Response to reviews of the paper: "The impact of the South-East Madagascar bloom on the oceanic CO2 sink" by Nicolas Metzl et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-283-RC1, 2021.

Reviewer 2, Ahmad Fehmi Dilmahamod

Reviewer comments in italic, our responses in red

We thanks reviewer 2, Ahmad Fehmi Dilmahamod for his enthusiastic review and suggestions.

This study makes use of in-situ data from the OISO Project, among others, to understand the impact of the South-East Madagascar Bloom on the oceanic carbon sink. This is a first attempt to investigate how this large sporadic austral summer bloom can potentially contribute to the oceanic carbon sink. The physical drivers of this bloom have long been investigated with various possible mechanisms, although it seems to be coming down to a combination of a few factors which can contribute to the initiation of the bloom. On this note, the question of its impact on the oceanic carbon sink were previously raised but the lack of in-situ data hinders any research.

This very interesting study is well-structured and well-written. And with the right data, it provides new insights on the biogeochemical signature of this bloom. It clearly shows the difference between fCO_2 during a bloom and non-bloom (or low bloom) year, and that this difference is due to biological processes during the boom. And that it acts as a CO_2 sink (between -1.7 and -2.7 TgC/month).

Having said that, I am slightly less impressed with the reconstructed fCO_2 and air-sea CO_2 fluxes from Chau et al. (2021), which is still in review. I would expect that the Chau et al. (2021) is acepted before the current manuscript. I am also a bit puzzled because from Figures 11, S10 and S11, it seems that the impacts of the previous bloom years (1999, 2000, 2004, 2006, 2008, 2012-14) on the reconstructed fCO_2 and air-sea CO_2 flux are almost non-existent, whereas a significant drawdown is found for the 2020 one. I think that this variability from the CMEMS-LSCE-FFNNN is interesting, and deserve to be included and possibly explained in the text.

Response: The reviewer is correct pointing to the low variability of reconstructed CO_2 fluxes in the model during other periods when the bloom was present but not as strong as observed in 2019-2020 (Figures 11, S10, S11). As can be seen on Figure S12 the low variability is also seen when fluxes are integrated over a relatively large domain (45-70E) and it is also seen in the fCO_2 data when available (Figure 2). However, data are not available for all years and we can only conclude that in 2000, 2006 and 2012 the observed fCO_2 was not low compared to the atmospheric concentrations as observed in 2020 (Figure 2). To highlight the impact of the bloom in 2019-2020 we thus focus the comparison on fluxes first for December (Figure 10) as well as for annual flux integrated in the region (Figure 11). We will add more information on the apparent low variability of fluxes for the full period investigated here (1997 to 2019).

However, these do not take away the importance of in-situ data in this data-limited region and I am sure that the few comments can be easily addressed by the authors and that might help to improve this already good paper. Thus, I recommend this paper for publication, once the comments have been addressed, and the Chau et al. (2021) paper accepted.

Response: The comment concerning the paper submitted by Chau et al (2021) was also suggested by Reviewer 1. The paper submitted by Chau et al (2021) was not reviewed at the time we prepared our paper. However, the reviews for their paper were posted on-line in late September and the authors responded to the reviews last November 15th. At the time of this response (3/1/2022), the paper by Chau et al (2021) is not yet accepted but a revision is on the way.

Note that when we started this analysis (last year) we explored a first version of the same model described by Denvil-Sommer et al (2019) and Chau et la (2020) and preliminary results motivated the use of such a model for our purpose. The results of the first version of the model were also successfully used in another study focused in the Mozambique Channel (Lo Monaco et al, DSR 2021) and we were confident with this approach to explore the results of the model in the South-Western Indian sector. The model developed by Chau et al (2020, 2021) is an improvement of the first version by Denvil-Sommer et al (2019). Model results are under quality control of the European Copernicus Marine Environment Monitoring Service (CMEMS) and available for public use since 2019 (https://resources.marine.copernicus.eu/?option=com_csw&view=details&product_id=MULTIOBS_GLO_BIO_CARBON_SURFACE_REP_015_008, product DOI: https://doi.org/10.48670/moi-00047).

As the model has been updated, we thought it was more appropriate to use the new version of this model (Chau et al, 2021) extended to end-2019 that included the bloom anomaly well reproduced for December 2019 (Figures 10, 11).

Note also that results of the model by Chau et al (2021) have been used for global estimate of the ocean CO_2 sink in the last Global Carbon Budget (Friedlingstein et al, 2021; T. Chau is co-author and results presented at COP26 last November). Similarly, results of the CMEMS-LSCE-FFNN model (Denvil-Sommer et al 2019, Chau et al 2020) were also used in the previous GCB (Friedlingstein et al, 2019, 2020). Here we show the model is relevant for regional analysis, i.e. not only a view of the global ocean carbon sink as used in the Global Carbon Project. We hope that by the time our present paper is revised, the paper by Chau et al (2021) will be accepted.

Minor Comment:

On lines 318-319, the authors mentioned the presence of a clear signal of the SEMC retroflection. A recent paper by Ramanantsoa et al. (2021) discussed the early-retroflection, retroflection and no retroflection of the SEMC, and the impacts of the early-retroflection on the SEMB. I recommend including this citation is the discussion.

Response: Thank you, this is a good suggestion: we came across this article (Ramanantsoa et al, 2021) just after we submit our paper; these authors analyzed the complex circulation in this region for the period between 1987 and 2018 (before the bloom in 2019-2020) with a focus on the retroflection of the EMC. It is relevant to refer to this article when discussing the dynamics in this complex region and its potential impact on the SEMB. In this context, it is also relevant to note that in January 2020 (period of OISO-30 cruise) the EMC retroflection started before reaching the southern tip of the Madagascar Island, around 24°S (see figure R1) that corresponds to the so-called “Early Retroflection” as defined by Ramanantsoa et al (2021). We will add the information to the MS and the reference to Ramanantsoa et al (2021) will be added accordingly.

Note also that the reviewer’s suggestions concerning either the “Eddy” (reviewer 1) or EMC retroflection (reviewer 2) leaded us to revisit the ADCP data in January-2020. We suggest to add a new figure to the Supp Mat (ADCP data in January 2020 and current field in this region) to describe the circulation during that period and it’s potential link with SEMB. This figure is attached below (Figure R1).

;;;;;;;;;; References added in this response (not listed in the manuscript):

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Figure R1: Top: Meridional section (Latitude/Depth) of zonal current (U in $m.s^{-1}$) observed from ADCP data collected in January 2020 in the South-Western Indian Ocean (OISO-30 cruise, see the track in Bottom). A strong westward current down to 600m is identified around 27-29°S. Figure produced with ODV (Schlitzer, 2013). Bottom: Map of monthly surface current for January 2020 in the South-Western Indian Ocean showing the retroflection of the East Madagascar Current here around 24°S (one of the forms of the EMC retroflection defined by Ramanantsoa et al 2021) and its complex meandering structure deflecting southward and recirculating northward around 54°E. Bottom Figure produced from https://resources.marine.copernicus.eu/ (MULTIOBS_GLO_PHY_REP_015_004) last access, 15-Dec-2021.