 and yet fails to mention that their algorithm does have dynamic lake storage as part of the drainage module (and not as a post-processing routine, detailed in Tarasov and Peltier 2006).

> *See response to comment 5 above.*

"flexural rigidity D, meaning that aside from local isostasy, contributions from remote locations are taken into account as well (Le Meur and Huybrechts, 2001)."

##should mention that this type of isostatic adjustment model has been shown to be inaccurate for glacial cycle modelling, (van den Berg et al, 2008). I would submit even more so for drainage modelling given sensitivity to sill elevations.

> *A comment will be added to section 2.1 to mention this limitation of the ice sheet model.*

"5 °— 5 resolution, called ETOPO5 (1998). We favoured the 5 resolution version since a DEM of higher resolution, which contains bedrock information of e.g. the Great Lakes and ocean bathymetry was not available at the time of model development. Note that"

##This is a lame excuse as one can always merge different topographies.

> *In the final version we will use HYDRO1k in combined with the newer ETOPO1 in regions not covered by HYDRO1k. See also detailed comment above.*

"current sea level. Furthermore, we increase the ocean mask by extending it radially around each CLIO grid point."

##I don't see any need for this. Simply keep going downslope, filling depressions along the way, until you hit your ocean mask.

> *The reason for extending the ocean grid boxes is to make a distinction between ocean simply not represented on the ocean model grid (for instance in the Baltic Sea) and internal lakes (for instance the Caspian Sea). Even if the water runs off to deeper*

regions (for instance -600m), it is not possible to determine automatically if we are in a deep lake or in the ocean.

" close to the dam lakes " # -> ice-dammed lakes I think? Though did offer some humour.

OK, we will remove this sentence.

##Also, how do areas of PD drainage basins compare between your model and that of the hydro1k dataset?

We will include side by side plots comparing against HYDRO1k in the final version, as discussed above.

##The resolution sensitivity tests are weak given that there is no indication of whether the modelled deglacial margin chronology has any close relationship to that inferred geologically.

As stated above, we argue that a wide enough range of topographic configurations is represented in such an experiment, whether or not the ice sheet evolution is realistic. It therefore represents a good test case to see if models of different resolution can be distinguished.

"way. Calculating changes in water storage and consecutive redistribution on the high resolution ETOPO5SP grid, although theoretically possible, is too time consuming. We

"4 Conclusion and discussion It was shown that the large-scale ocean basins and timing of major freshwater forcing periods is relatively stable between versions of different DEM resolution. Hence, for the range of DEM resolutions under scrutiny, the lowest version with 50 km resolution is sufficient for calculating the large-scale runoff pathways necessary to determine the routing of freshwater fluxes to an ocean model. Changes in the isostatic adjustment"

This conclusion is misleading. It only shows little change for the given DEM and deglacial chronology. I know from my own detailed deglacial modelling of North America that vanilla grid coarsening with the stated local edits is inadequate to accurately model deglacial meltwater drainage pathways (at least when ice margins are subject to geologically inferred constraints). There are, for instance, other critical choke point regions associated with the Eastern and Northern outlets of Lake Agassiz that also require hand editing. How do critical sill elevations in the coarsened grids compare against published values for the eastern Lake Agassiz outlet (Nipigon basin) (Teller and Thorleifson, 1983)? Furthermore, the applied grid coarsening algorithm will significantly change sill elevations for grid cells of sufficient topographic roughness (eg all the controlling sill regions for lake Agassiz drainage). Accurate sill elevations are especially critical for determining paleo lake levels and effective strand-line elevations.

We agree with the reviewer that the conclusion was based on only one given DEM and deglacial chronology. However, results for the present day configuration with the HYDRO1k DEM show close similarity to ETOPO5 (Figure 1). Furthermore, in order *to cover a wider range of possible chronologies we propose to add a comparison of different resolutions also for experiments with different isostatic parameters.*

##At 50km there is no reason why you couldn't dynamically compute actual surface water storage. My full drainage solver (ie with dynamic water storage) adds less than 30 minutes to the run-time at that resolution for a 120 kyr continental scale run (running on a single PC core).

We have not implemented a dynamic water storage model at this stage, but will add a comment in the conclusions as an outlook for further improvements.

Figs: #######

##Would really help to see how PD drainage basins change wrt resolution changes at a time when drainage is sensitive to the exact ice margin position (eg 12-13 ka for North America).

We are aware that our ice sheet model does not necessarily produce the exact ice margin position. We therefore propose other experiments that cover a wider range of challenging configurations varying the isostatic parameters, as mentioned above.

##fig 8., yellow (GOM) line too indiscernable, use a different colour. Also, the presentation does not facilitate comparison. Instead, plot all the resolutions for the same drainage sector on one plot (eg GOM and Pacific), and split different sectors between different plots.

OK, figures will be changed accordingly

##Instead of displaying the bland 20ka drainage topography, try a much more challenging 12.5ka drainage topography for North America that is in accord with geological constraints on ice margin position. I know from personal experience that meltwater routing during this time is much more sensitive to topographic details.

We have experimented with transient ice sheet simulation under geomorphological constraints, but without final conclusions. We therefore repeat our proposal to run additional experiments under different isostatic parameters. We believe this will yield an alternative test for the resolution dependence.

References

Toucanne, S., Zaragosi, S., Bourillet, J.F., Marieu, M., Cremer, M., Kageyama, M., Van Vliet-Lanöe, B., Eynaud, F., Turon, J.L., Gibbard, P.L., 2010. The first estimation of Fleuve Manche palaeoriver discharge during the last deglaciation: Evidence for Fennoscandian ice sheet meltwater flow in the English Channel ca 20-18 ka ago. Earth and Planetary Science Letters, 290, 459-473.

Figures

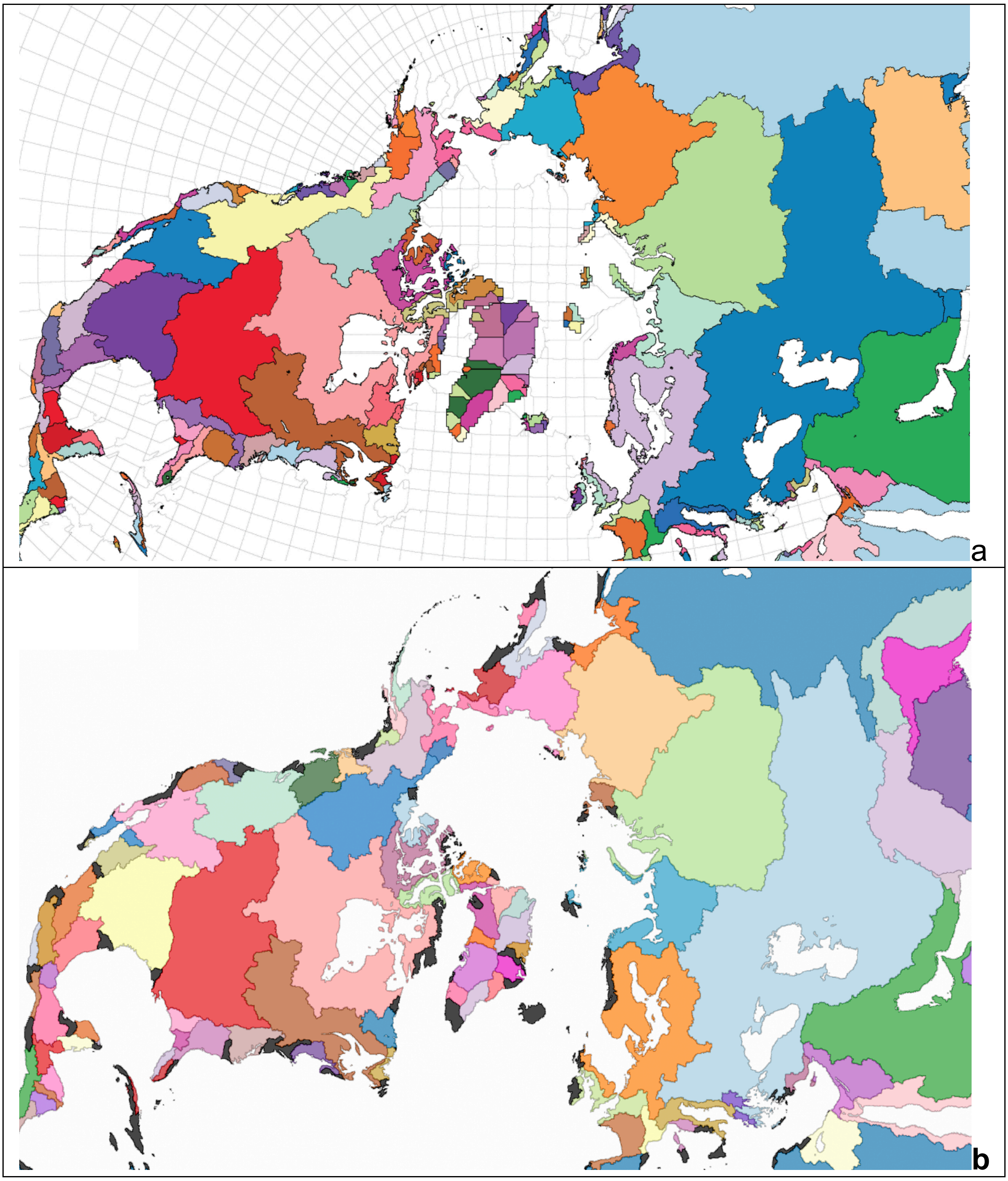

Figure 1 Basin division with ETOPO5 (a) and HYDRO1k (b) for DEM resolutions of 6.25km.