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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.

Anonymous Referee #2

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GENERAL CONSIDERATIONS:

Generally speaking, I am in favor of the general spirit of this paper - it sets out to be an overview or synthesis of a number of products relating to the North Pacific carbon cycle. However, the manuscript suffers from a number of serious limitations, and as a result of these deficiencies I recommend that the manuscript be accepted for publication only after undergoing significant revisions.

First and foremost, this study relies on the product that was presented in the study of Valsala and Maksyutov (2010). A fundamental problem with that 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 data sources continues here. For example, were any data sources from JAMSTEC, the MRI, or the international CLI-

VAR cruises used? This type of omission can result in a de-valuing of the contribution that data makes to science. This problem must be fixed in order for this paper to be considered for publication.

Secondly, on a related note, it is very peculiar that the authors have not included references to a number of studies that have come out of the MRI relating to observations of carbon. The multiple papers of Midorikawa et al. come to mind, as do the papers of Murata and Ishii, but there are a number of authors that are scientifically appropriate to reference here. In addition to referencing these studies, these data should prove to be of great value in "skill assessing" the data and model products considered here. It is surprising confusing that the introductory section includes references to atmospheric inversion results, but does not include appropriate references for ocean DIC and pCO2 measurements, especially from the Japanese community. The inversion references strike me as being completely irrelevant here, and the ocean data is completely relevant. I suggest that this be fixed, and a paragraph be included in the introduction that summarizes the results of previously reported pCO2/DIC data for the North Pacific. At the end of the manuscript, the authors should comment on whether their analyses with the different data products has helped them to interpret these data.

Third, again, on a related note, I am very strongly of the conviction that the results of the unpublished analyses of Telszewski et al. (2011) should not be included in this study. This is for two reasons - first of all, it doesn't add at all to the scientific arguments presented in the paper, and secondly, that work should be presented in an original publication where the methods are presented in full. Taken together, these considerations make a convincing case that this analysis does not at all belong in this publication. It is a distraction that weakens that paper, and I strongly urge the authors to remove this during their revision process. Again, the reason for doing this is to strengthen the science issues raised in the paper, rather than to distract from them.

It is critical with synthesis activities to appropriately reference existing published work, and to not include work such as that of Telszewski et al. (2011) in a study such as this where it serves as a distraction. It does nothing to support the questions raised about the carbon cycles over the timescales emphasized in the Abstract, especially for the case where the Telszewski et al. (2011) work has not yet been published. It is specifically because the work of Telszewski et al. (2011) is interesting an important in itself that it needs to be removed from this manuscript, and submitted independently. Doing that will strengthen the science of the respective publications.

In the first paragraph of section 4, the authors assert that: "Our analysis puts forward a recommendation that we should take into account the natural variability while assessing the secular trend in the pCO2 of the North Pacific, especially when they are found with short term observations. Our analysis suggests that such trends could be a part of the larger interannual to decadal variability"

I'm 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. If that is the meaning of this first paragraph of the Discussion section, then the authors should appropriately reference previous studies on the observational side that make this point. Clearly this point has been made with data as well as with observations. For this point, the authors should reference the North Pacific study of Takahashi et al. (2006) that focused on North Pacific trends in pCO2.

The study of McKinley et al. (2006) clearly made this point, and this should be mentioned in this paragraph. The study of Rodgers et al. (2008) on seasonality in CO2 fluxes in the North Pacific should also be discussed. In that study it was pointed out that the secular trend in pCO2 and CO2 fluxes projects differently onto winter and summer trends, and thereby the seasonal cycle is important to understanding the secular trend. In other words, one should be careful in separating timescales through filtering, as this can obscure the underlying mechanisms controlling the surface ocean carbon cycle. The study of Gorgues et al. (2010) substantiated that this separation into trends that are distinct during winter and summer can be expected even in the presence of a cyclo-stationary ocean circulation state. For the model-products and data-products

considered here, are the trends in winter and summer pCO2 and CO2 fluxes different in a statistically significant way? Or are the records to short to address this question?

To reiterate, the synthesis presented here of how different methods reproduce different characteristics of the North Pacific carbon cycle should prove to be useful to the research community, and for larger synthesis efforts. However, given that the lack of appropriate referencing of other studies and publication on North Pacific carbon cycle, I would like to see the revised version of the manuscript.

The manuscript and the science contained in the manuscript will be strengthened through a balanced presentation of what has been previously been done in North Pacific carbon research.

SPECIFIC COMMENTS:

(1) p.g 4241, line 15: Do the authors mean "El Nino years" rather than "ENSO years"?

(2) p 4246, line 7: What do the authors mean by "reasonable"?

(3) p. 4247, lines 24-26: It is probably not correct to refer to a warm phase persisting from 1977-2008; the authors should consult the work of McPhaden and Zhang on the 1997/98 shift.

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