Responses to Anonymous Referee #1

bg-2020-128

In this paper the authors combine off the shelf sensors for pCO2, pCH4, pO2 temperature and salinity into a flow through system and assess the utility of the system for measuring spatio-temporal variability of these parameters across the land-ocean interface.

Overall I found the paper to be well written and clearly presented. I have a few suggestion for improvement:

Response: Thank you for these comments!

1. To me, the pCH4 system with an apparent offset from standard methods, as well as an extremely long response time does not sit well within the stated aims of developing a system capable of detecting spatio-temporal variations across the land ocean boundary. Can the authors expand upon this, perhaps the pCH4 system described is advantageous for some experiments, but not so for others. Good to be upfront with the limitations as well as highlighting the benefits.

Response: Thank you for your comment. Concerning the offset from standard methods, as stated within the manuscript, the pCH_4 system accuracy is ± 2 µatm or 3% of the reading. For the brackish water campaign the sensor is within these specifications. For the limnic campaigns data are not within the 3% of the reading some of the time, as judged from comparison with discrete. We note and state in the manuscript that in a situation of high variability matching of underway data with discrete samples, one can have high matching uncertainty (specific lines 339-348) leading to apparent offsets which are partly incurred by inappropriate matching. It must also be noted that determination of dissolved CH_4 concentrations from discrete samples is also not fully mature yet, and have shown significant inter-laboratory offsets (Wilson et al., 2018; stated in the manuscript). We show in this study that with mathematical corrections however, the drawback of a long response time can be overcome. Please see line 307 ff. for very successful RT correction, where the corrected data vary in tight anti-correlation with the pattern of the O_2 data which have ~3 second response time. In addition to longer-term station deployments, where fast response time is not needed due to the slow change in pCH_4 concentration, we can therefore demonstrate the sensor's applicability in highly variable environments (Canning et al,. In Prep).

Of course, this system does have limitations, however when focusing on the advantages – long term stability or being able to pick up small variabilities with the response time correction (see section 3.2) while having continuous measurements in combination of other parameters – we believe this out-ways the limitations. We believe we have discussed and been open with limitations, however to make this clearer, a review of the manuscript has resulted in sentences becoming sharper based on this comment to ensure both the limitations and benefits have been presented.

Canning, A., Wehrli, B., and Körtzinger, A., 2020. Methane in the Danube Delta: the importance of spatial patterns and diel cycles for atmospheric emission estimates. *Biogeosciences Discussions* (soon to be submitted)

2. As the paper is currently written, it is hard to see what the advantage of the proposed system over the traditional methods for measuring these parameters. The description of calibrations, RT offsets etc are really useful, but I think a section dedicated to benefits over currently available systems would add value. This could cover aspects like power consumption, size, cost etc. For pCO2 - equilibrator-NDIR systems can cover an equally large concentration range, are cheaper, and have an equally quick RT. So essentially I am left asking, what are the benefits of this system for pCO2 measurements? Same for pCH4, although sensor cost is higher than CO2 NDIR and RT is also long (although quicker than reported for the Contros system presented here).

Response: Thank you for your comment. Within the paper the advantages (and limitations) of the system we feel have been addressed. The purpose was to combine and have multiple fully autonomous oceanic sensors to be able to measure across the boundaries, simultaneously. This can enable both oceanic and also limnic waters to be measured with the same system. Although there are separate sensors out there, these do not fulfill having all the necessary characteristics for both the ocean and inland waters: a system that measures all 3 gases completely autonomously, able to cope with a range of outside temperatures and variable conditions, non-demanding in terms of equilibrator systems and calibration gases, with only one person needed to supervise and conduct measurements in more demanding environments, while measuring simultaneously and being able to deal with the high accuracy needed in the ocean (of which these are tested, and are currently used in the ocean). The aim was all of these, while combined with being able to then also measure steep changes and high concentrations for all 3 gases. In terms of oceanic sensor prices, these are on the lower end, and portable enough to be adapted to inland water sampling. Therefore, specifically for pCO_2 measurements, we believe we showed the benefits of this system are stated above as we found it was able to cope in such challenging environments, yet could handle the accuracy needed, while measuring as a combined system. However, we can see your concern and sentences have been added into the manuscript with other systems for comparison and allow the reader to know what else is out there and therefore addressing the advantages and benefits of this system.

3. While the e-folding method of assessing RT is commonly used, I think it would also be useful to highlight the t90 values. This gives the reader a more relevant and directly relevant understanding of RT without having to do additional calculations to assess actual RT.

Response: As we recommend t_{63} and correct for it later in the paper, we have just added t_{90} data in for comparison when discussing the response time correction. For the HC CO_2 it would be 212s and for the HC CH_4 it would be 3145s.

A few typos Ref page 2 ln 39 change PA to Raymond

Response: This has been corrected, thank you.

Ln 281 The comment on no biofouling- later in the paper you highlight that biofouling may be responsible for some of the discrepancies (e.g. ln 317). Best to keep the message consistent

Response: On line 281, this is for no biofouling on the membrane itself, for line 317 it is for potential biofouling within the tubing, which was significantly longer for that specific cruise. I have changed it to make this clearer.

Ln 396 What is meant by "abnormal peaks", perhaps rephrase

Response: (actually line 369) Abnormal peaks is meant by small specific regions of increased concentration. This is rephrased to '…other regions showing large increases in CH_4 concentrations.'

Ln 384 - "from point of stationary", while I understand what is meant here, the terminology is a bit clunky, perhaps rephrase.

Response: Rephrased to 'However, during the second stationary zone measurements (Fig.11, 19/10) conducted within a lake, pCO_2 is shown to increase steadily during the station keeping while always remaining far lower than within the channel.'