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Interactive comment on "Adapting to life: ocean biogeochemical modelling and adaptive remeshing" *by* J. Hill et al.

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This paper discusses the advantages of using a vertically-adaptive model to simulate the ocean biogeochemical behavior. The authors have implemented in the unstructured-mesh ocean model ICOM a vertical mesh adaptation technique that can take into account both the physical and biogeochemical state of the ocean. The main novelty of the paper lies in the investigation of a mesh adaptation metric that depends on biogeochemical factors as most (if not all) previous studies on similar issues focused on adaptation metrics that depend solely on the physical state of the ocean. In my opinion, this is a worthy study that would deserve to be published in OS Discussions.

My main reservation about this study relates to the fact that it is highly linked to one specific model: ICOM. It is not clear to me if the conclusions drawn from this study

would be applicable to other models that use different mesh-adaptation techniques (e.g. r rather h adaptation or different mesh-to-mesh interpolations or different FE/FV discretisations in space). Furthermore, this study is purely 1D and hence structured in the vertical. The authors claim that their goal is to later use such an approach in their 3D model to tackle more realistic problems. I am wondering whether that would work out so smoothly as they use a 3D model with a fully unstructured mesh made of tetrahedra. How could they be sure that the method they have assessed in a 1D framework would be applicable in their own 3D model? This is especially worrying since the authors mention in their conclusion that "for this pseudo-1D domain each column of the domain must be identical" (see p. 2019, l. 4-5). I think this is in contradiction with their claim that computational saving will further increase in 3D thanks to vertical adaptation (see p. 2018, l. 14). There are no clear indications that it will actually work in 3D. I would thus advise the authors to "tone down" such claims and provide more details about the extension of their approach to a fully-unstructured 3D framework.

Below are some more specific comments : * Section 4.1: I think it would be useful to see the actual formulation of the metric. I'm sure there are some threshold values on the different terms appearing in the metric that would be useful to the readers. Also, I guess that the authors use the gradients of the density and velocity. That should appear clearly in the text (see p. 2013, l. 9). * Section 4.2: What do the authors mean exactly by "consistent interpolation" (see. p. 2014, l. 12)? * p. 2016, l. 21: "the meshes contains" -> "the meshes contain". * p. 2021, l.5: I think there is a typo in Eq. (A10). * References: There seems to be a problem with the publication year of each paper. At the end of each citation, 2 or 3 different years appear. * References: Note that one third of all the citations come from the ICOM group. I don't think that this is very "healthy" and certainly suggests that this paper is a bit too "ICOM-centered".

Interactive comment on Ocean Sci. Discuss., 10, 1997, 2013.