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Ocean Science Discussions

Interactive comment on "ENSO-correlated fluctuations in ocean bottom pressure and wind-stress curl in the North Pacific" *by* D. P. Chambers

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Thank you for your comments. I will review them briefly below and then give my response.

1. Comment: Several papers addressing interannual variations in the North Pacific are not discussed or reviewed. Reply: I am aware of all of these papers, but did not include them in the review as they focus on upper ocean processes and not ocean bottom pressure. However, because they do discuss the relationship between low-frequency variations in the winds in the area and upper ocean processes, it is appropriate to add them briefly to the discussion. I have added severeal sentences to the Introduction to

comment on the previous studies, and then point out that they were examining upper ocean processes, while the studies of Song and Zlotnicki, Chambers and Willis, as well as this one are examining the smaller variations related to mass (OBP) variability. The new text is below: " There have been numerous studies describing interannual variations in the upper ocean temperature, salinity, and sea level in the North Pacific (e.g., Qiu and Joyce, 1992; Miller et al., 1998; Fu and Qiu, 2002; Qiu, 2002; 2003), with most of the variations linked to wind-forcing and the Rossby wave reponses that are confined to the upper ocean (Fu and Qiu, 2002). A substantial portion of the internnaul variability has been tied to the Pacific Decadal Oscillation (PDO) (Qiu, 2003). Qiu (2002), based on an analysis of time-dependent Sverdrup balance along with an assumption of constant bathymetry across the North Pacific, concluded that there were no low-frequency fluctuations of mass in the area related to wind forcing. However, changes in topography over the region are quite substantial, and have been shown to cause trapped modes (Ponte, 1999). More recently, Song and Zlotnicki (2008) used a wind-forced model with realistic bathymetry and demonstrated significant interannual variations in OBP related to WSC in the region. They also concluded that there was a small, barely significant correlation between both WSC and OBP with an El Niño/Southern Oscillation (ENSO) climate index at a lag of 0 to 2 months." 2. Comment: In order to study the relation to ENSO, the author performs an EOF analysis of the GRACE data. Does the method used to reduce noise as it is based on projecting onto model EOFs therefore have an effect on this type of analysis? Are there any circular effects when using the method on the data as well as for the analysis?

Reply: The reviewer misunderstood that in the EOF analysis (Figure 2 and related discussion in Section 3) the analysis used the model data, not the GRACE observations. Note that the time in Figure 2 goes back to 1992 (10 years before GRACE was launched). I will add a note in the caption to Figure 2 that the OBP is from the model. "Figure 2. Leading EOFs (top) and principal components (bottom) of WSC from CCMP (left) and OBP from OMCT (right)..." and also change the beginning of Section 3 to read: "The leading EOF of non-seasonal WSC from the satellite data and OBP from

OMCT over the North Pacific is shown in Figure 2. The spatial mode is similar to the pattern of seasonal variability for WSC, with the largest variations occurring just north and south of 35°N." 3. Comment: The analysis is mostly statistical and does not include a detailed physical explanation behind the processes leading to the trend in wind stress curl.

Reply: While I agree that an examination of the cause of the interannual variability in the winds (and especially the reason why ECMWF does not see the trend that the satellites do) is warranted, I believe it is beyond the scope of this paper, and would require a significant effort that is better included in a follow-up study. In fact, there is quite possibly a coupling between the winds and low-frequency variations in ocean heating that would require a fully coupled atmosphere-ocean model to fully diagnose the physical mechanisms. The goal of this paper was to assess the ENSO-correlated low-frequency variations in OBP and WSC first identified by Song & Zlotnicki , quantify whether the correlation was robust for all periods, and to understand whether the 3-5 year trend at the end of the record identified by Chambers & Willis was related to this ENSO variation. I believe that this paper as written meets all of those goals.

4. Comment: "On page 1633, line 10-11. Ponte (1999) studies seasonal variability in OBP: how does this relate to interannual and longer term variability as discussed in this manuscript?"

Reply: The point of the line in question is that the variability is intensified in the western portion due to trapping by the bathymetry, which was first identified in the seasonal study by Ponte. It was also shown by Song and Zlotnicki and Figure 2 of this paper that a similar intensification occurs in the same area for interannual periods. This sentence is included to point out to readers unfamiliar with the region why the OBP variations are expected to be larger in this area.

5. Comment: " On page 1637, line 7-8 state that the EOF analysis allows to estimate the contribution of ENSO to the signal."

Reply: The Reviewer's points about portions of the ENSO signal being possibly projected onto other modes without them being correlated with an ENSO index is a good one, and is the main reason we go beyond the EOF analysis. We merely use it to identify the regions with the highest correlation with ENSO in order to identify areas where we examine the full time-series in more detail. We have modified the sentences in question to be more qualified in the assessment:

" The EOF analysis also allows one to estimate the fraction of variance explained by ENSO in the area, at least the amount of ENSO that is projected onto the first mode. The leading mode explains slightly more than 50% for both WSC and OBP in the area of 35°N-45°N and 160°E-185°E, where the amplitude of the OBP variation is the largest (indicated by the box in Figure 2). Thus, although the leading mode does explain about half the low-frequency variance of OBP in the model, it is suggestive that there are additional low-frequency variations uncorrelated with ENSO."

6. Comment: " On page 1637, line 19- page 1638, line 13, the author comments on ENSO eventsthat do not coincide with OBP or WSC fluctuations in the North Pacific. Providing a physical explanation or a suggestion on what physical processes might be involved and why, would help to strengthen the message of this study."

Reply: Again, while I agree that such an analysis is warranted, I believe it is beyond the scope of this paper, and would require a significant effort that is better included in a separate follow-up study. The main goal of this study was to reconsider the analysis of Song and Zlotnicki, who focused mainly on two events, the large ENSO events of 1982/83 and 1997/98, and concluded ENSO causes large OBP and WSC variations in the North Pacific. I wanted to show that the correspondence is not as robust as they concluded and that there are several identifiable periods of El Nino and La Nina where there was not an extreme WSC or OBP anomaly, as well as periods where there was an an extreme WSC or OBP anomaly when there was an ENSO. Thus, the relationship between the WSC/OBP in the North Pacific and ENSO is more complicated than suggested by Song and Zlotnicki.

With that said, it is likely related to modulation by the PDO, and we will add the following sentences at the end of that paragraph:

" It should not be surprising that there are interannual variations other than ENSO in this area, since the Pacific Decadal Oscillation causes low-frequency changes in the winds over the area (Mantua et al., 1997; Qiu, 2003), and there is evidence of the PDO modulating the amplitude of ENSO events (Yeh and Kirtman, 2005)."

7. Comment: References listed but not in the text.

Reply: Thanks for finding these. They were remnants from an earlier draft. However, in the revision, we have used them as references.

Interactive comment on Ocean Sci. Discuss., 8, 1631, 2011.