Geoscientific Model Development Discussions

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Comment on gmd-2021-427

Yanxu Zhang (Referee)

Referee comment on "The 3D biogeochemical marine mercury cycling model MERCY v2.0 – linking atmospheric Hg to methylmercury in fish" by Johannes Bieser et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-427-RC2, 2022

The Marine Hg cycle is an important component of its global biogeochemical cycling. Numerical models, especially 3D ones, are useful to reveal the interaction between transport and biogeochemical processes, interpret observations, and test hypotheses. However, the history of the 3D ocean Hg model is less than 10 years and there are only a handful of such models, which limits our ability to conduct multi-model intercomparison and adopt a model ensemble approach. This manuscript includes a detailed description of the processes and configuration of a numerical multi-compartment model for marine Hg cycling, MERCY v2.0. It presents the model evaluation from regional simulations of two shelf seas. The model also includes important and novel updates regarding Hg biogeochemistry, such as the S-Hg chemistry, larger Kd value consistent with field observations, sedimentation/resuspension, and the tentative inclusion of fish in 3D models. Overall, I consider it an important addition and advancement to existing ocean Hg modeling efforts, which merits publication in GMD. Congratulations to the author team.

Some suggestions and questions:

1. The model mainly follows a prognostic equation, and the authors describe each sub-term of the transformation term in great detail. Among them the implementation of the sulfur chemistry of mercury is novel, yet an evaluation of the importance of this newly added process seems to be lacking. How does it compare with observations?

2. The simulation of the bioconcentration process considers the biological uptake of Hg by organisms from higher trophic levels through the body parts exposed to seawater other than phytoplankton. Can you quantify the contribution from this pathway and via food consumption?

3. Although the model is claimed to be improved by employing a high Kd, the authors do not seem to have explicitly considered the effect of the biological pump on the mercury species at different depths. A single sinking velocity wd is utilized to calculate the vertical transport but the association between this and the biological pump was not given in detail. Nevertheless, including the sedimentation and resuspension in the model makes it more complete than previous models.

4. The quality criteria proposed in this manuscript entails sophisticated statistical analyses, and the elaborated presentation enables other ocean modelers to reproduce and apply. Also, the authors emphasize the importance of observational data and indicate that some processes are poorly constrained in the discussion. This can help field and laboratory studies address these issues.

Specific points (some may also be spotted by other reviewers):

1. Line 81, the authors state “The only real sink for Hg in the environment is a burial in the lithosphere mainly as stable cinnabar (HgS) in anoxic marine sediments.” However, there are several data suggesting that the sedimentation of compounds to organic material is a major sink in coastal and open-ocean systems. This may need the authors to include some references to address.

- Line 106, "red-dox chemistry" should be "red-ox chemistry".
- Line 115 and line 116, Rosati 2022 paper was mentioned twice but they did not appear in the reference list.
- Line 162, "en-to-end" should be "end-to-end"
- Line 171 Table 1, “GOM”, “PBM” - it would be better if these abbreviations be written out in full on first use.
- Line 270, "concentrationdependent" should be "concentration dependent". Line 271, "raction" should be "reaction". And it would be better to add the note on R12 about the remineralized organic matter concentration dependency.
- Line 295, is there a literature-based argument to support the use of negative oxygen concentration to represent sulfur ions concentration?
- Line 306, “chemistrcy” should be “chemistry”.

- Line 320, the square symbol in the formula should be the superscript of "T_w" instead of "w".
- Line 348, what does “R(C,B)” mean here? According to the previous introduction (line 188), it represents the transformation term in the prognostic equation?
- Line 353-359, 392-399 and 463, 559-563, the units (even unitless or 1) are needed to be written out in the description of the variables.
- Line 420 Figure 2, the labels of the color bar may have gone wrong. ‘60’ appears twice for different colors.
- Line 436, “due to the comparably low surface areas of these species”, do the authors mean diffusive uptake by zooplankton is less important due to the low surface-to-volume ratio of zooplankton? Since zooplankton generally has a larger diameter, thus larger surface areas but a lower surface-to-volume ratio.
- Line 455, readers may wish to know about the exact feeding relationship of “17 × Feeding rates for biological species (x) on species (y)” from Table 1, however, the predation related to fish is not mentioned here.
- Line 628 Eq. 44, what is “k1” here?
- Line 626 and 640, it seems that the stated “the measurement error to range from 20% (Hg0 and HgT) to 50% (MeHg)” is the same value as “U = measurement uncertainty” that is used to calculate MQO? It would be easier for the reader to understand if it could be phrased consistently.
- Figure 12: the title of the x-axis is missing.
- Figure 13: according to line 918, the lower panel should be the profile of the North Sea, but it is not listed in the caption.
- Figure 16: the caption and axes are not clear.