Thank you very much for the thorough review and your valuable recommendations and suggestions, which surely help to improve the manuscript.

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 2017. 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.

Thank you for your critical and very helpful assessment. We agree that the scientific objectives of our study have to be stated clearer in the introduction and that some points in the discussion are missing. Please refer to our answers below for detailed comments. In addition to addressing seasonality and interannual variability, we do see the development of techniques for upwelling detection and extrapolation also as important part of the scientific scope of the paper (in agreement with Reviewer #2). Nevertheless, we absolutely agree with you that these objectives have to (and will) be presented more clearly in the introduction.

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.

There is indeed room for improvement regarding clearly stating what the major objectives of our study are and we agree to rewrite the introduction accordingly. More specifically, we plan to:

- **Address the climatic and anthropogenic driving forces in the Baltic Sea, which lead to environmental changes (including reference to the BACC II report concerning upwelling frequency), which makes the Baltic Sea a good study site to see feedbacks early and to develop methods that can be used to analyse long-term data sets with respect to e.g. upwelling-induced trace gas dynamics.**
- **Add the issue of bad coverage of seasonality due to (biased) individual RV-based studies in contrast to the full coverage of a SOOP strategy as a motivation of our approach.**
- **Add model character of our study for similar sites and clearly indicating in the discussion which findings we expect to be applicable for the treatise of upwelling in other regions and which are specific for the Baltic Sea.**
- **Sharpening the introduction in general to interconnect all these points, clearly stating what the objectives are and clarifying what is out of scope, also taking your recommendations below into account.**

We are certain that this rewrite and restructuring of the introduction will improve the manuscript and hope it finds your approval.

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)

We follow this suggestion by moving sections 2.4, 2.5, and 2.6 to the appendix, as they address side aspects of the paper or data handling. (This also solves the incorrect figure labelling.)

Section 2.2, however, describing wind and model data, is a section dealing with two main input parameters of vital importance to the study. Thus, after thoughtful consideration and discussion, we would like to keep this section in the main part of the manuscript.

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.

Upwelling in the Baltic Sea – compared to oceanic upwelling – is not persistent, but episodic, and admittedly of less importance for the global budget and fluxes. This information is partly in the introduction already, but we intend to expand this, also with respect to the objectives of the study (see above). Our study is focused on pCO_2 and cCH_4, specifically (i) on their seasonality, interannual variability, and relaxation, (ii) on the drivers and possible feedbacks behind the observed dynamics, and (iii) on providing tools/methods for the community to deal with similar data in other upwelling areas, some of which might be more important in a global context. As stated above, we will expand this in the introduction to make our focus clearer. The last section concerning the flux estimate is intended to be an outlook on future perspectives, however, because the resulting fluxes are based on extrapolated rather than measured data and are, thus, in our eyes, not reliable enough to be compared with other data. Therefore, we would like to refrain from a comparison table, which is beyond the scope of the study focusing on the characteristics of our data set and methodological advancements.

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.

The phrase "needs further investigation" is indeed an unfortunate one. We would like to replace the sentence with: "Despite a more detailed analysis of the statistical prevalence of upwelling in this study, the question of the importance of upwelling on the annual trace gas balance of the Baltic Sea cannot be answered here based on the data available. Apart from the high variability within observed upwelling events, general statements on this matter are further complicated by little knowledge about fluxes in shallow areas (Humborg et al., 2019), large heterogeneities between basins (Gülzow et al., 2013), and the unknown CO_2 source/sink behaviour of the entire Baltic Sea (Schneider et al., 2014b). Answering this question in the future requires more knowledge on the Baltic Sea CO_2/CH_4 balances in general and extended insight into limitations of upwelling-induced flux estimates in the Baltic Sea (discussed in Sect. 3.5)." This should give proper justification to our statement. As stated above, we will also make clearer in the introduction that our main objectives are to find controls of the observed variability and dynamics within eight years, and to show ways to deal with extremely variable conditions in long-term data sets.

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?

Indeed, looking for trends in the data set was one objective of ours, which we will stress in the introduction, including information on environmental changes in the Baltic Sea. However, we did not find any trends on time scales of a few years as a result of the large variabilities that surpass any trends that might exist. This information is missing in the manuscript so far, so we added a short paragraph on this matter at the end of section 3.4: "It was not possible to identify trends in frequency or magnitude of enhanced pCO_2 and cCH_4 caused by upwelling events on time scales of a few years. The main reason for this are the high spatial and temporal variability of upwelling (and of several other processes with influence on dissolved trace gases) in the Baltic Sea, which surpass any trend

that might exist. Moreover, the observed endmembers of minimum SST and maximum pCO_2/cCH_4 are dependent on data coverage, which adds another layer of uncertainty to any trend analysis on the data set, especially in boxes around Gotland (two different ship routes) and during years with larger data gaps." We further plan to address typical/dominant time scales in the Baltic Sea, the well-above-global-mean warming of the Baltic Sea, and both potential mechanisms for enhanced CH_4 and CO_2 production as well as potential changes in upwelling intensity will be discussed.

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').

The criticised sentence was intended to provide context for the following paragraph, but you are right that it is an unfortunate choice of words. We will replace it with: "Based on the long-term SOOP data set, we identified controlling parameters of upwelling-induced trace gas dynamics in the Baltic Sea on large spatial and temporal scales:" This should indicate clearer which advancements have been made compared to previous studies.

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'.

Equilibrium concentration is in fact the more suitable expression, thank you. We will replace it accordingly throughout the manuscript, but will stick to the phrases "supersaturated / undersaturated" as these are clear within the context (we defined relative saturation as ratio of cCH_4 to equilibrium concentration) and are used frequently in the literature in similar context.

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

Done.

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.

We will correct/clarify both statements. We plan to replace with: "Note that – neglecting the very small influence of temperature on water density – we can handle the concentration (of CH_4 in nmol L^{-1}) as a quasi-conservative parameter with respect to temperature changes in the discussion. In contrast, the partial pressure (of CO_2) in equilibrium with the water phase is temperature-dependent (see also Sect. 3.3)."