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Interactive comment on "A decade of methane measurements at the Boknis Eck Time-series Station in the Eckernförde Bay (Southwestern Baltic Sea)" *by* Xiao Ma et al.

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We thank reviewer 1 for the helpful comments that helped to improve the manuscript. Please find our replies to the general and specific comments below.

For characterising marine ecosystem shifts over time, especially in highly anthropogenically impacted regions, sustained time series data are invaluable, but such records are sparse. Their documentation is essential so papers of this type, in this case presenting decadal records of dissolved methane, dissolved oxygen and chlorophylla from the Boknis Eck time series site in the Baltic, are welcome. The Boknis Eck site is subject to severe eutrophication and is an active site of methane production so

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this paper has potential to provide important insights into methane temporal variability. As such this paper clearly falls within the scope of Biogeosciences. The authors represent a group that has a long experience of marine methane measurements and of working at the Boknis Eck site. Their methodology is well established and sound, and it is described concisely yet in enough detail to enable their reproduction by others. The observations presented are rather straightforward, and while no novel concepts or ideas are described the data are worth reporting and are adequately set into the wider context, citing relevant sources. Overall the paper is well structured and generally easy to follow, and the figures are clear. I was however, a little unclear as to the authors explanation of the unusually high surface methane observed in December 2014. They mention a major inflow at this time, of high salinity, oxygenated North Sea water but it was not clear to me whether they were implying this water to be high or low in methane (or the same) relative to in situ conditions. I think an additional sentence or two would help clarify this.

Reply: Thank you for your suggestion. A direct comparison of the dissolved CH4 concentrations in the North Sea and Baltic Sea would be necessary to assess the impact of the saline water inflow. According to the published results of Bange et al. (1994) and Rehder et al. (1998), CH4 concentrations in surface North Sea is much lower than in the Eckernförde Bay. Advection of water with high CH4 concentration does seem to be unlikely. We thus hypothesize that the MBI led to lower concentrations in the bottom water, substituting previously high concentration throughout the water column in the lower part below the mixed layer, hence causing the observed anomaly in the CH4 concentration profile. We will include the above information and the corresponding references in section 4.2.

They also describe a major outflow period in which sea levels declined prior to this inflow, and extreme weather that could have affected the sediment structures in the Eckernförde Bay. Presumably this could have led to methane release, but I think they stop short of saying this. Instead, they tend to favour hydrostatic pressure release due

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to the falling sea level as a cause of methane release from the sediments. It is not especially clear to me how this signal is transferred to the surface.

Reply: We suggest that enhanced CH_4 concentrations could be attributed to sedimentary release, and high CH_4 concentrations could be either homogeneously distributed all over the water column (via gas bubbles) or only detected at the bottom (via pore-water exchange) when the hydrostatic pressure decreased at first. The CH_4-enriched water was subsequently lifted to the surface by the saline inflow, which is heavier than the low salinity-water in the Eckernförde Bay. This is supported by the negative correlation between CH_4 concentrations and salinity in the water column. The decline of hydrostatic pressure could be one of the potential causes of the enhanced CH_4 release from the sediment. There might be other potential causes, for example, sediment re-suspension, resulted either from the storm or the flushing of the strong saline inflow, but this is not supported by the variation of Secchi depths. The occurrence of MBI is usually associated with storms and strong winds, but this is beyond the discussion of this study. We do not have any evidence and therefore, did not discuss the potential impact of the extreme weather conditions. We will add more detail in section 4.2.

Also, the hydrostatic pressure change, equivalent to the order of 1 metre in a 28-metre water column is rather small relative to the changes that occur in some estuarine and mangrove environments the authors cite. Can they provide evidence that such changes can produce the observations they describe? I wonder how important this mechanism might be relative to other possibilities.

Reply: Lohrberg et al. (2020) reported the detection of a widespread CH_4 ebullition event in the Eckernförde Bay in October 2014, shortly before the occurrence of the strong MBI. They demonstrated that storm-associated fluctuations of hydrostatic pressure induced the ebullitions and estimated a sedimentary CH_4 flux of $\sim$1900 μmol m-2 d-1, as a result of the changes in water level ($\pm$ 0.5 m) and air pressure ($\pm$ 1500 Pa, equivalent to approximately $\pm$ 0.15 m of water level fluctuation). Air pressure is not recorded at the BE time-series station, and we calculated the sea-to-air flux of $\sim$3100

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μmol m-2 d-1, with the changes in water level of $\pm$ 1 m. Water level fluctuation, when there was no strong wind or inflow event, was approximately $\pm$ 0.2 m in the Eckernförde Bay. Ignoring the CH4 oxidation in the water column, the sharp increase in sea-to-air CH4 fluxes in December 2014 are generally in good agreement with the sedimentary CH4 release reported by Lohrberg et al. (2020), which provides a strong evidence that the changes in water levels are capable of inducing such strong changes in CH4 release. We will incorporate this in section 4.2.

It has been documented for example that current flows across the seabed that could be induced by surface inflows in shallow water, can set up pressure gradients driving pore water flow (e.g. Ahmerkamp et al., The impact of bedform migration on benthic oxygen fluxes. JGR Biogeosciences https://doi.org/10.1002/2015JG003106). I think perhaps a little more in-depth discussion of the various possibilities would be insightful. For example, is it possible to estimate the amount of methane that would be expected to be released from the sediments over the duration of the hydrostatic pressure drop, and is this consistent with the observed effect?

Reply: Thank you for your suggestions. Porewater exchange might be an important benthic CH4 source, and we will add more detail in section 4.2. Sedimentary CH4 release via ebullition from Lohrberg et al. (2020) is generally consistent with our results. Please see the reply above.

The authors could perhaps also clarify why they chose to use a different equation for calculating flux densities (Nightingale et al., 2000) to that used in their earlier paper (Bange et al. (2010), i.e. Raymond and Cole (2001), which gives a lower gas transfer velocity. The authors point out that the two sets of results agree if the same equation is adopted but I was curious about their reasoning in selecting Nightingale et al (2000) for this study. I am not suggesting they are incorrect in this, rather I just wanted to know their reasoning.

Reply: We choose Nightingale et al. (2000) over Raymond and Cole (2001) because

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we would like to compare our results with other time-series analysis in section 4.4. As we discussed in section 4.3, there might be a great difference in flux densities originated from the different equations adopted. SI and ALOHA used Nightingale et al. (2000) and Wanninkhof (2014), respectively. Generally fluxes calculated from these 2 equations are close, and we choose the first one because it lies in the middle of many different gas transfer parameterizations, which makes it widely used and well-accepted.

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