

## **Review's comments**

**Manuscript Number:** egusphere-2025-1736

**Title:** The Atmospheric Potential Oxygen forward Model Intercomparison Project (APO-MIP1): Evaluating simulated atmospheric transport of air-sea gas exchange tracers and APO flux products

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The authors conducted an atmospheric transport model (ATM) intercomparison (TransCom) experiment by using APO as a transport tracer to evaluate the representations of the transport and fluxes. They used three air-sea APO fluxes and eight atmospheric transport models including the model variants to simulate spatiotemporal variations in APO. The annual means, seasonal amplitudes, and day of seasonal minimum of the simulated APO were compared to those of observed APO from the Scripps surface measurement network, several airborne meridional cross-section observation programs, and shipboard measurements made on transects crossing the Drake Passage. Intensive comparison revealed that there is large spread in the performance of the ATMs. Especially from the comparison of the global airborne measurements between observation and simulation, the authors succeeded in evaluating the validity of APO fluxes. Additionally, the authors, applying a previously developed framework to evaluate diabatic mixing rate to this study, investigated the simulation-based and observation based diabatic mixing rates and APO gradients. The result also clearly showed the ATM-related biases in the diabatic mixing rate and differences in the representation of APO fluxes.

Previous TransCom experiments, using atmospheric tracers like CO<sub>2</sub> with strong land sources and/or sinks, did not necessarily evaluate the atmospheric mixing above the ocean. Since APO is a unique tracer to study air-sea gas exchange, this TransCom experiment can provide a new perspective on the evaluation of the mixing processes in the ATMs especially above the ocean. I found that the paper is excellent, well organized, and contains material that should be published in EGUsphere. I highly recommend the manuscript to be published with the minor corrections as suggested below.

## **Specific comments:**

Page 1, line 25: Although air-sea CO<sub>2</sub> exchange seldomly affects APO seasonal cycle, it may be oversimplified to say that APO is a tracer of air-sea O<sub>2</sub> exchange. I think it would be better to reword the relevant wordings to “a unique tracer to study air-sea gas exchange” or something like that.

Page 3, line 84: There are two “Nevison et al., 2008” in Reference. (“Nevison et al., 2008” is also cited in line 442.)

Page 6, line 165-166: Is it possible to quantify the extent of the “minor effect” for the  $\pm 0.05$  change in the O<sub>2</sub>:C exchange ratio for the terrestrial biota.

Page 9, line 256: It would be better to change “the ERA5 reanalysis” to “the ERA5 reanalysis wind fields (Hersbach et al., 2020)”. Additionally, the reference should be removed from line 270.

Page 13, line 339: “Schuldt et al., 2021” is not listed in Reference.

Page 16, line 382-385: I think that it’s not easy to imagine the meridional gradients in the annual mean APO from Fig. 4A. It would be better to refer Fig. 5A here.

Page 16, line 389: In this study, the authors mentioned that the days of seasonal minimum are latitudinally uniform in each hemisphere. Is it the same for the seasonal maximum? As far as I know, onboard observation in the western Pacific revealed that there was a difference in the meridional distribution pattern of the phases between seasonal minimum and maximum (Tohjima et al., 2012). If the APO observations adopted in this study show different meridional distribution patterns between seasonal minimum and maximum, it would be better to describe it in the manuscript to avoid misunderstanding of the readers.

Page 482-483: I’m not sure what the sentence means. Figure 6B seems to show that the simulated SCAs based on CESM agree with the observation while those based on DISS are underestimated.

Page 24, Figure 6: Please check the titles of figures: “( C ) Column-average APO SCA” and “( E ) Column-average APO Seasonal Minimum Day”. Additionally, it would be better to add black line indicating the observation in the upper figure legends.

Page 29, line 664: It seems that only two key aspects are described in the paragraph.

Page 31, line 694-695: “(a-b)”, “(c-d)”, and “(e-f)” should be “(A-B)”, “(C-D)”, and “(E-F)”.

Page 37: line 807: I’m not sure why the authors indicate Figs. 8 and 9 here.

Page 40, line 925: I think it would be better to explain the “rectifier effect” in the manuscript or to add appropriate reference here.

Page 44, line 1025: Does the scaling factor of 0.82 partially explain the underestimation of the simulated SCA based on DISS flux? If it’s true, should it be mentioned in the manuscript?

Page 44, line 1025: “Naegler et al. (2006)” should read “Naegler et al. (2007)”.

Page 60, line 1410-1413: “Kenneth et al., 2021” is not cited in the manuscript.