

This manuscript presents a realization of the MIT gcm as a regional modeling tool. It is admirable to try to tackle a large geographical region such as the northern South China Sea. The model builds on an earlier iteration of modeling and thus I presume is already being used in one form or another.

I have used the MITgcm myself, and understand its utility in this context. My usual toolbox is more process study oriented and is typically high order. I am keenly aware that this cannot be the choice for the present authors. Nevertheless, the modelling presented in this manuscript requires more clarity.

The resolution is likely controlled by the need to resolve such a large area, but if the aim is to represent internal solitary-like waves, the large grid spacing needs justification. The reader should know how many points per wave a typical wave form, and how this compares to the standard in process studies. The resolution seems really low to me, but then I am one of those process study modellers I mentioned in my previous sentence.

Similarly the discussion of turbulence is a bit misleading. The authors quote a number of turbulence schemes applied at different scales and use this to justify constant eddy viscosity values in the vertical and horizontal. These values strike me as at least an order too high to me. The reader needs to know how a single ISW would be affected by these choices. Such model runs should take a day or

so, and their results can be summarized in a table. These would provide an important counterpoint to the rather ambiguous validations provided in the present version.

While not a theory paper, what is presented on ISWs is pretty dodgy. Gear and Grimshaw is a very old paper, the results of which have been superseded by other (often open source) tools. There are even monographs on the theory which would provide a more modern link to discussion, literature and codes.

The discussion needs to be cleaned up as some parts read very strangely. There are various tools the authors can use for this. In terms of content, the aforementioned turbulence models are presented as interchangeable when as a point of fact they are designed for very different things (i.e. Mellor-Yamada versus Gent-McWilliams). There are also strange statements about the source of turbulence that seem at odds with my understanding of ocean physics.

Finally, I was left wondering how the present methodology compares and contrasts with well established models like Getm-Gotm when applied to something like the Baltic Sea. The two tools are different in purpose, but it would help the context to contrast them.

I realize this is a rereview, but this is my first time seeing this manuscript, and it needs significant revision before it can be deemed “ready to publish”.