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Interactive comment on "NCAR global model topography generation software for unstructured grids" *by* P. H. Lauritzen et al.

Anonymous Referee #2

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The manuscript documents a method to generate topography-related information needed by atmospheric models, either by the dynamical core (smoothed mean topographic height) or by subgrid-scale parameterizations (subgrid-scale variance). Section 3 presents simulations obtained with CAM-SE and different strengths of orography smoothing.

Generating topography-related datasets on the model grid is definitely required for any Earth System Model. The novel character of the method is that it deals with unstructured, rather than longitude-latitude target meshes (it also handles lon-lat target meshes). The technically hard pieces are borrowed from previous work related to remapping-based transport schemes. The method is essentially a combination of these technical tools and common sense. The presentation of the method is clear and

contains essentially all the information that would be needed to re-implement it, except for the generation of the "supermesh" (see comment 7).

Section 3 is quite independent from the first two. It might be better to study the impact of topography smoothing in a more in-depth, separate paper. Unfortunately without section 3 the present manuscript would boil down to a short, descriptive technical report. My understanding of GMD is that the manuscripts should go beyond this and provide insight into the rationale and consequences of choices made in the method, insight that could be valuable for GMD readers designing similar methods. I would therefore strongly encourage the authors to better document and analyze important characteristics or potential pitfalls of the method, which in fact has probably been done in the design process. I would especially suggest to discuss quantitatively grid imprinting (see comment 6), algorithmic complexity (see comment 7), computational cost/performance. When relevant, it would be important to address as a target mesh not only the cubed-sphere (as in CAM-SE) but also more complicated meshes (e.g. SCVT/MPAS), since a strength of the method is the variety of possible target meshes. This would improve the usefulness of the manuscript for an audience broader than CAM users.

Overall I recommend a major revision.

Detailed comments

1. p. 4624, line 18 : maybe cite Lott (MWR 1999) rather than Lott & Miller (1997) for mountain drag and planetary waves
2. p. 4625 line 1 : I am not sure how orography is represented in global high-resolution databases but from a physical point of view, especially with variable gravity, only the surface geopotential is a generally well-defined specification of

orography. Φ_s can then be expressed as an elevation "above" a reference iso-geopotential surface. This conversion is simplest ($\Phi_s = gh$) when the variations of gravity are neglected.

3. A related question pertains to what "latitude" precisely means, i.e. geodetic vs geocentric latitude. Models typically make the spherical-geoid approximation, in which case both coincide. However the real Earth is slightly flat and high-resolution databases must presumably specify what they mean by latitude and what geodetic system they use, see e.g. Monaghan et al. (2013) http://dx.doi.org/10.1175/MWR-D-12-00351.1

4. p. 4625 line 25 : while the importance of subgrid anisotropy is stressed in the introduction, the presented method does not compute quantities characterizing this anisotropy, e.g. the coefficients γ and ψ of Lott & Miller 1997, Appendix A. Do CAM physics not require this kind of information ? It might be fair to stress that the presented method adresses only isotropic subgrid-scale physics.

5. p. 4631 : maybe this page could be replaced by a reference to Nair et al. (2005) ?

6. p. 4632 : the cubed-sphere mesh has non-uniform isotropy, as cells near the cube corners have angles close to 60 and 120 degrees while cells near the face centers are close to square. One therefore expects that the position of the cube corners will somewhat affect the results of the method (grid imprinting). This impact might be small but it would be interesting to quantify it. It would be easy to generate $Var^{(tms)}$ with different positions of the cube corners (e.g. rotating the cube by 45 degrees and/or positioning corners at the poles) and plot the difference.

7. p. 4634, (13) : enumerating the non-empty Ω_{kl} can be a very time-consuming part of the method if done naively. An upper bound of algorithmic complexity is given

by a brute-force approach where the intersection of each Ω_k and each A_l is computed. For large meshes this is very inefficient as most intersections are empty. More sophisticated and efficient approaches exist, e.g. doi:10.5194/gmdd-8-4979-2015 ; how does this part of the method compare, in terms of algorithm and efficiency ? Is it a time-consuming task ?

8. More generally, could the authors provide at least indications about the computational time spent in each step of the method for a few representative target meshes/models (e.g. CAM-SE, MPAS) ?

9. p. 4634-4635, about defining variance with smoothed orography. Definition (16) of variance corresponds to :

$$Var(h) = \overline{\left(h-\overline{h}\right)^2}$$

where $h = h^{(cube)}$ and $h \mapsto \overline{h}$ ($\overline{h}^{(tgt,raw)}$) averages h over a target cells, resulting in a piece-wise constant function on the target mesh. Since $h \mapsto \overline{f}$ is a projector one has also

$$Var(h) = \overline{h^2} - \overline{h}^2. \tag{1}$$

and therefore the total "energy" $\sum_l \Delta A_l h^2$ is exactly split into resolved and subgrid components. However after replacing $\overline{h}$ by a smoothed orography $\tilde{h}$ (noted $\overline{h}^{(tgt,smooth)}$ in the manuscript), this decomposition is not exact any more, i.e. using $\tilde{h} = \overline{\tilde{h}} = \tilde{\overline{h}}$

$$Var(h) + \tilde{h}^2 = \overline{h^2} - 2\tilde{h}\left(\tilde{h} - \overline{h}\right).$$

One may then consider other (also imperfect) formulations such as :

$$\begin{aligned} Var(h) &= \overline{h^2} - \tilde{h}^2 \\ Var(h) &= \widetilde{h^2} - \tilde{h}^2 \\ Var(h) &= \widetilde{\left(h-\tilde{h}\right)^2} \end{aligned}$$

Are there reasons to prefer (18) ? Would switching to one of the above definition make a difference in practice ?

10. p. 4638, section 3 : the experiments are presented as “topography smoothing experiments”. However if I understand correctly, each (differently smoothed) $\tilde{h}$ will also be associated to a different subgrid variance $Var^{(gwd,smooth)}$. Hence the differences between the experiments are due not only to different $\tilde{h}$ but also to different $Var^{(gwd,smooth)}$. What is the predominant effect here ?

11. More generally, the impact of topography smoothing is a problem quite independent from the method used to generate the topography-related quantities. Furthermore this problem exists independently from the structured/unstructured character of the mesh. Different models might have different sensitivities to topography smoothing, but the reason may lie more in the numerical formulation or physics than in the mesh per se. Section 3 only scratches the surface of the problem. It might be more relevant to address it more in-depth in a separate paper.

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