

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

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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 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.

First issue is about the analysis in two different periods. I understand this is done be-

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

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

I miss an introductory figure with the map of the zone of interest with the major wind patterns. For the introduction it would be useful to clearly state if HAP and LAP are symmetric atmospheric situations. L90-94: I think this does not fit as a final sentence for the introduction and should be moved elsewhere. 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. L110. Show the location of the stations in an introductory figure. L118. It is not clear if the data is 15-mins or hourly. Section 2.4. It would be useful to briefly describe what is

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Ekman transport and Ekman pumping, what physical process involves, for the 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 a best option you should discuss the implications of that extrapolation in your results, as you may be overestimating the Ekman pumping near the coast. L148. It would be better to show the sections you are using in an introductory figure. In Fig 5 is not clear at all. L156-157 "This method " This sentence is not needed. L158. "...the three LEADING modes" L163. The hourly, daily and monthly means are exactly the same. If you refer to computing the means for each hour, then you should explain it better. L163-171. I think this paragraph is repetitive with ideas presented before and can be rewritten. 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 analyse here (e.g. hourly, daily or monthly). L174. Why the period 1999-2015? It doesn't match with the period covered by the products. L193-196. Beware, EOF analysis works on anomalies, so they reflect weakenings or strenghtenings of the mean field, and may not mean a change in the direction of the total wind field. Please, reconsider your statement. Figure 3. What do the arrows mean?. 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 is more confusing. L257-259. It is not clear that ET/EP are 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. 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. 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. Figure 9. It is not clear what the bottom panels represent. Figure 10. The caption is

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not clear. L345-354. This is not supported by any result shown in the paper. Either the authors show new figures or remove this.

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