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Interactive comment on "Upwelling-induced trace gas dynamics in the Baltic Sea inferred from 8 years of autonomous measurements on a ship of opportunity" *by* Erik Jacobs et al.

Anonymous Referee #1

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Carbon dioxide (CO2) and methane (CH4) are climate-relevant trace gases. Therefore, investigations of their distributions as well as estimates of their natural and anthropogenic sources and sinks have received a lot of attention during the last five decades. In general, the coastal oceans are an overall sink of atmospheric CO2 and an overall source of atmospheric CH4. However, getting a comprehensive picture of the CO2/CH4 distributions in coastal ocean environments is hampered by the fact that the seasonal and interannual variabilities are usually not well known or even unknown. To this end, the manuscript (ms) under review presents underway time series measurements of dissolved CO2 and CH4 concentrations from the surface layer of the Baltic Sea made on-board a commercial vessel commuting between Lubeck and Helsinki in

the period from 2010 to 2O17. The data set is used to show the effects of coastal upwelling on the distributions and air/sea fluxes of dissolved CO2 and CH4 in various (selected) regions of the Baltic Proper and the Gulf of Finland. Although I think that the ms presents a new data set of high relevance to address questions about seasonality and interannual variability of dissolved CO2 and CH4 in coastal areas such as the Baltic Sea, its major scientific objectives remain unclear. In large parts, the ms reads more like a technical or methodological report and thus needs considerable re-writing. Therefore, I can recommend publication only after significant major revisions.

Major points:

1) The introduction needs significant re-writing. It should give the basic scientific background why this kind of measurements and data analysis are done. Moreover, the overarching scientific objectives addressed by the study need to be given.

2) Section 2 'Data methods': I would like to suggest to move sections 2.2 and 2.4-2.6 to the Appendix. The information given in these sections is relevant only for side aspects of the data analysis. (Please note that Fig. 9 is already mentioned in section 2.4, so the numbering of figures is not correct, it should appear as Fig. 5)

3) Section 3 'Results and Discussion': Coastal upwelling as significant sources of trace gases such as CO2 and CH4 have been found in other coastal systems as well (for example in the eastern boundary upwelling systems off Oregon, Peru, Mauritania, NW Africa). Please discuss the results from the Baltic Sea in light of the results reported in the literature from other coastal upwelling systems. An overview table with saturation/flux data from literature may help to facilitate the comparison.

4) Section 3 'Results and Discussion': I am wondering if the authors could now quantify the significance of the contributions of upwelling-induced CO2/CH4 fluxes to the overall emission estimates of the Baltic Sea. And indeed, on page 18, lines 372-373, I found a statement on this issue saying this '[. . .] still needs further investigation.'. This is rather confusing (and disappointing) since the authors have the data sets at hand to come up

with some numbers to prove the significance.

5) Section 3 ‘Results and Discussion’: Moreover, I am wondering why the authors do not discuss the effects of the ongoing environmental changes of the Baltic Sea (such as warming, changing wind patterns etc.). An important question to be addressed might be: Are there any trends detectable for the upwelling-induced CO2/CH4 fluxes during the course of the study which after all covers eight-years? If yes, what are the main factors causing this trend?

6) Section 4 ‘Conclusions’: It is well-known that CO2 and CH4 are affected by upwelling in the Baltic Sea. This was already shown in publications by the same group (see Gülzow et. al., Biogeosci., 2013; Schneider et al., J Mar Sys., 2014) and thus it surprising to see this stated as a major conclusion (see page 23, 2nd paragraph of section ‘4 Conclusions’).

Minor points:

1) Section 2 ‘Data and Methods’ (and throughout the rest of the text): The authors use the term ‘saturation concentration’ which is misleading. This term should be replaced with ‘equilibrium concentration’.

2) Figure 1: Please indicate the location of the Uto station in the map.

3) P5L101-103: Please note that a concentration is only independent from temperature when it is given as mol kg-1. If it is given as mol L-1 (as in the ms) it is not independent from temperature. Moreover, the partial pressure is depending on the temperature when you refer to the partial pressure in equilibrium with the water phase. Please correct.

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