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Interactive comment on "The Finite Element Sea ice-Ocean Model (FESOM): formulation of an unstructured-mesh ocean general circulation model" *by* Q. Wang et al.

Q. Wang et al.

qiang.wang@awi.de

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Dear reviewer,

Thanks for your review and very helpful comments. We revised the manuscript, and the detailed reply to your comments is provided below.

Major comments

1. The major novelty claimed is that this represents the first unstructured mesh ocean model developed for climate research. However the provided motivation for unstructured meshes in this context is insufficient. The description of the possible benefits

is too brief, and details of the drawbacks are largely absent. A detailed performance analysis would be highly desirable. Do the potential benefits, in simulations of the global climate, really justify the very significant additional computational cost of using an unstructured mesh? This central question is almost completely side-stepped by the article.

Reply: The focus is the description of an OGCM formulation with FESOM, in the context of climate research. We are not going to discuss benefits of model's unstructured mesh functionality here; this should be (and is) the subject of separate papers, and some of them are cited in the paper. Proving that the model has benefits can only be made by using it, and any general discussion will not persuade a reader. The details of numerics are intentionally avoided — they have been addressed in other papers. Unstructured-mesh models are slower than their structured-mesh counterparts, but whether one can justify the extra computational cost depends on the problem one is going to explore. We also note that the higher computational cost is much less an issue in a setup coupled to the atmospheric model, because of the higher cost of the latter in the coupled system.

2. An open question in large-scale unstructured mesh ocean modelling is the implementation of parameterisations when the mesh resolution varies significantly (e.g. Ringer et al 2013, cited in the article). While some details in this direction are given in section 3.9, this issue is in general given too brief a treatment.

Reply: Sections 3.9 and 3.10 discussed some practical aspects of this issue. The topic is indeed largely open and need much research in the future, as mentioned in the paper.

3. I find the article poorly structured and imbalanced. The model history appears in section 2.3, but it seems logical that this should appear before the numerical details of sections 2.1 and 2.2 (e.g. in the introduction). The description of the ice shelf model appears at the very end of the article (and is very brief) even though this motivates design choices described in much earlier sections. The description of the model nu-

merics is brief, while the description of ocean model parameterisation schemes is very lengthy.

Reply: The section 2.3 has its location because some terms should be defined first (so in sections 2.1 and 2.2) to allow a smooth reading for general readers (including users and general OGCM developers). The ice shelf model is linked to a few other components, so we put it at the end after these individual components were discussed; this make it easier to understand. As mentioned above, the focus of the paper is on the "formulation of OGCM" with FESOM, so we picked up most of the key components, not just numerics.

4. The model integrations seem to be rather short (60 years) for examples of climate relevant simulations, particularly given the coarse resolution.

Reply: Sensitivity experiments reported in this work serve to provide motivation for choosing a particular physical/numerical scheme. We use longer runs in practical tasks, especially in the context of the coupled climate model (FESOM-ECHAM)

Minor comments:

1. A large number of footnotes are used. Many of these could be integrated into the main text.

Reply: The paper is long, so we move some of the text to the footnotes to make it easier to read for broad readers, as they can neglect these footnotes without losing the key information.

2. A brief discussion of mass conversion issues would be useful when discussing pressure stabilisation in section 2.1

Reply: No mass conversion issues are associated to our implementation of pressure projection method. The procedure is described in Wang et al. 2008 and Danilov et al 2008. There are some energy conservation issues, but our code is not energy conserving anyway.

3. Is forward Euler really sufficiently accurate for use with GM?

Reply: GM is a parameterization, it cannot be accurate by definition, so the forward Euler is OK.

4. Page 3901 line 20: This language could perhaps be a little more precise, as Triangle has some non-free-libre license restrictions.

Reply: According to the Triangle web site, it is free, but "is copyrighted by the author and may not be sold or included in commercial products without a license", which still means that it is free for anybody to download. We do not see what has to be more precise here – we are not going to distribute it.

5. The definition in footnote 7 should be more precise. What is the z=0 level (a reference level, or ocean surface)? What is the range of k? The provided definition is dependent upon the division operator associativity.

Reply: the first layer is ocean surface. 1<=k<=N (added to the text). Changed to .../(2tanh..)

6. The GM parameterisation usually refers to the adiabatic eddy advection only, with Redi diffusion considered a separate parameterisation.

Reply: Like many others, we keep the convention of the original GM paper.

Other typos: Corrected, many thanks.

Sincerely, The authors

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