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Interactive comment on “Chemical and physical transformations of mercury in the ocean: a review” by N. Batrakova et al.

Anonymous Referee #1

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The paper goes through chemically and physical transformation processes for inorganic and organic mercury in the seawater and sediment. It is suggested that the review can be used as a guide for model developers. The idea to write a review-paper to help ocean Hg modelers is a good one, but unfortunately the manuscript omits critical literature and does not reflect the current understanding within the community. I therefore suggest that the manuscript is rejected.

The most central paper on methylation rates that has been published in the last couple of years, Lehnher et al. 2011 (Nature Geoscience), is not mentioned in the manuscript at all. This paper contains the most comprehensive work on rate constants for methylation and demethylation (photolytic and biotic) in the ocean to date. The fact that the authors are not aware of this study, which should be central in their review, is one of

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many indications that they are not up to date with the literature. The manuscript several places refer to sediment MeHg production as the most likely or an equally likely source of methylated mercury to the water column (page 4 line 8-12; page 20 line 16-18; page 26 line 9-15). The authors should read other recent reviews (and the studies referenced in these) that present recent evidence that water column production is the most important source of methylated mercury in the open ocean (see reviews by Mason et al. 2012, Kirk et al. 2012, Driscoll et al. 2013). I also suggest reading Blum et al. 2013 where the importance of subsurface production of MeHg is demonstrated using isotope measurements. I find it problematic that the review does not reflect the current understanding of the community on this subject especially as it is made to guide modelers on which processes they should focus on including in their models. In the last paragraph before the conclusion the authors refer to the relative stability of methylmercury in ocean waters and say that this stability is an indication that sediments and coastal area production of MeHg could be important for open ocean uptake of MeHg in fish. I am not sure what “relative stable” refers too but Lehnherr et al. (2011) finds that the lifetime of MeHg in the Arctic Ocean is 2-5 days, which indicates that methylmercury is far from stable and does not support long range transport of MeHg within the ocean but support water column production as the major source. Again the results from the Lehnherr et al. (2011) paper are never discussed.

In a review the authors needs to present a comprehensive knowledge of the current literature and an understanding of new insights within the field. Based on the manuscript I am worried that the authors do not have the necessary insight to publish a comprehensive review on this topic.

Interactive comment on Ocean Sci. Discuss., 11, 1, 2014.

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