

Interactive comment on “Biogeochemical processes accounting for the natural mercury variations in the Southern Ocean diatom ooze sediments” by S. Zaferani and H. Biester

Anonymous Referee #2

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This paper presents more details of a study on Hg fluxes around Antarctica that was recently published and uses correlation approaches to investigate the factors controlling Hg fluxes. The PCA results are interesting and the conclusions from this support to some degree the authors conclusions about what support the high sediment fluxes measured at this location. As in the Arctic, based on air-sea exchange literature, the waters are likely a net source of Hg to the atmosphere and not a sink for Hg given the low atmospheric inputs. Contrary to the Arctic, there is low inputs from the terrestrial realm and so the source of the Hg being removed is a critical question to be answered, which the authors do to some degree, but I was not totally convinced. The data are presented in the figures and are not discussed in any detail in the paper

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and it would be good to have some more discussion of the various figures and why some regions are highlighted. This is in the text at present but not well represented. My main criticism is therefore the calculations of Hg removal that is needed to explain the sediment fluxes and the fact that these calculations do not make sense in terms of sources (ln 212 onwards). The paper states that the inputs from lithogenic material is small and I agree with this, and also the atmospheric inputs are small compared to the fluxes. The authors calculate that a few blooms per year scavenging all the Hg out of the water but then state that “Formation of Antarctic bottom water which is linked to the polynyas (Ohshima et al., 2013) can rapidly “refill” the Hg inventory in the water column after a scavenging event.” How can the further formation of ABW water, which has its Hg removed because of the previous scavenging events replenish this water column Hg, and what is the timescale for this. To me, this calculation makes no sense. The external inputs of Hg are too small and the authors state that they are removing all the Hg from the water column. So, where is the Hg coming from to replenish. The authors need to make a more convincing argument and calculation to support his idea. I am not convinced, and the calculations just reinforce the idea that it is very difficult to support the calculated Hg fluxes in these sediments given the known water column concentrations and the magnitude of the external inputs. Additionally, the idea that “similar to CO₂, Hg fluxes from the atmosphere will increase during algae blooms as a result of continuous removal of dissolved phase Hg by diatom particles and the resulting shift of the dissolution equilibrium towards the dissolved phase which should additionally increase the Hg flux from the atmosphere into the water” (lns 220-223) is likely not true as based on the data in the various papers, (Wang et al., 2017; Nerentorp et al., 2017a; 2017b) which the authors should read and include their conclusions in their manuscript. Overall, the results of these studies suggest that evasion is actually higher during higher productivity so removing Hg, and is therefore a loss term that the authors don’t consider. Also, less ice leads to more evasion. Overall, the data do not support the contention in this section of the paper. The authors need to make a more convincing argument that there are sources of Hg

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to support the high fluxes they estimate I shared the paper with a colleague to help with the review process, who made additional editorial comments that are highlighted in the attached file.

Please also note the supplement to this comment:

<https://www.ocean-sci-discuss.net/os-2019-132/os-2019-132-RC2-supplement.pdf>

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2019-132>, 2020.