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Interactive comment on "Seasonal variability of the Ekman transport and pumping in the upwelling system off central-northern Chile (~ 30 S) based on a high-resolution atmospheric regional model (WRF)" *by* L. Bravo et al.

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We thank to Dra. Catalina Aguirre for her time and constructive comments. We carefully read each of the comments and will address them as stated below:

Specific comments

1.- Something that perturbs me is the use of the term "coastal divergence" exclusively for the Ekman Transport. Ekman transport occurs due the divergence caused by the presence of the coast, Ekman pumping occurs due the divergence in the coastal region caused by the spatial variability of the wind field. Thus, both physical processes are finally due to coastal divergence.

In the manuscript the term "coastal divergence" is used exclusively for the coastal upwelling due to the divergence caused by Ekman transport out of the coast and under the presence of the coast. This is different to the vertical transport that may result from the wind-stress curl, as different authors have recognized (see Graham and Largier, 1997; Capet et al., 2004; Castelao and Barth, 2006; Jacox et al., 2014)

In general, Ekman pumping (positive) is caused by the horizontal divergence of the Ekman transport (represented by the vertical component of the cyclonic wind stress curl) without requiring a physical boundary (like the coast in the coastal upwelling or the change in f in the equatorial upwelling).

We understand the essence of the comment that the reviewer's specifies, therefore to give the reader a better understanding we have clarified this in the manuscript.

We propose a modification in the introduction as it follows:

In the eastern boundary current systems wind-induced upwelling has mainly been described using two primary mechanisms. The ïňĄrst one is coastal divergence which is the result of offshore Ekman transport due to alongshore winds (with an equatorward component), earth's rotation and the presence of the coast (i.e. coastal upwelling).

2.- In the abstract and results, authors mention that both alongshore wind stress and wind curl show a clear seasonal variability with a marked semiannual component. Could authors propose a mechanism that produces that semiannual component?.

Indeed, both, atmospheric simulation and satellite observations show a semiannual variability in the along shore component of wind stress, but certainly weaker than the annual variability (Fig. 1). In the case of the atmospheric model this is observed in Figure 4 (manuscript), with a secondary maximum that occurs between March and April. However, the mechanism that produces semiannual component is not clear to us.

OSD

12, C1716–C1722, 2016

Interactive Comment

We have not found any study in the southeast Pacific off Chile or Peru. However, this may have some connection to the equatorial variability associated to the sun "crossing" the equator twice a year, as is presented by Li and Philander (1996).

But nevertheless this paper indicates that although the sun "crosses" the equator twice a year, the eastern equatorial Pacific has a pronounced annual cycle in both components of surface winds. This is in contrast to the Indian Ocean and western Pacific where a semiannual oscillation of the wind is dominant on the equator”.

Despite the differences between the eastern equatorial Pacific compared to the western equatorial Pacific and the Indian Ocean, as it is shown in the study of Li and Philander (1996), there is a semidiurnal component in eastern equatorial Pacific but smaller in magnitude than the annual component. Notably, the aforementioned work is based on a short observation period.

However, describing the way how this influences the southeastern Pacific off Chile is not yet evident and we think is out of the scope of this study. However, one option could be that it comes from a teleconnection similar to the mechanism that produces intraseasonal variability along our study region.

3.- In the article, the potential role of Ekman pumping on the spatial structure of sea surface temperature is also discussed. It is not clear to me why authors used one data set to force the atmospheric model (OSTIA) and another to analyze the role of Ekman pumping on SST (MUR). Please clarify.

The use of high-resolution SST (OSTIA) products derived from satellite sources to initialized WRF has been shown to improve the representation of surface parameters in coastal regions in the SEP (LaCasse et al., 2008; Renault et al., 2012ab; Toniazzo et al, 2012; Renault et al. 2015). Here we evaluated two high-resolution daily products, the OSTIA (Stark et al. 2007) and the RTG_SST (Thiébaux et al., 2003) analysis these have a spatial resolution of 0.05° and 0.5°, respectively.

Furthermore, SST-MUR was used to compare the results obtained from Ekman pumping with the purpose of comparing the results with a different product that the one used to "force" the atmospheric model, and that also has a higher spatial resolution ($\sim$ 1 km) than OSTIA.

4.- Page 3008, line 1-5 say: “Additionally, local high frequency forcing in the region is associated with atmospheric coastal jets with period less than 25 (Garreaud and Muñoz, 2005; Muñoz and Garreaud, 2005) that are related to the variability of the South Pacific Anticyclone and play a major role in coastal upwelling (Renault et al., 2009; Aguirre et al., 2010)”. I am not agree. The atmospheric coastal jets are related to synoptic dynamics of the mid-latitudes pressure perturbations (in this case high pressures) that migrate toward the east, as demonstrated by Muñoz and Garreaud, 2005 and after by Rahn and Garreaud, 2013.

We thank the reviewer for his/her comment the text has been modified to:

“Additionally, local high frequency forcing in the region is associated with atmospheric coastal jets with period less than 25 days that are related to synoptic dynamics of the mid-latitudes pressure perturbations, in this case high pressures, that migrate toward the east (Muñoz and Garreaud, 2005; Rahn and Garreaud, 2014) and play a major role in coastal upwelling (Renault et al., 2009; Aguirre et al., 2010)”

5.- Page 3010 line 15. The reference Garreaud and Muñoz, 2005 is not correct here, due that study does not involve simulations. The correct reference should be Muñoz and Garreaud 2005, due this study involves simulations with MM5.

The mistake has been corrected.

6.- Page 3011 line 13 say: “. . .the results indicate a better fit in diurnal variability when model if forced with SST (OSTIA)”. I wondering why you get a better fit in diurnal variability when force the model with OSTIA, if this data set lacks of a diurnal cycle, due it have daily temporal resolution as mentioned in page 3010 line 22.

OSD
12, C1716–C1722, 2016

The Sea Surface Temperatures (SST) are prescribed at the lower boundary (parent and inner domains) from the OSTIA daily product (Stark et al., 2007). To include the diurnal cycle we have calculated the 6-h anomalies with respect to the daily mean from the FNL SST and then added it to the daily OSTIA SST. Like this we generate the 6-h lower boundary updates with the same update rate used for the LBCs (Renault et al. (2015).

7.- I suggest that a final section with the summary of the major findings and conclusions should be included.

We agree with the reviewer's comment major findings and conclusions were included
Technical comments

Figure 3e. Due the seasonal variability of the wind stress and wind curl, it should be useful add the date of the measurements in the legend to know which line correspond to dates mention in page 3014 line 18.

We included dates in the figure and figure caption

Figure 6a. The choice of the colorbar is not adequate. Usually the drastic differentiation between reds and blues is used to distinguish between positive and negative values as in figure 6b. But the use of this colorbar in figure 6a could be confused.

Change has been made, a new colorbar using blue-red-yellow is used in Figure 6

Page 3013 line 11 say: (due upwelling). It should be (producing upwelling) ?

Text was corrected

Page 3019, line 4. Maybe the letter M is not the best choice for the Meandering index as it was used previously as Ekman transport.

We agree with the suggestion and changed the letter for Ekman transport.

Page 3023, line 28. There is a reference to the “horizontal SST gradients”. It is not clear

OSD

12, C1716–C1722, 2016

Interactive Comment

how those SST gradients were calculated. Are zonal SST gradients? Are cross-shore SST gradients? Are the maximum SST horizontal gradients? Please clarify.

The Horizontal SST gradient considers both components (zonal and meridional). We plotted the magnitude of the gradient. Text was clarified.

Page 3016 line 4 say: by astraigther coastline . . . it should be . . . by a straighter coast- line.

The mistake has been corrected

Page 3027 line 12. Reference say Jacox and Edwards, 2002. Should be Jacox and Edwards, 2012.

Reference was corrected

Interactive comment on Ocean Sci. Discuss., 12, 3003, 2015.

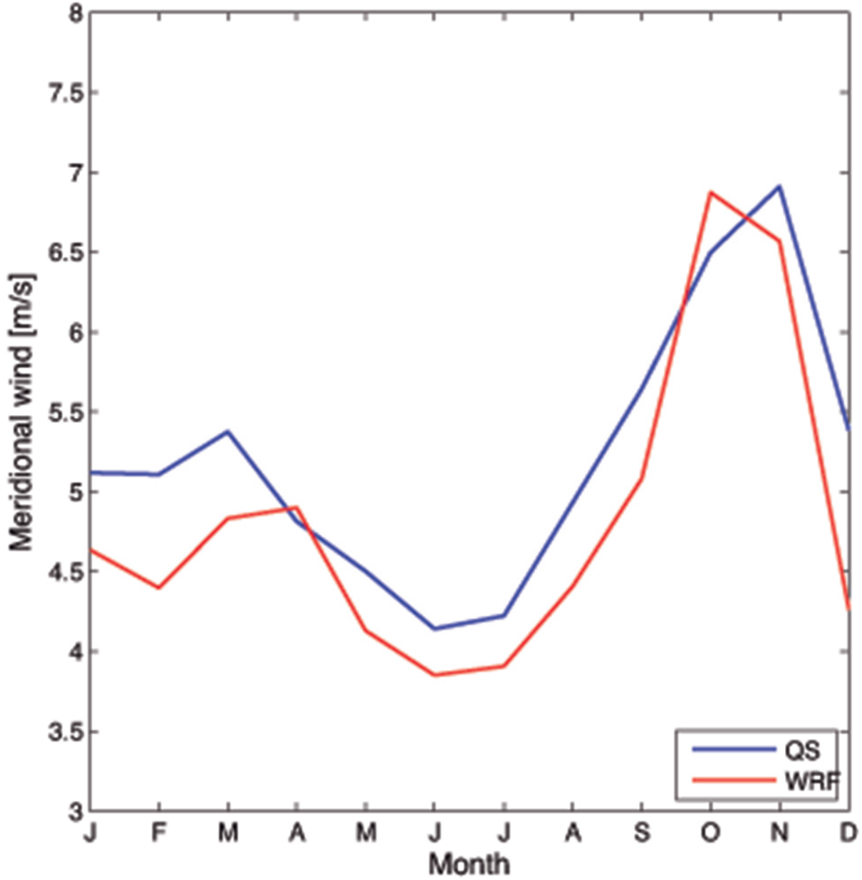

Fig. 1. Annual cycle of wind to QuikSCAT (blue line) and WRF (red line). Time period (2007-2009) and spatial zone (31.75°S-32.75°S; 71.75°W)

OSD
12, C1716–C1722, 2016