Review's comments

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Title: Two decades of flask observations of atmospheric $\delta(O_2/N_2)$, CO_2, and APO at stations Lutjewad (the Netherlands) and Mace Head (Ireland), and 3 years from Halley station (Antarctica)

Authors: Nguyen, L. N. T., Meijer, H. A. J., van Leeuwen, C., Kers, B. A. M., Scheeren, H. A., Jones, A. E., Brough, N., Barningham, T., Pickers, P. A., Manning, A. C., and Luijkx, I. T.

I found the authors revised the manuscript properly in accordance with the most of the reviewers' suggestions and comments. However, I think that there are some ambiguous and/or erroneous descriptions in the revised manuscript. Therefore, I think that several points addressed below should be clarified before acceptance for publication in Earth System Science Data.

Thank you very much for your comments. We hereby address them in details below.

Specific comments:

Page 9, line 283-286: This paragraph is very important for this study because the stability of the CIO scale is discussed here. However, I think that some additional figure or table should be required to conclude the scale stability of "less than 3 per meg over the 14 years". This is because the differences in the $\delta(O_2/N_2)$ value between WT5279 and WT6168 increased to 7.3 per meg from MREF6170 period to MREF6123 period as listed in Table 2.

We have now included a graph showing the annual averages of the 3 WTs over the years, along with the fitted trends and the slopes of the trends. From the graph, the year-to-year variability of the cylinders shows the stability of our internal scale better than just the averaged values during different MREF periods.

Page 15, line 457: Is "20-year period" right? Or "17-year period"?

This is now fixed to 17-year period

Page 15, line 474: Is "the COI scale stability (13.5 per meg in 14 years)" right?

This is now fixed to less than 3 per meg in 11 years

Page 21, line 594 (Figure caption): "diagram"

This is now fixed.

Page 22, line 607-615: If my understanding is correct, the land and ocean sinks reported by Friedlingstein et al. (2021) do not include the riverine flux. The correction of the riverine flux is applied only to the ocean sink estimate based on the ocean pCO_2 observations in Friedlingstein et al. (2021) (see section 2.4 Ocean CO_2 sink). Additionally, in their study,

global ocean biochemistry models (GOBMs) are used to evaluate the anthropogenic ocean sinks, which are the additionally acquired ocean sinks from the natural ocean condition, in which the ocean is considered the CO_2 source due to the riverine flux. As the authors discussed in the manuscript, the land and ocean sinks based on the observations of the atmospheric O_2 and CO_2 do not take into account the riverine CO_2 flux. (It is considered that the land biomass is the source of the riverine carbon, which is accompanied by O_2 consumption.) Therefore, those fluxes should be directly compared to those reported by Friedlingsteine et al. (2021) without the correction of the riverine flux (0.6 Pg yr^{-1}).

We would like to politely disagree with this comment. First of all, the land and ocean sinks as reported by Friendlingstein et al. 2021 are taking into account the adjustment of the riverine flux to derive the fCO_2 (previously pCO_2) based estimate which feeds into the reported ocean sink, as this is the mean of the GOBMs estimate and the data based fCO_2 estimate. Also, the atmospheric inverse results in Friedlingstein et al. 2021 are adjusted in the similar way as the fCO_2 estimate. This is because these methods are based on contemporary observations, are therefore it is necessary to remove the pre-industrial ocean source of CO_2 to the atmosphere of 0.61 PgC/yr (see also Hauck et al. 2020). The same logic holds for the $\delta(O_2/N_2)$ estimate.

Page 22, line 614: “higher” should be “lower”.

This is now fixed.