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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.

Anonymous Referee #1

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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 chlorophyll-a 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 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. 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 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. 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. 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? 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.

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