Reply to R1:

We thank R1 for the positive review of our manuscript. We are pleased to find that R1 finds the work interesting and useful.
We find both the major and minor comments useful, and propose to incorporate them in the paper as described below:

L590-591: We acknowledge that this phrasing was unclear, and propose to rephrase to: "The results of our regression model analysis we feel confident that we can use the regression models defined in this study to predict the CO_2 flux variability in future EC-Earth3-CC scenario runs. Using the regression models for predicting the CO_2 flux variability in the North Atlantic on scenario runs from EC-Earth3-CC and potentially other ESMs would be beneficial for the understanding of the future climate system"

L591-592: We agree that using the regression models set up for EC-Earth3-CC on other ESMs might be complex and propose to include a discussion emphasising the possible intermodel differences and outline future perspectives with this in mind. A collaborative study across a number of ESMs comparing the primary predictors on a regional basis and their sensitivities (regression coefficients) could lead to an interesting synthesis and new insights.

L556-557: R1 rightly addresses the issue of correlated predictors which we address methodologically by limiting the number of selected predictors in the regression modeles by requiring a certain model improvement for each predictor. This could be explored further but we propose to add instead a cautionary note on the possible issues and our way around it which we find robust. For the MLD and winds it is true that they cannot be expected to be independent, but MLD is as a proxy for the upper ocean state and dynamics, whereas wind speed is directly influencing the CO_2 flux variability through the gas exchange equations.

L560-561: It definitely highlights the NSE region as an interesting region to study, and emphasises the need to investigate the dynamics of the smaller regions individually. Figure 2 shows the weighted mean CO_2 flux variability - the NSE is showing the highest variability and mean flux in relation to it's size, but the overall flux of the full North Atlantic is the greastest if you do not look at the weighted mean. The larger regions defined and regions influenced by sea ice in general show an interplay of processes where partly cancelating local anomalies also influence the average level of variability. A further stratification in sub regions is not considered constructive. We have balanced defining regions with (model dependent) dynamical characteristics and still geographically recognisable and partly established.

L569-586: Yes, we agree. We propose to include in the conclusion a statement on this finding along the lines suggested: "The main conclusion is that the CO_2 flux variability cannot be attributed to simple linear relationships with individual predictors but instead emerges from complex interactions among multiple processes."

We thank R1 for the rest of the minor comments, and if not commented above, they will be included in the revised manuscript.

Reply to R2:

We are pleased to read that R2 finds that 'the manuscript has the potential to be an important contribution to the community' and thank the reviewer for a thorough, constructive and generally positive review. We acknowledge the apparent need for a more clear motivation for our approach and scope of the paper as discussed by R2. By doing this it will also become clear why we have chosen the specific methods of analysis including working with indicators of ocean dynamics. We would like to note that this has not been raised as an issue with R1 which on the contrary nicely summarise and support our approach and storyline as follows:
"The novelty of the current work is the establishment of which predictors that can explain the CO2 flux variability in five regions in the North Atantic that are subject to quite different dynamic processes and atmospheric forcings."
"The concept of the work is interesting and useful, especially because it allows connecting basic variables and processes that can be obtained from different ocean climate and biogeochemical models commonly used to study climate change."
Still we see a need to better guide the readers as highlighted by R2. To address this we propose specifically to revise and clarify the introduction paragraph 1.1 (L104-122) clarifying the objective and logical progression of the sections.
Furthermore, other revisions suggested below will serve to address this concern. The replies will be listed in order of the review.

Relation to MLD:
We argue that MLD represents the process of vertical mixing, which is not necessarily represented directly by other indices. We have chosen to use MLD to be able to describe and include dynamical processes indirectly affecting the CO_2 flux variability such as ocean mixing. The different oceanic metrics and indicators will be partly correlated and interlinked through forcing and dynamics. See also comment to R1 on how we limit the predictors and regression models.

Relation to ΔpCO2:
Our focus on physical parameters is addressed above. Still R2 is correct that repeating the arguments for not including any non-physical parameters will be useful when it comes to the discussion on ΔpCO2 as an otherwise central and obvious parameter in describing the CO_2 flux variability. Rephrasing of section 1.1 L104-122 as proposed above is one step in clarifying this issue.

Section 4.3:
The authors agree with this point to some degree, and suggest to rephrase the discussion point to focus on the possibility of using the regression model to predict the CO_2 flux variability directly on scenario data from EC-Earth3-CC. However, we do believe that the regression models defined in this study could form a solid framework of explaining the CO_2 flux variability in future scenarios from other ESMs or even uncopuled ocean-only simulations. They might not be applicable directly, but could work as a starting point for explaining the future CO_2 flux variability and trends.
Both reviewers have commented on the discussions in this section and we will rephrase section 4.3 to modify the discussion with an emphasis on both R1's and R2's perspective. We propose to refocus the discussion towards using the regression models to predict the CO_2 flux in EC-Earth3-CC scenario runs, and to expand the discussion of using the regression models

on other ESMs. The authors agree that the regression models defined in this study will not be directly applicable to other ESMs, to be elaborated in the revised discussion section. Furthermore, we will include the relevant references kindly highlighted by R2 as a discussion point on the ability to understand and predict interannual-to-decadal variations in ocean CO_2 uptake.

We thank the reviewer for the minor comments and all of them will be incorporated in the revised manuscript as the authors believe they will improve the manuscript. Comments referring to major comment two has been addressed above. A few of the minor comments are commented below:

L130-152: We thank the reviewer for these clarifications and acknowledge that L133-136 was unclear. We propose to rephrase L133-136 to: "The configuration allows for simulations with emissions forcings, and the CO_2 flux is calculated from and proportional to the difference in partial pressure of (ΔpCO_2) between the atmosphere and the surface of the ocean (Döscher et al., 2022)." as this is the accurate description of the dataset used. We thank the reviewer for noticing the mistake.

L154-162: We thank R2 for the suggestion and will update figure 1 with a subplot showing the gridded CO_2 flux from Landschutzer, so it is possible for the reader to visually compare the observational CO_2 flux with the EC-Earth3-CC CO_2 flux.

L208-210: This sentence will be rephrased based on R2's comments to clarify the meaning and to enhance the readability: "The reproducibility of simulated intregrated fluxes (F) derived from other parameters (Eq. 1-3), particularly their variability across different timescales, provides a useful benchmark. It sets an upper limit on how much of the model variability we can expect to explain using physical quantities from archived monthly-averages data. "

L221-224: The authors also agree with this point and will rephrase as follows: "It is also expected that the CO_2 flux variability is dependent on SST and SSS variability, however the effect of SST and SSS on the solubility constant (K_0) is too small to be considered important in these calculations, and the SST and SSS components of Eq 1-3 is therefore not scaled."

L248-253: We thank the reviewer for pointing this out and suggest to rephrase this section to: "These parameters include parameters already presented above (SST, SSS, SIC, ΔpCO_2 and wind), however for the next part of the analysis we add mixed layer depth (MLD) and sea surface height (SSH). These parameters represent larger scale dynamics such as ocean circulation (SSH, gyre strength) and vertical mixing (MLD), which are candidates to be indirect processes controlling the CO_2 flux variability in EC-Earth3-CC."

Section 3.1: The authors agree with this clarification and has reworded the section using 'compares spatially' or 'mirroring' and not 'correlates'. Furthermore, we thank the reviewer for suggesting a new title for the section, which will be added in the revised manuscript.

L566-570: The authors agree that the description does not reflect the figures well, and have reworded the paragraph in the revised manuscript, focussing on the counter-intuitive patterns of the MLD.