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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 #1

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Summary

The authors use four different data sources to investigate the air-sea CO2 flux in the North Pacific region. The focus is set to natural modes of variability, i.e., the PDO and partly ENSO and their impact in the CO2 fluxes.

General recommendation

In general I think the study delivers useful insights of the air-sea flux of CO2 on the regional scales and is this suitable to be published in BGD. The manuscript is well structured, may be a bit too long and has some technical short comings regarding the figures. However, before potential publication I recommend to substantially revise the manuscript, as I found a number of major shortcomings, which needs at least a deeper discussion.

Major comments

I. On major concern with the analysis arises from the fact that the authors use a rather short time series. I understand that this is the best we have but still 25 yrs are at the limit to say something about potential connection between CO2 fluxes and climate modes like the PDO which has its preferred variability on time scales of 20-30 years. On page 4250, the authors state that they detrend the time series before calculating correlations. This is in principle correct as the statistics assumes stationarity, however given the fact that the authors use only 25 years and the dominant time scales of the PDO is 20-30 yrs, I think that the authors will remove relevant variations which mit illustrate the connection between PDO and CO2 fluxes. So a suggestion is to show at least the trend patterns which are removed so that the reader gets some insights on this problem. Note also that the PDO has a negative trend from relative positive values in the 1980s to negative values at the beginning of 2000.

II. Page 4247, section 3.1: I am a bit puzzled about the separation of the seasonal and interannual component. The seasonal component is obtained by applying a harmonic filter to the time series. This is a rather unusual procedure (assuming a yearly cycle to be close to sine or cosine). Normally one would just estimate the means seasonal cycle by averaging all Januaries, all Februaries, . . ., separately and then subtract this cycle from each year. Maybe this has not an effect on the analysis but still I would be pleased to see the seasonal cycle (at least in the point-to-point response).

III. Page 4253, analysis of the variability (> 6.5, < 6.5 yrs): I think this analysis could be removed (or strongly shortened) as 25 yrs are not enough to make statistically meaningful statements. Applying a low-pass filter of 6.5 yrs to a 25 yr time series leads to degrees of freedom of the order of 3 to 4, so I doubt if the correlations are significant in Fig. 6.

IV. On important general problem is that pattern of the PDO (more or less a dipole pattern) and the "four-pole" pattern of the CO_2 fluxes shows not a good correspondence. So, I think a clear mechanism how such a dipole pattern (PDO) could generate a four pole pattern in the fluxes is needed. The authors present this partly but maybe a schematic overview in the conclusion could help.

Minor comments

1. Page 4244, line 3: Missing blank. 2. Page 4244, line 4-7: This is more or less a repetition of page 4243 line 21-27. 3. Page 4244, line 9: I think a line break after "fluxes." will increase the readability. 4. Introduction: I think the seminal paper of Latif and Barnett should be included in the introduction when investigating North Pacific decadal-scale variability: Latif, M. and T.P. Barnett, 1994: Causes of decadal climate variability over the north Pacific and North America. Science 266, 634-637. Still the introduction could be shortened a bit. 5. Page 4249, line 19: I think it would be helpful for the reader to write how Mantua has defined the PDO and derived the PDO index, used in this study. Also explain how the pattern looks like (or even show it). 6. Page 4249, line 21: Please avoid the phrase "cycle" as it could be misunderstood that there is a process (PDO) which follows a sine/cosine function (which is not found). The authors could write that the PDO shows enhanced variability on the decadal spectral band of 20-30 yrs. 7. Page 4251 / Fig. 3: The authors select different boxes for the different data sets, so how comparable are the time series? 8. Page 4254, line 14-17: Please explain in more details how the partial correlation works, one could also think of applying a regression analysis between ENSO and CO_2 fluxes, subtract this linear connection from the data and analyze the residual with respect to the PDO. 9. Page 4255, line 25: Why using a 12-month running mean before computing CEOF. The authors include by this method an artificial autocorrelation of the data which might affect the CEOF analysis. 10. Page 4256 line 6-8: The sentence "Therefore, the dominant SST . . . controlled by PDO." does not make sense. The dominant SST mode is the PDO. 11. Page 4259, line 6-8: It is unclear how the authors estimate a net sink from the regression pattern. If it is a sink or a source depends on the product of

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pattern and index. 12. Page 460, second paragraph: I wonder how the signal could instantaneously penetrate so deeply in the ocean as suggested by the correlation. Is it due to a high auto correlation of the water masses. Have the authors checked for lead and lags between PDO and DIC.

Technical comments

In a lot of Figures (1,3,5,6,7) the patterns seems to be contorted. So please change to the original aspect ratio (to have a better orientation with the contours of the continents. Fig 3: Very small labels at all figure axes. I can not believe that correlation of 0.1 are statistically significant at the 10% level (please correct this through out the manuscript, either significant at the 10% level or 90% confidence interval), given the fact that you has 25 years so roughly 25 degrees of freedom. Fig.4/5: Same significance level as Fig. 3? Fig. 6: will be probably removed (see major point III), but if not please increase the labels of the color bars and change orientation similar in Fig.3. Fig. 8 Please increase the labels of the color bar. Fig. 9-11: The longitude labels are not visible. I also recommend to change the color bar from rainbow to the color scale used in Fig. 8 (red – white – blue), as in particular at the top panel we only see green areas. Also all labels are too small. Fig. 12: I am puzzled why the explained variance is increased when using 3 variables rather than only two of them. This is an unexpected result as each variable added will introduce some noise to the method. The color bar of the middle panel is missing, the CEOF consists of three pattern not just one so please show the missing ones. Fig. 14: The contours are invisible, the labels too small.

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

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