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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.

Anonymous Referee #2

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This article describes the latest iteration of FESOM, a global-scale finite element ocean model which is being developed for use in climate research. In general I find the development of FESOM extremely interesting, and I personally applaud the particular emphasis on practical large-scale applications of unstructured mesh methods. However I find the motivation for the developments in this article unclear, and key open questions are side-stepped or are given far too brief a treatment.

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.

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.

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

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

Minor comments:

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

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

3. Is forward Euler really sufficiently accurate for use with GM?
4. Page 3901 line 20: This language could perhaps be a little more precise, as Triangle has some non-free-libre license restrictions.
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.
6. The GM parameterisation usually refers to the adiabatic eddy advection only, with Redi diffusion considered a separate parameterisation.

Typos: Page 3894 line 13 ("wider range group"), 17 ("Despite of"), page 3895 line 2 ("allow to use"), page 3896 line 21 ("chosen the same"), page 3897 line 5 ("above sloping"), footnote 1 ("$A_v < 0.02$" should be "$A_v \leq 0.02$"), page 3900 line 14 ("prove to be"), footnote 4 ("one of possible"), page 3902 line 4 ("an triangle"), line 27 ("a suit of"), page 3904 line 16 ("This type of grids"), page 3097 line 3 ("In Weddell"), page 3908 line 4 ("of hydrostatic pressure"), page 3915 line 1 ("more importance source"), footnote 18 and elsewhere ("constrain" instead of "constraint"), page 3919 line 2 ("in case of"), line 18 ("gird"), page 3921 line 24 ("of Gulf"), page 3929 line 15 ("can have impact").

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