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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-1

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. The reviews are thoroughly undertaken. A point wise reply to each of the comments is given below. For your reference the comments are given below.

Comment-1

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.

Reply-1

This is a very thought-provoking comment from the reviewer. Actually when one de-trend the data for the interannual signal analysis, because of the nature of analysis here (i.e. PDO vs CO2 fluxes), which might need a longer record of 50 or 100 years, the detrending may remove some of the useful trends related to PDO. Therefore it is important to show whether the trend patterns of the data for the given period has correspondence to the low frequency interannual variability examined in this paper. Following the suggestions of the reviewer, we have calculated the trend patterns of the individual boxes under concern. The Figure 1 attached with this reply note shows the trends of individual boxes which were used in the recognition of PDO footprints in the north Pacific CO2 fluxes. The figure illustrate that the trend pattern has no distinct polarity as the PDO footprints explored in the manuscript. This clearly shows that the de-trending has not removed the useful variability in the CO2 fluxes which are directly related to the PDO. We thank the reviewer for noticing this important point to our attention. Because these trends are not contributing to the patterns explored here, we did not include this figure to the manuscript, but included a sentence mentioning this aspect.

Comment-2

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).

Reply-2

We partially agree with the reviewer that the usual procedure of separating the interannual signal from a data series is by removing the climatological mean. However, this method will result the interannual anomaly with noise embedded in it. It is also possible that the interannual signal obtained with this method can be taken to harmonic analysis to separate the noise. But that eventually end-up in the harmonic filtering which is essentially our method (see also Ballabrera-Poy et al., 2003). However for consistency here we demonstrate the seasonal and interannual variances obtained by the method suggested by the reviewer. Figure 2 of this response note shows the interannual and seasonal variances which are obtained by subtracting the seasonal climatology form the time series of CO2 flux from each grid point. It is apparently visible that both the variances has identical structure and scale as given in our manuscripts. However, using our method, we were able to further separate the error variances from the interannual variances. In Figure 2 in this response note the interannual variance also embeeded the residual errors because only the seasonal cycle is removed from it. The Figure 2 here and the Figure 1 of our manuscripts are consistent to each other. Therefore this picture is not included in the manuscript to avoid redundancy.

Comment-3

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.

Reply-3

The authors believe that the separation of modes into low and high frequency responses north Pacific CO2 fluxes are worth including because, the PDO itself has strong modulation from ENSO (see text Section 3.3 and related references therein). Although the 25 year span of data may not be completely resolving the PDO signals, it can effectively resolve the PDOs high-frequency modulations of ENSOs. And that is the hypothesis behind the illustration of CO2 responses into two time scales in Figure 6 of the original manuscript. This section not only separate the modulation of PDO footprint by ENSO but also provide an analysis of dependent and independent patters of ENSO by winter time PDO analysis as well as partial correlations. Therefore, the authors believe that it is essential to include this section at the given length especially because our study is the first one that identifies the PDO footprints of CO2 in the north Pacific its regional characterization. The section 3.3 complements the two important components (6.5 years below and above) of this variability.

Comment-4

IV. On important general problem is that pattern of the PDO (more or less a dipole pattern) and the “four-pole” pattern of the CO2 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.

Reply-4

The authors partially agree with the reviewers that the study itself does not completely resolve an answer why PDO footprints on CO2 fluxes has four-box polarity while PDO itself is of two box (subtropical and mid-latitude) polarity in the north Pacific. However, as the reviewer himself/herself pointed out, we are trying to explain it by combined EOF analyses. The complete identification of the mechanism would require a state of the art biogeochemical GCM study which can switch various mechanisms into on and off states and reproduce the four box patterns. However, our study points out, first, there is a footprint of PDO on CO2 flux in the North Pacific. Second, we examined its causes by examining two major (highly possible) components of it such as dynamics and thermodynamics. The combined EOF of SSH, SST and CO2 flux indeed resolved the four-box polarity of CO2 fluxes related to the PDO. The result itself stand as a proof of the mechanisms behind it. However, the two sided polarity of PDO versus the four-box polarity of its footprint on CO2 fluxes require an answer.

'The response of BGC is not only physically driven', can be best chosen as an answer to this question. The carbon cycle has dependency on SST, dynamics, mixed layer depth, precipitation, SSS, Alkalinity etc, and the surface pCO2 relation to each of these variables or mechanism can be quite non-linear. A combination of these relations can turn into a different response in the regional patterns of PDO footprint in CO2 flux than the shape of SST or SSH itself.

However, constructing a more general schematic based on our analysis does not seem appropriate because, such generalizations should only emerge from a complete analysis of the mechanism such as based on B-OGCMs. On the other hand, our study putforward a suggestion that PDO has a footprint in north Pacific CO2 fluxes, and that has been consistently proved by four data sets.

Comment-5

The authors select different boxes for different data sets, so how comparable are the time-series?

Reply-5

The "slightly" different boxes opted for the three data set presented in the Figure 3 is justifiable considering the level of complexities involved in the individual models. Each of these models are driven by different set of wind forcing as well as with varying levels of complexities of ecosystem. Therefore, assuming a unique box in these three data product is not strictly necessary. What is important here is to show the regional patterns of PDO-CO2 flux relations has four polarity. The box boundaries, however, is not exactly fixed in this study.

Comment-6

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.

Reply-6

We partially agree with the reviewer's this comment. The 12-months smoothing might induce some autocorrelation to the CEOF pattern. However, the leading mode of SST pattern obtained in our CEOF is consistent with the PDO related SST pattern. Although the resulting CEOF elevate the correlation, the patterns are unlikely to change if we remove the 12-months smoothing from the data prior to the CEOF analysis.

Comment-7

It is unclear how the authors estimate a net sink from the regression pattern. If it is a sink or source depend on the product of pattern and index.

Reply-7

Figure 13 represents the regression of CO2 flux anomalies onto the standardized PDO index, which has unit of mole/m2/yr. This can tell us the net source or sink in the PDO related CO2 footprints.

Comment-8

I wonder how the signal could instantaneously penetrate so deeply in the ocean as suggested by the correlation.

Reply-8

The correlation depicted in Figure 14 is produced with the interannual anomalies of DIC and PDO. In this case the vertical penetration to a depth of mixed layer can be by autocorrelation of watermasses. However, beyond the mixed layer depth, the penetration of correlation pattern might be due to the PDO forcing only. Please note that the correlation reflects only the interannual to interdecadal variability, therefore the response itself doesn't have to be instantaneous. Persistent forcing of PDO over a longer period can cause the penetration of PDO signals to the deep levels.

Answers to Technical comments.

The figure aspect ratios are changed. They are stretched to appear correct aspect ratio.

Figure 3 panels of correlations are found with 244 data points of monthly anomalies. Therefore 90% level of significance are nearly 0.1. Only the PDO index and CO2_PDO indexes are smoothed by 12-month running mean, whose d.f. will reduce to 25.

Figure 4/5 are also shown at same convention as Figure 3.

Figure 12 is corrected with variance value 16%, added the missing color-key. The pattern of CEOF is only for CO2. Others are similar to the one for corresponding SST and SSH shown in previous figures.

Captions:

Figure 1: Trends of CO2 flux from 1980 to 2004 shown for the regions of four boxes where the PDO footprints of CO2 fluxes were identified. The inset on each box shows the area averaged CO2 flux trends from the individual boxes.

Figure 2: Interannual and seasonal variances obtained using method suggested by the

reviewer. They are identical to the corresponding variances shown in the manuscript. Note that the interannual variances here embed the error variances as well.

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

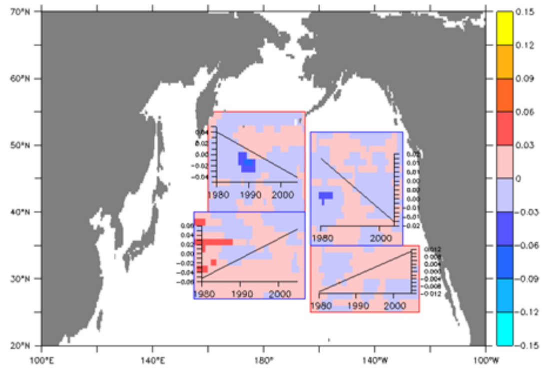
70°N
60°N
50°N
40°N
30°N
20°N
100°E
140°E
180°
140°W
100°W
0.15
0.12
0.09
0.06
0.03
0
-0.03
-0.06
-0.09
-0.12
-0.15
1980
1990
2000

Fig. 1.

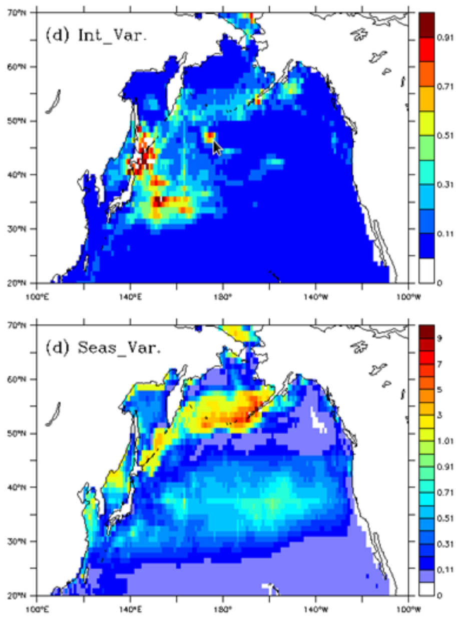
(d) Int_Var.
(d) Seas_Var.
70°N
60°N
50°N
40°N
30°N
20°N
100°E
140°E
180°
140°W
100°W

Fig. 2.