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Biogeosciences Discussions

Interactive comment on "Climate impacts on the structures of the North Pacific air-sea CO_2 flux variability" *by* V. Valsala et al.

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Reply to the comments from Reviewer-2

Thank you so much for reviewing our article titled "Climate impacts on the structures of the North Pacific air-sea CO2 flux variability" submitted to BGD. Your review was highly valuable for us to make a revision and strengthen the scientific value of the manuscript. A point wise reply to each of the comments is given below. For your reference the comments are given below.

Comment-1

A fundamental problem with the study is that it did not appropriately reference a number of the data products that were included in the analysis, and this problem of not

appropriately referencing dat sources continues here. For example, were any sources from JAMSTEC, the MRI or other international CLIVAR cruises used? This type of omission can result in a de-valuing of the contribution that data makes to science.

Reply-1

We understood that the manuscript lacked a direct referencing to individual papers that contributed the data set of global ocean pCO2. However, the work of Valsala and Maksyutov, 2010 used LDEO data base referenced as Takahashi et al, 2007, version 1.0 comprising of 236 777 observations. By stating this reference, we are further referencing all subsequent documents cited in the Takahashi et al, 2007. Not only the JAMSTEC or the MRI data, several other cruises contributed to the LDEO database. Therefore it is impractical to include all the references.

We do agree that, in the previous work of Valsala and Maksyutov, 2010, the DIC data derived from three cruises in the north Pacific from the MRI was not properly cited. That was an omission happened there.

Comment-2

The citations to other papers in the north Pacific and citations to inversion CO2 flux papers.

Reply-2

We apologize for the omission of citing relevant papers in the north Pacific, especially in this study. We added two paragraphs in the introduction where we add related works from observational aspects.

Comment-3

Usage of Telszewski et al., 2011. In our article.

Reply-3

We do not agree with this view point of reviewer in saying that the data obtained by Neural network is not providing any scientific insight for this study. The reviewer argues that that data should not be included because it was not published as of the time of writing of this manuscript. However, the reviewer also found that the data itself is important. The authors consider that this important piece of data should not be removed from the analysis only because it is not published, and we note the editor that the manuscript preparation for the data publication is underway. The Neural network method of that data actually shares from a previous paper of Telszewski et al, for the North Atlantic where they published their methods. Therefore Telszewski et al., 2011, manuscript under preparation; is an extension of their previous north Atlantic work. The author believes that the scientific input of this particular data is countable although the data are of much short time scale. It provides an entirely independent measure of pCO2 compared to other data presented in the manuscript. Therefore we retained the analysis in the manuscript.

Comment-4

In the first paragraph of section 4, the authors assert that: "Our analysis. . .". I am curious to know if the authors are emphasizing as the main point of this study that the observations reflect a mix of secular trends and natural variability. . .

Reply-4

This is an interesting comment from the reviewer. In the view of Takahashi et al., 2006, we, however are unable to conclude completely that the secular trends are indeed affected by interdecadal variabilities. Moreover, our study focuses only on decadal scale analysis but not on trend analysis. However comparing the magnitude of trend reported in Takahashi et al., (2006) (i.e. roughly 10 ppm per decade) is somewhat comparable with the interdecadal variability described in our study. A precise separation of natural modes from secular trend is difficult from our study alone. However, we can still usefully point out that secular trends may have some modulations of decadal variability,

and those may be obscured in a sparse resolution observations. Our focus is not on the seasonal trends of CO2 fluxes in the north Pacific. In a deseasonolized, detrended data, we state that we can see visible footprints of PDO. Therefore commenting on seasonal trends is not possible with the present analysis.

Interactive comment on Biogeosciences Discuss., 8, 4239, 2011.