The Finite Element Sea ice-Ocean Model

Wang et al.

General Comments: Over the past years I have read the AWI/FESOM papers with great interest. As such, I was excited to have the opportunity to report on the model documentation. At present, the paper is lacking in several aspects that are detailed below.

Major Comments:

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 authors 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.

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.

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.

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?

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.