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# *Interactive comment on* "Global carbon budgets estimated from atmospheric $O_2/N_2$ and $CO_2$ observations in the western Pacific region over a 15-year period" *by* Yasunori Tohjima et al.

**Anonymous Referee #1**

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In this paper, the authors present recent global CO2 budgets based on precise measurements of atmospheric O2/N2 and CO2 in the western Pacific region. The data quality is excellent and the authors' effort to maintain the long-term observation over the wide area is highly commended. Although the paper does not provide particularly surprising new findings such as the equatorial bulge and mid-latitudinal trough of APO reported by the authors' past studies, the data itself is noteworthy and the presented results constitute an important contribution towards an independent validation of the global carbon cycle reported by Global Carbon Project (GCP). The paper is well written and clear. I recommend this paper for publication in ACP with a few additional minor revisions below.

1) The uncertainties of F and Z_eff should be presented explicitly somewhere in the main text and/or Tables. Did the authors assume the uncertainty of 100% for Z_eff following Keeling and Manning (2014)? Also, for those readers not familiar with $O_2/N_2$ studies, it would be better to present the representative values of alpha_F for the period 1998-2016 or the respective values for the periods in Table 2.

2) The discussion on the evaluation of the needed interval to suppress the temporal variability in Z_eff is useful in deriving reasonable interannual variations in $CO_2$ sinks from $O_2/N_2$ observations. The needed interval was estimated to be 5 years in the paper, and the authors used the ocean heat storage of 0-2000 m layer to estimate Z_eff based on the gas flux / heat flux ratio reported by Keeling and Garcia (2002). However, I think the circulation time of ocean deep layer water is much longer than 5 years. Please explain why the authors have considered the use of heat storage of 0-2000 m to be more reasonable than that of 0-700 m. I suppose there is an implicit assumption in the analysis that the ocean circulation and oxygen concentration are in steady-state from the surface to the deep layer. However, temporal variations found in the 5-years average Zeff in Table2 suggest that the ocean is not in steady-state.

3) I think it may be helpful for the reader to note the differences in land and ocean $CO_2$ uptakes expected from the 3% difference in the span sensitivity between NIES and SIO. Has the conclusion about the comparison of the $CO_2$ uptake reported by the present study with those by GCP changed significantly due to the difference in the span sensitivity?

4) The authors have compared land and ocean $CO_2$ sinks estimated in the present study with those obtained by GCP, with and without the imbalance sinks added. It seems to me that the authors conclude that the differences between the present study and GCP are reduced, both for land and ocean $CO_2$ sinks, by adding the "total" imbalance to the respective sinks. However, I actually think we can only add the imbalance to the land and ocean $CO_2$ sinks based on an appropriate differential distribution. I understand it would be difficult to suggest the best distribution due to uncertainties of

the estimated $CO_2$ sinks, but I would like to hear the authors' thoughts on this.

5) P3, line 32: A literal error "...heat content,." should be corrected.

6) P7, line 19: I think the unit of Z_eff is not TgC yr-1 but PgCyr-1 in this context.

7) References: Please consolidate the format of references. For example, some journal titles are written in Italic and the others are not.

8) Caption in Fig.7: The phrase "changing ratio" should be changed to "changing rate".

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