Response to Reviewer 3 Comments (RC3):

Strengths: Given the pivotal role of N_2-fixation, the topic of this technical note is certainly relevant. Although the principles of the individual methods (gas equilibration and MIMS) have been used in other studies for similar applications, their combination and optimization for underway measurements is novel. Besides the obvious advantages of being able to derive N2-fixation rates and NCP over large areas and with potentially unprecedented temporal coverage, this approach might enable a better understanding of carbon and nitrogen dynamics in surface waters. Overall, the manuscript is well written, the approach followed is clear and the specific aims (1. Assessing equilibration times and full equilibrium; and 2. Assessing the system's performance in terms of precision, accuracy, limits of detection) are adequately addressed and substantiated with laboratory-based experiments.

Weaknesses: The major drawback of this contribution is that the authors present it in a way that it has been optimized for surveys on board voluntary observing ships (VOS), without providing data/experiments derived from an at-sea deployment. As it stands, the manuscript shows an assessment that the system is, in principle, capable of conducting measurements on such a vessel just as much as it could do in any other type of application. Beyond this, perhaps semantic issue, there are practical considerations that need to be accounted for when systems are installed in an unattended manner (as I am sure it is known to some of the coauthors). These include strong temperature variability (potentially affecting both hardware and software), potential contamination, vibration, biofouling, etc. Because of this, several parts of the text (starting with the title) can be considered misleading in the absence of direct evidence.

Overall, it is my opinion that this is a contribution worthy of being published after some issues are addressed. I would be reluctant to ask the authors for data from an at-sea deployment at this stage, but my recommendation would be to reformulate so that it is clear that their approach paves the way for further studies that do carry out the deployments on VOS.

Reply: We would like to thank the reviewer for acknowledge the value of our work and hope that the changes we applied will help to clarify the concerns. Among other things, we will present a more detailed description of the practical implementation of the measurement system on a voluntary observing ship (VOS).

Specific comments:

Throughout the text: I spotted a few format inconsistencies with the usage of chemical names (e.g. sometimes "O_2", sometimes "oxygen", and also not all subscripts are correct).

Reply: These will be corrected in the revised manuscript.

l. 1 – 3 (Title):

This approach can, in principle be applied to any survey type and in this manuscript no data from VOS is shown. I would therefore include this as a potentially useful application in the context of long-term observatories.

Reply: We will clarify in the revised manuscript that our approach is primarily intended for use on voluntary observing ships (VOS), while also acknowledging its applicability to

other survey types, particularly in the context of long-term observations. We have also revised the title to reflect this broader applicability.

l. 18 – 19:

(“The GE-MIMS is designed for…”): Perhaps the authors could describe this as a "proof-of-concept" in view of its future application to conduct observations in VOS.

Reply: We will emphasize that our measurement system is suitable for long-term observations in general using various platforms. However, it will be made more clear that VOS or similar platforms are essential for achieving the temporal and spatial resolution required for our approach, when investigating areas such as the Baltic Sea.

l. 84:

“provide” instead of “provides”

Reply: This will be changed.

l. 99 (Figure 1 caption):

To me most abbreviations were clear, but there might be readers not yet familiar with this kind of analytical setup. Therefore I would recommend the authors to include abbreviations also here (I noticed that they are used in the text, which is good, but some are far from the actual figure).

Reply: We appreciate the feedback and will ensure that all abbreviations are clearly defined in the caption.

l. 103 - 104 (“A pressure gauge (P2) was installed”):

I was wondering whether the authors could add some values (or an empirical threshold) here. This would be good both to ensure repeatability and also guide potential new users of this approach.

Reply: We will include a recommendation in the text, saying that for our setup the filter has to be replaced at 1 bar overpressure to ensure the safety and reliability of the system. This is a value we determined in laboratory tests using our method.

l. 113 – 115:

Virtually unattended deployment in a VOS will require a suitable alternative. I am guessing the authors might be able to provide useful suggestions on this.

Reply: We agree with the reviewer and will clarify in the manuscript that other alternatives to the Liquicel membrane other than Permselect were not investigated.

l. 150 – 154:

This information could be conveyed more clearly with a graph (e.g. an Allan plot).

Reply: We will include an Allan plot to discuss the results more clearly in our revised manuscript.

l. 215 – 249:

The full mathematical derivation is not novel and it seems unnecessary in this part of the manuscript. I would suggest the authors to shift this to an appendix.

Reply: We will move this part to the Appendix.

l. 258 – 260:

It is hard to grasp how the underlying assumption of no water flow could be directly applied to operation conditions in which indeed there will be seawater flowing through the system. In my opinion this needs further explanation.

Reply: The purpose of using a model that examines gas exchange with stagnant water was to provide a simplified representation for understanding the fundamental principles of gas dynamics. This approach was intended to help readers, especially those less familiar with the topic, grasp the essential mechanisms at play. However, in response to the reviewer comments, we have moved this model to the appendix to clarify its role and significance in our overall analysis.

l. 382 ("(…) denitrification in deep waters."):

A citation seems to be missing here.

Reply: We will remove the section 4.1 since the focus of our manuscript is on the determination of the N2 fixation and its importance for the surface water N budget.

l. 394 (“Ignoring vertical mixing (…)”):

This choice should be substantiated.

Reply: We will provide a more detailed explanation to substantiate this choice in the revised manuscript, by describing the surface stratification of the Baltic Sea during mid-summer and especially during periods that favor the development of cyanobacteria blooms, which indicate that vertical mixing might be ignored.

l. 434 (“(…) such that also currently used for continuous pCO2 measurements (…)”):

A citation seems to be missing here.

Reply: We will add the missing reference (e.g., Schneider et al., 2014 and many others).

l. 435 (“(…) will facilitate determinations of NCP”):

A further potential application of the approach presented by the authors would be to combine it with underway measurements of N_2O, since this might help further constraining uncertainties in O_2/Ar based NCP estimates (see Cassar et al., GRL, 8961–8970, 2014).

Reply: We thank the reviewer for the suggestion, which will be taken into account for future applications.

References (Authors)

Schneider, B., Gülzow, W., Sadkowiak, B., and Rehder, G.: Detecting sinks and sources of CO2 and CH4 by ferrybox-based measurements in the Baltic Sea: Three case studies, J. Mar. Syst., 140, 13–25, https://doi.org/10.1016/j.jmarsys.2014.03.014, 2014.