Comment on egusphere-2025-5050

General comments

This work is solid and worth publishing. The authors have performed a thorough analysis of the available dataset and are proposing data analysis techniques that are not commonly used for coastal or marine carbonate system chemistry, but their use is robust and provides the means to reveal the complex carbonate chemistry dynamics and potential driving mechanisms in the study area. Worth commenting that the authors advocate the need, importance and significance of long term high frequency measurements and observation programmes as a cornerstone to study how complex marine ecosystems behave and respond to multiple stressors.

The main drawback of the study is that it focuses on observations and data from a single location, from which assumptions on carbonate chemistry and carbon fluxes dynamics of a larger area are inferred. Moreover, the justification on how representative the sampling location is for the entire area (Wadden Sea) is rather thin.

Besides that, there are some minor points in a few sections that can be clarified and maybe phrased in a simpler way so that it's even easier for the reader to follow.

Some specific comments on the sections are mentioned below.

We thank the reviewer for their thorough and constructive assessment of our manuscript and for their positive evaluation of our work.

Regarding the main concern raised about how representative a single monitoring station, we acknowledge this is a valid limitation. As detailed in our responses to the specific comments below, where we address this point, we also point out the reader to Section 4.3 where the spatial limitations of our study and the need for broader basin-wide monitoring efforts are already explicitly discussed.

We have also addressed all minor points raised in the specific comments, as detailed below.

All replies below reference changes made in the track-changes version of the manuscript.

Introduction

Comprehensive and solid, setting the scene for what will be presented later.

Line 54: how accurate is "accurately"? Can the definition introduced by (Newton et al., 2015) be used in coastal carbonate chemistry?

We thank the reviewer for this suggestion. We acknowledge that the term accurately was vague in this context. Newton et al. (2015) define two measurement quality levels for ocean acidification observations: weather quality, sufficient to identify spatial patterns and short-term variability, and climate quality, sufficient to detect long-term anthropogenically-driven trends over multi-decadal timescales. These definitions are applicable to coastal carbonate chemistry as Newton et al. (2015) explicitly address shelf seas and coastal environments in their framework, despite the added complexity and dynamic nature of coastal systems compared to the open ocean. Our measurements meet the climate quality thresholds (Based on the calculated uncertainty in TA and DIC) and meet the weather quality for pH (based on the calculated uncertainty).

We have revised the introduction text (see line 54). We have also added the newton et al. (2015) reference to our reference lists (see lines 1095-1096).

Materials and Methods

The section is well written and clear. There are, however, 2 points, that might not sound constructive as to an extent the authors will not be able to resolve them easily, however they are worth mentioning. Any further elaboration and explanation from the author might strengthen the document.

The accuracy of the Temperature sensor (± 0.2 oC) is not the best and not ideal when used in carbonate chemistry and/or air – sea CO2 fluxes. On a similar note, the use of a bucket for collecting samples that will be analysed for DIC and pH.

Both points mentioned above can introduce errors. The temperature accuracy can be accounted for when evaluating error and uncertainty estimation, however the use of a bucket might give an error that will not be possible to characterize and account for. Saying that the statistical analysis and comparisons that are presented do give confidence that any introduced errors are more than likely minimal/non significant, however these points "stick out".

We thank the reviewer for these pertinent observations. We acknowledge that bucket sampling is not ideal; however, surface water at the sampling location is in continuous contact with the atmosphere prior to sampling. Given that samples were collected within seconds of deployment and at the surface, the additional atmospheric exposure during collection is negligible compared to the natural air-sea exchange that is already occurring. Therefore, any CO_2 exchange during the very short sampling would not introduce a bias.

Regarding the temperature sensor accuracy, we followed the reviewer's suggestion and propagated the ± 0.2 °C temperature uncertainty into our pH uncertainty estimation. The difference between the uncertainty with and without the temperature contribution was negligible, with a maximum difference of 0.0009, compared to the pH uncertainty range of 0.02–0.05. As the reviewer notes, the statistical comparisons presented in this study give confidence that any errors introduced are minimal and non-significant.

Additional remarks.

Section 2.1: As already mentioned in the general comments section, how representative is the sampling location for the Wadden Sea when one investigates carbonate system dynamics and biogeochemical patterns?

We thank the reviewer for raising this important point. We fully acknowledge that a single monitoring location cannot represent the full spatial complexity of the Wadden Sea, given its highly dynamic nature, with varying tidal forcing, freshwater inputs, sedimentary processes and biological activity across the basin. We would also note that even within the Marsdiep inlet itself, a single point is insufficient to fully characterize the spatial variability of the carbonate system. That said, the NIOZ jetty is located at the Marsdiep, the largest tidal inlet of the Wadden Sea and the primary exchange gateway between the North Sea and the Wadden Sea basin, making it a relevant location for investigating carbonate system dynamics at this critical interface. The variability in carbonate system dynamics observed at this single location, spanning tidal and seasonal timescales, already highlights the complexity of the system and the necessity for expanded monitoring across the basin.

We would like to point out that the spatial limitation is already explicitly acknowledged in the manuscript, both in Section 4.3, where we discuss the spatial heterogeneity across the basin in detail, and in the Conclusions, where we call for region-specific models and broader monitoring efforts across the different basins of the Wadden Sea.

Data Quality

Line 379: The authors mention: "… using different statistical metrics, including…". Are there more techniques/metrics? More than likely not so might need to rephrase and be specific.

We thank the reviewer for this comment. We have rephrased the sentence to be more specific, as these three metrics (R^2, RMSD, and NSE) are the only ones we used. See line 404.

Is this work following the methodology and equations presented in Nondal et al., 2009? If so to which extent? It's also not clear where the results are presented (e.g. is it table 2?).

We thank the reviewer for this comment. We have corrected the reference citation. The Nash-Sutcliffe efficiency (NSE) metric was originally presented in Nash and Sutcliffe (1970), and was applied in a similar context to ours by Nondal et al. (2009), following whose approach we calculated the NSE. We have updated the citations accordingly (see lines 405-405). Regarding where the NSE results are presented: the NSE values are reported in the text (lines 430-432) but not in Tables 1 and 2, which only show R^2, RMSD, and p-values for individual model components and residuals.

We have also added the following citation in the reference list (see lines 1039-1049):

Nash, J. E. and Sutcliffe, J. V.: River flow forecasting through conceptual models part I — A discussion of principles, J. Hydrol., 10, 282–290, https://doi.org/10.1016/0022-1694(70)90255-6, 1970.

Line 415: Does the term "positively" suggest "strong " or "similar" comparison or it also has a "positive" (i.e. increasing) direction? Might need to rephrase.

We have rephrased the sentence, to clearly convey that our results are similar to those of other coastal MLR studies. See line 444.

Line 459: How much is slightly? Same in lines 466 and 467 were the terms "minimal" and "substantial" are used. Can the authors provide numbers for them? In general, it might increase the clarity if such generic terms are not used (subject to editorial team as well).

We thank the reviewer for this suggestion. We have replaced the vague terms with quantitative values (see lines 489 and 496-499).

Section 4.1.2. Valid assumptions, which would have been even stronger if supported by oxygen data/information. Again apologies for not being very constructive, but having DO information would have provided a better picture for NCP and respiration. Surprised also that the nutrient data are not mentioned here rather in the following section(?)

We thank the reviewer for this suggestion. Regarding the dissolved oxygen data, an oxygen sensor was deployed at the jetty during the study period, however no oxygen samples were collected for sensor calibration. Given the dynamic nature of the Wadden Sea, and the well-known susceptibility of sensors to drift, using uncalibrated sensor data was not considered appropriate. Furthermore, given that our sampling is conducted at the surface, dissolved oxygen equilibrates with the atmosphere considerably faster than CO_2, making discrete oxygen sampling particularly challenging in this context, as collected samples may not accurately reflect the in-situ oxygen concentration by the time of analysis. We therefore chose not to include the oxygen data.

Regarding the nutrient data, we acknowledge that linking the nutrient data more explicitly to the biological processes discussed in section 4.1.2 could have strengthened the interpretations. Our nutrient data (Fig. S6 in the track change document) indeed shows the expected patterns associated with phytoplankton blooms, which support the bloom interpretations made in section 4.1.2. However, we chose to discuss the nutrient data in section 4.1.3 because their primary role in our analysis was to draw parallels with the results of Salt (2014) and to discuss biogeochemical processes driving TA and DIC variability beyond NCP, such as denitrification and sulfate reduction. Nonetheless, we have calculated the Redfield ratios and included them in the main text, in lines 593-595.

We also added the following reference to the reference list (see line 1118):

Redfield, A.C., Ketchum, B.H. and Richards, F.A. (1963) The Influence of Organisms on the Composition of the Sea Water. In: Hill, M.N., Ed., The Sea, Vol. 2, Interscience Publishers, New York, 26-77.

Figure 7; Some errors in the caption e.g. May is used instead of march, June instead of summer, etc. Please make consistent with Figure labels.

We thank the reviewer for catching these errors in the figure caption. We have corrected the caption to reflect the actual months shown in the figure (See lines 783).

Section 4.2.3. Nice work and Figure 9 has a very interesting approach!

We thank the reviewer for this positive feedback. We are pleased that the reviewer found our approach interesting.

Suggested references mentioned in the review

Newton J.A., Feely R. A., Jewett E. B., Williamson P. & Mathis J., 2015. Global Ocean Acidification Observing Network: Requirements and Governance Plan. Second Edition, GOA-ON, http://www.goa-on.org/docs/GOA-ON_plan_print.pdf.