11th July 2022.

Biogeosciences.

Dear Koji Suzuki,

Thank you for your and the reviewers' comments on our manuscript entitled 'Identifying the biological control of the annual and multi-year variations in South Atlantic air-sea CO_2 flux' by Ford, Tilstone, Shutler and Kitidis. We have addressed all of the reviewers' comments and implemented these in the new version of the manuscript. We provide detailed responses to all of the reviewers' comments below. In the responses we refer to page and line numbers in the tracked changed document. We hope that you find these changes satisfactory and acceptable.

Thank you for your time and we look forward to hearing from you.
Yours sincerely,

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Response to Anonymous Reviewer #1

Whereas the authors have responded to most of my comments, I still don't fully agree with some of the statements. I however feel that the paper's clarity, aims, and presentation of main results have improved to the level that it can be published.

Response: *We thank the reviewer for the second review of our manuscript. We have addressed your remaining comments below and hope you find the revisions we have made satisfactory.*

I appreciate the drop of NAO in the discussion. Maybe the comments and the discussion of the role of the Southern annular mode (as correctly stated, partially influenced by equatorial Pacific modes of variability) in flux variability could be improved.

Response: *We understand the reviewer's point. Unfortunately, further discussion of the SAM within the manuscript is limited by our analysis not resolving correlations south of 45° S. Keppler and Landschützer (2019) investigated the control of the SAM on CO_2 flux variability south of 35° S and showed this effect to be more pronounced below 45° S, and therefore outside the domain of this study. We have therefore added a sentence to highlight this limitation, on Page 14 Lines 337 – 339, which reads: "It should be noted that the effect of the SAM may be more pronounced outside the domain of the present study (i.e south of 45 °S; Keppler and Landschützer, 2019).". As the significant correlations between the SAM and CO_2 flux in this study occupied a region between 30 °S and 45 °S, we do not feel that further discussion on the SAM would be helpful.*

My main remaining comment is with respect to the equatorial upwelling: I understand the reasoning of the response, but I still don't think that this is applicable for the equatorial Atlantic (l. 420). There is no reason for the upwelled subsurface water to have maintained constant pCO2 (age of upwelled water from the time they were at the surface somewhere in the southern Atlantic Ocean has been known since the 1980s, for example, from CFCs and 3H/3He derived ages (Reverdin et al., 1993, JGR, https://doi.org/10.1029/93JC00976). There is no doubt that the bulk of this water has been at the surface 10-15 years before being upwelled, somewhere in the southern Atlantic Ocean. Furthermore, since then, regularly acquired DIC data at some regularly-sampled equatorial stations of the FRENCH PIRATA cruises in the eastern equatorial Atlantic indicate (together with earlier cruises) an increase in time of DIC over the last two (four) decades.

Response: *We agree with the reviewer's comment, and have now modified the sentence to say that the ΔpCO_2 trend in the Equatorial Atlantic should be ~0 under the assumption that the upwelled water's CO_2 concentrations are also increasing, due to recent contact with the atmosphere. These sentences, on Page 15 Lines 377-380, now read: "For the Equatorial upwelling, an increase in ΔpCO_2 (as shown here and in Landschützer et al., 2016) is counter intuitive because there is evidence that upwelled water has recently been in contact with the atmosphere (~15 years; Reverdin et al., 1993). Dissolved inorganic carbon in these upwelled waters has been shown to increase at a similar rate to the surface waters (e.g Woosley et al., 2016). Therefore, the trend in ΔpCO_2 should be ~0 with increasing $pCO_{2\,(atm)}$.".*

Response to Anonymous Reviewer #2

The authors followed most of my and the other reviewer's suggestions, and those that were rejected were properly substantiated, in my opinion. My main concern was about demonstrating the uncertainties in the parameters used in the CO2 flux calculations and the authors inserted them in the manuscript. Another concern, shared by the other reviewer, was about a more detailed explanation of the method for assessing seasonal and interannual drivers of dpCO2 and CO2 flux, and this was duly improved as well. I just suggest a last general review of the manuscript to correct some typos, such as lines 84 (where a ";" should be ":") and 459 (where "it's" should be "it is"), if this really is grammatical errors, as I am not very knowledgeable about English grammar.

Response: *We thank the reviewer for the second review of our manuscript, and are glad that are revisions to the manuscript were satisfactory. We have now corrected these two typographical errors and we have checked the complete manuscript for typographical and grammatical errors.*

References

Keppler, L. and Landschützer, P.: Regional Wind Variability Modulates the Southern Ocean Carbon Sink, Sci. Rep., 9, 1–10, https://doi.org/10.1038/s41598-019-43826-y, 2019.

Reverdin, G., Weiss, R. F., and Jenkins, W. J.: Ventilation of the Atlantic Ocean equatorial thermocline, J. Geophys. Res., 98, 16289, https://doi.org/10.1029/93JC00976, 1993.

Woosley, R. J., Millero, F. J., and Wanninkhof, R.: Rapid anthropogenic changes in CO_2 and pH in the Atlantic Ocean: 2003-2014, Global Biogeochem. Cycles, 30, 70–90, https://doi.org/10.1002/2015GB005248, 2016.