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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 enclosed below.

a. Manuscript does not address FESOM deficiencies: The most recent AWI paper I have read is by S. Danilov entitled "Ocean modelling on unstructured meshes." None of the challenges discussed in that manuscript are even mentioned here. This manuscript states in the abstract that "FESOM .... provides an excellent platform for further development" but my reading of Danilov (2013) concludes otherwise. I request that the au-

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thors reconcile their own papers in a rational and thorough manner! The specific points requiring reconciliation are too numerous to mention. I have difficulty agreeing that a model which requires global inversion to do 1D vertical mixing, does neutral physics along sigma-layers and can not afford to use z-star as a default vertical coordinate is a viable platform for the next decade of model development. I am willing to be convinced otherwise, but at present the evidence is lacking.

Reply: The focus of the paper is to describe "how to formulate FESOM to an OGCM". The model is currently used in numerous projects, and here we share our practical experience, but not to discuss the model numerics in details, which was done in other publications. The paper partly took a review form. We never argued that FESOM's numerical core is optimal, but its efficiency and accuracy is sufficient to be used in practice. Independent of possible updates of the model numerical core in the future, it is the experience in setting up and running realistic applications that is valuable and needs to be accumulated to guide future model development. The statement in the paper is based on this understanding. To be more precise, we changed the last sentence in the abstract, "and that its applications will provide information useful for the advancement of climate modelling on unstructured meshes".

Specific remarks: (a) Implementation of 1D vertical mixing in FESOM does not require solving global problem because on tetrahedral meshes the operator of second derivative in vertical direction couples only vertically aligned nodes. (b) Neutral physics is not applied in the sigma-grid part of the mesh because this part in our current practice is limited to the continental shelf and slope (around Antarctic), where the isopycnal slope is relatively steep and GM parameterization would be switched off within the currently used GM scheme. It is a common task in the sigma model community to adequately implement GM, and within FESOM we are not advanced in this aspect and just take the practical solution now. (c) We do not use z*, but apply ALE, now to the upper layer only. It is sufficient to implement moving free surface in order to use surface freshwater flux, the major motivation of free surface formulation.

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b. While point a. is very critical, this manuscript has much to offer the community. In particular, a forthright discussion of the positive and negative aspect of FESOM would be very valuable to the community. At the end of the paper, the reader still has no idea of what aspects of ocean modelling does FESOM excel and at what aspects is it challenges. In addition, the authors suggest that this manuscript will be valuable to other modelling groups. I agree. But, almost without exception, such value emerges from the discussion of a model's deficiencies.

Reply: Once again, we did not plan this manuscript as yet another paper discussing FESOM numerics, it is sufficiently discussed in our previous papers, where we had explicitly pointed to the issues stemming from continuous Galerkin finite elements. The form of this manuscript was partly inspired by the work of Griffies et al. (2005). The intention is to report on the status of the model (the stably used version) and discuss the experience gained in configuring FESOM in practice. While the issues of numerical core are important, not least important is to learn that the model, despite its very different numerics, simulates the global ocean circulation comparing well to that of other models on climate time scales, and that it reacts in a predictable way on parameterizations commonly used by the community. In our opinion, such knowledge is most valuable for FESOM users and for broader audience too.

We state in the manuscript that FESOM is the first unstructured-mesh model used as a global OGCM, but given our goal, the illustration of advantages brought about by local mesh refinement is beyond the scope of the paper. It should be provided by separate studies (see the cited work in the paper). We see the task of improving the model CPU efficiency as most challenging, and we are working on it. In the revised manuscript we add a sentence at the end of the paper to re-emphasize the issue: "Large model uncertainty as shown in the previous IPCC reports and recent COREs model intercomparisons (Griffies et al., 2009; Danabasoglu et al., 2013) indicates that model development requires long-term continuous efforts in the broad modeling community; Both international collaboration and individual effort from each model development group

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are necessary to advance the field. The model development team in our institute is continuing with the research on unstructured-mesh modelling issues related to both model numerics and physical parameterizations."

c. Manuscript reads like a modelling review, not a documentation of a specific model: The authors state that their focus in on unstructured grids, but I found no such focus in the text. Rather, I found the text to be wordy and not particularly tied to issues related to unstructured grids. For example, the paper devotes about 5 pages to the discussion of vertical mixing which, it appears, has no connection of the use of unstructured grids. At the same time, the fact that the FE method forces 1d vertical mixing to be computed using globally-connected matrices is not even mentioned. In another example, there is a full one-page discussion of virtual salinity fluxes before the reader is told what FESOM uses. Please focus on the text and remove generic discussions of ocean modelling.

Reply: Yes, the manuscript has the form of modeling review, but that was an intention. Unstructured-mesh ocean models have their special aspects, but model uncertainty could be fairly linked to all components of the model. Our focus is on the "formulation of a global OGCM" that can be used in practice. The issues related to unstructured-meshes (topography treatment, grids, lateral viscosity, eddy parameterization etc.) were discussed, but we keep other components at the same important level in the description, and indeed they all need specific consideration in an OGCM as a practice of model tuning. The 1D mixing inversion is mentioned in section 2 and not repeated in section 3.

d. Application of this model outside the polar regions: As best as I can surmise, the model has only been applied to problems in polar regions. Is this correct? If so, is there any modelling issues with applications to equatorial or midlatitude regions?

Reply: Our current applications are biased to polar regions, but FESOM participates in CORE-II intercomparison and its performance in other regions is analyzed (together

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with other models) in COREII papers to appear. Modelling issues, like vertical diffusivity and mesh resolution in the equatorial region, and lateral viscosity for the western boundary region, have been discussed in the current paper.

e. Inconsistency in model description: For example, the paper states that z-level grids are recommended and later discusses the merits of “uses the arbitrary Lagrangian Eulerian" approach (which it turns out is too expensive to use due to global matrices). Please tighten the text and provide coherency across manuscript.

Reply: The ALE is currently only used for the surface layer, so the grid is still z-level in principle.

Sincerely, The authors

Interactive comment on Geosci. Model Dev. Discuss., 6, 3893, 2013.