

Interactive comment on “Synoptic scale variability of surface winds and expected changes in the ocean–atmosphere dynamics of the eastern Austral Pacific Ocean” by Iván Pérez-Santos et al.

Iván Pérez-Santos et al.

ivan.perez@ulagos.cl

Received and published: 29 March 2019

Interactive comment on “Synoptic scale variability of surface winds and expected changes in the ocean–atmosphere dynamics of the eastern Austral Pacific Ocean” by Iván Pérez-Santos et al.

Anonymous Referee #2

Received and published: 30 January 2019

This paper presents an analysis of wind variability in the eastern Austral Pacific Ocean, and more precisely over the southernmost part of America. Also, the authors look for

C1

relationships between wind patterns and the ocean response, as well as the potential impact on nighttime heat waves. I think the goals are interesting and can shed light into the mechanisms behind coastal ocean variability in that region, but I'm concerned about the robustness of their conclusions as the methodology presents some flaws. Probably the intuitions of the authors are right, but a more careful analysis should be performed to support their conclusions.

General comment to RC2: We appreciate all recommendations of the reviewer, especially the addition the time series of different variables and processes, such as those related to the Ekman upwelling quantification and the ocean response. The new total Ekman upwelling quantification demonstrated the dominance of Ekman pumping instead of the Ekman transport as was proposed before and recently by other authors. Additionally, supplement material has been added which presents different figures associated with the validation processes between wind satellite and reanalysis products with in-situ data (buoys and navy lighthouse). These analyses demonstrated the high correlation and low root mean square error and standard deviation between in-situ data and the ERA5 reanalysis climate data set.

First issue is about the analysis in two different periods. I understand this is done because of the time coverage of each satellite product, but by doing this it is not clear if the differences reported between periods are due to the period or the product. I think that more efforts should be put in the comparison between products and after calibration use them as a single product and perform the analysis for the whole period.

-In order to validate the results obtained with the two scatterometers, the ERA5 reanalysis data set was incorporated into the manuscript. The ERA5 covered the complete and continuous sampling period of satellites (1999-2015). The data analysis of ERA5 confirmed and validated the results showed by the scatterometers. New figures and text were added to the manuscript.

The relationship between the wind structures and the ocean response (SST and Chla)

C2

is the most important part of the paper, in my opinion. Therefore should be presented in a more robust way. Using only snapshots is not enough to prove anything. Either you show time series (e.g. SST/Chla evolution against EP/ET or TUT), or use composites (i.e. average the SST for the periods in which HAP/LAP situations are dominant).

-As was recommended by RC2, we extracted a time series of the Total Ekman Transport, same as TUT, along with the west coast of Chiloé island, where favorable upwelling conditions were observed. The ERA5 data set was used for this calculation. Time series of Chl-a, normalized fluorescence line height and SST from MODIS AQUA were used in a temporal and spatial resolution of 8 days and 4 km. A new figure is presented in the manuscript to show the precise relationship between wind structures and the ocean response during the period 2002-2018.

Something similar happens with the results concerning the nighttime heat waves. Using two hand-picked cases to demonstrate the influence of LAP systems in the nighttime heat waves is not robust. Some statistics as composite images associated with nighttime heat wave periods or time series analysis would be much better.

-We added a new subplot figure (Fig. 13) that shows the time series of the nighttime heat wave events. Also, a correlation process was applied between air temperature and the atmospheric pressure of each of the events revealing a high correlation coefficient.

The separation between Ekman pumping and Ekman transport is interesting. This should probably be discussed in more depth in the discussion section, as well as the implications the different components may have on the ocean evolution.

-We added more information and a discussion of the relevance of Ekman upwelling in the ocean response. New references were incorporated, and the new analysis and quantification of total upwelling demonstrated the dominance of Ekman pumping instead of Ekman transport along the western coastline of Chiloé island.

Detailed comments

C3

The title doesn't seem adequate. The ocean-atmosphere coupling is not taking into account (the only atmosphere forcing on the ocean). Also, no expected changes are analyzed.

-We modified the title, to "Synoptic scale variability of surface winds and ocean response to atmospheric forcing in the eastern Austral Pacific Ocean".

I miss an introductory figure with the map of the zone of interest with the major wind patterns. -We added a new figure 1 to show the geographical position of stations and analysis.

For the introduction, it would be useful to clearly state if HAP and LAP are symmetric atmospheric situations.

-We incorporated new information in the Introduction.

L90-94: I think this does not fit as a final sentence for the introduction and should be moved elsewhere. -We eliminated the final sentence of the introduction section.

L105. Has ERA-Interim or the satellite data been validated in this region? This is important as the quality of those products is not the same everywhere. If you have wind data from local stations it would be worth comparing them with it to assess the quality at different time scales. -We added supplemental material that incorporates different figures of the validation process carried out between satellites and reanalysis surface wind products with in-situ local stations, such as, buoy and navy lighthouse. A Taylor diagram was applied showing satisfactory results.

L110. Show the location of the stations in an introductory figure.

-As we mentioned before, a new figure 1 was added to the manuscript.

L118. It is not clear if the data is 15-mins or hourly. - We have clarified the information in the text. The raw atmospheric data from the buoy (3 minutes) and the meteorological station (15 minutes) were hourly averages.

C4

Section 2.4. It would be useful to briefly describe what is Ekman transport and Ekman pumping, what physical process involves, for the non-oceanographers.

-We added a new paragraph in section 2.4 that describes the importance of Ekman transport and pumping as physical processes to the biology and in general for non-oceanographers.

L142-145. Why can't you compute the curl? Can't you use the wind over land for that? Or alternatively, a 0 wind? Also, I'm not sure your choice of extrapolating the wind curl to the near-coast points is better. Although there is probably not the best option you should discuss the implications of that extrapolation in your results, as you may be overestimating the Ekman pumping near the coast.

We computed the wind stress curl as shown in equation 5. In the computation of the wind stress curl, only the data available over the sea was used. During the wind stress curl calculation, the closest grid point to the coast is lost. The extrapolation process thus only incorporated this point in the data. We compared the wind stress curl calculation with and without the extrapolation processes and the results did not change. The positive Ekman pumping velocities registered in the coastal zone, especially in the northern domain, extended for more than five grid point into the ocean. We decided to continue with this methodology, but the results obtained in the interior fjords and channels were deleted in the new figure.

L148. It would be better to show the sections you are using in an introductory figure. In Fig 5 is not clear at all. -As we mentioned before, a new figure 1 has been added to the manuscript.

L156-157 "This method " This sentence is not needed. -We eliminated the sentence.

L158. "...the three LEADING modes" -We added "leading" to the sentence.

L163. The hourly, daily and monthly means are exactly the same. If you refer to

C5

computing the means for each hour, then you should explain it better.

-We eliminated this sentence from the text.

L163-171. I think this paragraph is repetitive with ideas presented before and can be rewritten.

-We eliminated this sentence from the text.

L168. Time correlation is not a statistical moment. Also, you should compare the differences in magnitude (e.g. STD) and the RMSE. Also, time correlation should be computed for the different data sampling you analyze here (e.g. hourly, daily or monthly). -Correlation process and Taylor diagram were applied in all cases where necessary. New results and discussion was added to the manuscript.

L174. Why the period 1999-2015? It doesn't match with the period covered by the products.

-We agree with the comments, but the manuscript was written some years ago, with the data set available at this time. In this version, we have incorporated the ERA5 reanalysis data set to December 2018, with which we demonstrated the similarities to QuikSCAT and ASCAT. The ocean response to the surface wind using the derived parameters, e.g., Ekman pumping and transport were presented for the period reported by ERA5 reanalysis (1999-2018).

L193-196. Beware, EOF analysis works on anomalies, so they reflect weakenings or strengthenings of the mean field, and may not mean a change in the direction of the total wind field. Please, reconsider your statement.

We have clarified this sentence "The spatial structure for the first three modes from the QuikSCAT and ASCAT databases were similar (Fig. 2). In the case of the spatial structure of mode 1 (Fig. 2a and 2d), southerly and southwesterly winds dominated the study area, when the time-dependent coefficient was positive (Fig. 3a and 3j, PC-1). When PC-1 (principal component) was negative, the spatial structure of mode 1

C6

changed the direction, and northerly and northeasterly winds occurred”.

In this manuscript, we are using the same EOF method (real-vector EOF) proposed in Kaihatu et al., (1998). In the description of the methodology, the authors wrote: “The time-dependent coefficients show the magnitudes and directions of the vectors; negative coefficients denote a 180° shift in the direction relative to that shown on the spatial map”.

Figure 3. What do the arrows mean?.

The arrows in figure 3 (c, f, l, and o) indicated the normalized eigenvector patterns presented in figure 2 (a, b, d and e). We have decided to eliminate the arrows from the figure.

Figure 4 is strange. Here there are more than one EOF acting. If not, EOF+ and EOF- should be exactly the opposite. I think this figure, as it is more confusing.

-We have clarified the information in figure 4, but we believe that figure 4 is important to the manuscript because it is the only figure that shows different examples of the influence of HAB and LAP systems in the study region.

L257-259. It is not clear that ET/EP is strong in those examples. Probably showing time series of ET/EP would be more illustrative than single snapshots. Also, about the maps, they are confusing, too much information there. Probably a simpler figure with the wind field and the TUT in colors would be clearer.

-We eliminated ET/EP examples and added the time series of the Total Ekman Transport together with the ocean response variables, e.g., Chl-a, SST, etc. A new figure is present in the manuscript.

L268-270. This sentence doesn't seem very relevant in this context. Again it would be better to show the location of the station in an introductory figure. -We eliminated the first sentence from the text and added the location of the stations in figure 1.

C7

L273-274. I don't think the histograms are enough to prove the solar radiation forces the diurnal cycle. Although is probably the case, correlations or explained variance diagnostics would be better suited for that.

-We carried out a new statistical analysis to prove the relationship between variables. New subplot figures were added.

Figure 9. It is not clear what the bottom panels represent.

-The bottom panels in figure 9 (e and f) represent the histogram of the maximum surface air temperature as we mentioned in the caption of figure 9.

Figure 10. The caption is not clear.

-The information presented in figure 10 shows the results from the nighttime heat wave events. We improved the caption of figure 10 and also included more details inside the figure.

L345-354. This is not supported by any result shown in the paper. Either the authors show new figures or remove this. -We removed lines 345-354 from the text and also the reference includes in this paragraph (Alvarinho et al., 2006).

Please also note the supplement to this comment:

<https://www.ocean-sci-discuss.net/os-2018-119/os-2018-119-AC3-supplement.pdf>

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2018-119>, 2018.

C8