Ocean Science
Discussions

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 would like to begin our response by thanking the reviewer for his/her insightful comments we believe that our manuscript has been improved after addressing the recommended suggestions and comments

Specific comments:

1.- Influence of Diurnal Cycle There is a substantial change in the wind stress and wind stress curl diurnally. Does this impact the relative contribution from month to month?

First, we must clarify that the outputs of the WRF simulations are instantaneous wind values every hour, which later become daily averages. We know that by estimating daily averages it smoothens the influence of the wind's diurnal cycle, which is important considering the intensification of the coastal jet during the afternoon, but this does not mean the disappearance of the low frequency signal (subinertial variability), which is what we are interested in this work (see Figure 1).

In our study region, the atmospheric coastal jet extends from the coast for several tens of kilometers to the west, showing some nearshore maximums, like in Punta Lengua de Vaca (Garreaud and Muñoz, 2005; Muñoz and Garreaud, 2005, among others). In addition, near Punta Lengua de Vaca it has been found an atmospheric local and baroclinic jet (local origin), with a marked diurnal cycle, with a maximum around 18:00 (LT) (Garraeud et al, 2011; Rahn et al, 2011). Which confirms what the reviewer said.

On the other hand, we understand the meaning of the comment made by the reviewer. Therefore, differences in monthly estimates are analyzed using the proposed two-hour from the daily cycle instead of using daily averages (see replies below).

Overall, considering the objectives and the results shown in Figure 1, the daily cycle and specifically the intensification of the wind stress in the afternoon did not significantly affect the results.

However, given the importance of the daily cycle in the area and its intensification in the afternoon, we included a paragraph in the discussion regarding this issue and what would happen if only the afternoon information is included.

2.- It was never clearly stated, but what is the "daily output" (3014, line 26)?

We consider that the use of daily output is not appropriate therefore we changed it to daily averages obtained from hourly simulations. We fixed the correspondent paragraph.

3.- Is the wind field averaged over every hour?

The wind field obtained from WRF simulations contains instantaneous wind values every hour.

4.- What happens to the relative contribution if just the 0 or 12 UTC is used for each month?

To answer this question we worked with the hourly output of wind from the WRF simulation for the period 2007-2012. We selected the zonal and meridional wind components (10 m) at 0 UTC and 12 UTC, and with this information Ekman pumping was calculated.

Afterward, an average for the spring season was obtained from wind information and Ekman pumping from October and November (during this period the most intense winds are observed, specially at the local coastal jet in Puna Lengua de Vaca). Daily averages were also obtained using the same procedure.

The results are shown in Figure 1. The upper panel corresponds to the wind during spring conditions taken at a) 0 UTC, b) 12 UTC and c) from daily averages. While in the lower panel Ekman pumping is shown using winds at d) 0 UTC, e) 12 UTC and f) daily averages.

Simulations show an intensification of the wind at 0 UTC, emphasizing the coastal jet at Punta Lengua de Vaca ($\sim$30.5 °S, south of Tongoy Bay), strong winds were also observed north of Punta Choros (29 °S) and south of 31°S. These characteristics of strong winds in the afternoon (local time) disappear during the morning (12 UTC). Rahn et al. (2011) propose: "Local baroclinicity (daytimes) enhances the development of the Coastal Jet that develops from the northward advection of diabatically/subsidence heated continental air over Tongoy Bay. For reference, a layer about 500 m thick and 5 K warmer that replaces the cooler air column in the morning in the southern portion of Tongoy Bay would be associated with a local surface pressure drop of $\sim$1 hPa inducing a strong ageostrophic acceleration of the flow"

However, when we use the daily average, we can distinguish the coastal jet and high

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winds in Punta de Choro and south of 31°S, but with smaller magnitudes at 0 UTC. This is due to the smoothing produced by the average. On the other hand, if we look at the structure of Ekman pumping for the three cases, all showed a similar pattern near the coast, with a positive values (favorable to upwelling), but differed in their magnitude, which is greater at 0 UTC, lower at 12 UTC, and intermediate when considering the daily averages. Further away from the coast, the negative Ekman pumping occurs when considering the 0 UTC, when the daily average is used Ekman pumping is smaller, and is not observed in the case of 12 UTC

Therefore, we believe that for the purposes of this manuscript, using daily averages of wind from the WRF simulation time is valid. The observation made by the reviewer will help the discussion, especially regarding the importance of the intensification of the coastal jet in the afternoon.

5.- Are the results in spring dominated by the intense late afternoon coastal jet? This is an important aspect for interpreting the seasonal variation.

Reply was given above

6.- Interpretation of coastal wind/drop-off zone. This is a major issue and needs to be corrected/improved. Starting at the end of page 3018-3020, the interpretation of coastal wind is based off of primarily Renault et al. (2015). There is a lot of literature on coastal wind that is much more complete. Some of the earlier works are Beardsley et al. 1987 (cf. Section 3, JGR), Burk and Thompson (1996, Mon. Wea. Rev.), Haack et al. (2001, Mon. Wea. Rev.), and many, many more. Archer and Jacobson (2005 Mon. Wea. Rev.) do a much more complete treatment of vorticity generation than Renault et al. (2015).

We thank the reviewer for this suggestion. Therefore, it corrects and improves the sections including the works mentioned by reviewer

7.- Page 3020, line 5: It says that the cool SST stabilizes the air column and results in

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a shallower marine boundary layer. This is not correct.

Here we refer to a particular result of Renault et al. (2016) that show, based on a sensitivity analysis, that adding an SST front of 3°C over a coastal band strip of 25 km results in weaker surface wind associated with more stable and shallow marine boundary layer.

To avoid any further confusion we have reformulated this part:

"Another minor factor is the sharp coastal sea surface temperature front associated with upwelling. Renault et al (2016) show that in their sensitivity experiment adding a sharp SST front over a coastal band strip leads to weaker surface wind associated with more stable and shallow marine boundary layer. This response of wind may be due to so-called "downward mixing" mechanism (Wallace et al. (1989); Hayes et al. (1989)), which was used by many authors to explain the observed tendency of surface winds to decelerate over colder flank of the SST front and accelerate over warmer flank of the SST front (cf. Small et al (2008) and references therein): warm (cold) SST would destabilize (stabilize) the PBL and cause enhanced (reduced) vertical turbulent mixing, increasing (decreasing) downward fluxes of horizontal momentum form the faster flow above to the slower near-surface flow. Nevertheless, a large SST anomaly (by -3 C in the experiment of Renault et al., (2016)) is needed to induce a significant weakening of wind and significant additional wind drop-off. Therefore, the SST effect can be considered as secondary compared to the orography effect over the California coast."

8.- Page 3020, line 26: Perhaps leave speculation of the atmospheric forcing mechanisms out of this.

The suggestion is accepted and was remove from the text of the manuscript.

9.- Model issues: Section 2.1 has some vague parts. Was the WRF initialized just once and ran from 2007-2012?

The WRF model was implemented and integrated for 7 years (2000-2007) using the best configuration obtained in the preliminary experiments (see Question 11). The initial and Lateral Boundary Conditions (LBC) are derived from the National Centers for Environmental Prediction's (NCEP) final analysis (FNL) fields at 1°x1° horizontal resolution and 6 hourly interval.

For each year the model was re-initialized with the FNL reanalysis every three months leaving 6 overlap days as a spin-up, the outputs during this period were excluded from the analysis. The LBCs (updated every 6 h) are prescribed over the parent domain with the depth of 5 grid-cells where simulated variables are relaxed towards the FNL solution. The Sea Surface Temperatures (SST) is 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 to the daily OSTIA SST. In this way we generate the 6-h lower boundary updates with the same update rate used for the LBCs (Renault et al. (2015). That configuration was suggested by Lo et al. 2009 in order to mitigate the problems of systematic error growth in long integrations and inconsistences between the developing flow and the lateral boundary conditions.

10.- Why does the outer domain extend all the way to 10N (Fig. 1)?

Beyond the focus of the present study we are also interested in assessing the impact of the downscaled winds from the coarser domain over a regional ocean model of the Humboldt system (Dewitte et al., 2012) whose domain extends from 5°N to 40°S following the approach of Cambon et al. (2013), that explains the northerly extension of the parent domain.

11.- It was stated that at least six different parameters (cumulus, PBL, soil, SST forcing, land surface, and topography [how/why was that changed?]) were evaluated (3010, line 25).

Given the complex interactions between alongshore winds, topography, cloudiness,

land heating and coastal upwelling in the study region (Rahn and Garreaud 2010a, b; Wood et al., 2011; Toniazzo et al., 2011) we tested the WRF model in different configurations associated to the aforementioned process and characteristics. The objective was to identify the configuration that leads to a better model representation of the near-shore surface mesoscale atmospheric circulation in the study region. A set of eight sensitivity simulations (see Figure 3) were carried out for the control period, i.e. from 1 October 2007 to 31 December 2007 corresponding to the upwelling season in north central Chile. The results were evaluated against surface observations from meteorological automatic stations and scatterometers (QuikSCAT, ASCAT), particular attention was paid to the shoreward decrease and temporal variability of the surface wind speed near the coast. The sensitivity experiments were divided in four sequential phases selecting progressively the best parameters for the optimal model configuration for the long period 2007 2012:

a) Parameterization and soil models: The first four experiments (see Figure 3) were implemented to compare the representation of the clouds and land surface exchange processes from two combinations of parameterizations (cumulus-PBL) and soil models already used in previous studies in the SEP (Renault et al., 2011a, b; Rahn and Garreaud 2010a, b; Toniazzo et al., 2011; Renault et al. 2015a, b).

b) SST forcing: The use of high-resolution SST 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 (experiments 5-6 in Figure 3) these have a spatial resolution of 0.05° and 0.5° respectively.

c) Topography and Land-Use: We have incorporated the high resolution 3-arc second SRTM topography and the accurate MODIS (1-km) land use and soil categories in order to compare the results with the previous experiments implemented with the

standard 30 arc second USGS topography and Land Cover (experiment 7 in Figure 3).

d) The last sensitivity test (experiment 8 in Figure 3) was performed with the aim of quantifying the impact of the nesting technique over the model diagnostics and the associated CPU requirements.

12.- This means that there are a lot of different 5-year runs at 36/12/4 km that were done. Was it really just a subset that was evaluated? There is no need to show all of these runs if that is really the case, but don't oversell the evaluation of model sensitivity.

Reply to this comment in the previous question.

13.- Specific model issues: Page 3010, line 7: Increasing resolution does not always translate to greater skill, and there are other issues to consider. (see Ranjha et al. 2015, Meteorol. Atmos. Phys.)

The main objectives of the atmospheric model were to generate the mesoscale surface wind patterns that influence nearshore circulation and evaluate the sensitivity of the model resolution to capture those local wind anomalies. Given the small-scale of these wind features and the influence of the orography, coastline shape and air-sea interaction the high-resolution is a necessary requirement for the model. However as you highlight increasing resolution does not always translate to greater skill so to avoid any confusion we will reformulate this part of the model description.

14.- Page 3010, line 13: Half of the model levels are below 1.5 km? Keep in mind that a good rule of thumb is that the lowest full level should be 0.990 or 0.995 if a PBL scheme is used.

The levels in the vertical are stretched to âĂĺprovide higher vertical resolution toward the surface, such telescopic resolution was needed to properly simulate the MBL depth over the ocean. This is a common choice in previous studies in the SEP with the WRF model (e.g. Garreaud and Muñoz, 2005; Rahn and Garreaud 2010a, b; Toniazzo et al, 2011; Renault et al 2012a, b; Rutllant et al, 2013; among others)

15.- Page 3011, line 1: What does it mean to simulate at hourly intervals? I don't think that is the time step since the integration would be unstable. Does that mean that the output is saved every hour?

We mean that all model diagnosis in our runs were stored at hourly intervals (see Figure 2), the time steps were set to 108, 36 and 12 seconds for the domains with horizontal grid spacing of 36, 12 and 4-km respectively.

To avoid a misunderstanding we have fixed the paragraph accordingly.

16.- Page 3011: Include the range of dates for WRF in all of the figure captions. Some are 2007-2012 and others are 2007-2009. It has to be clearly stated.

We agree with the reviewer's comment and clarified this issue in the figure caption and the manuscript.

17.- Other specific comments: Page 3012, line 15: Assume that it goes to zero right at the coast?

If the question is correctly understood then the reply is, it does not go to zero right at the coast.

Taking into account an approximate the total upwelling velocity as:

Wup $\sim=$ Tc/(rho*f*Lcu) + (To-Tc)/(rho*f*Ldrop)

Where:

Tc: is the alongshore wind stress at the coast. To: is the alongshore wind stress at the offshore end of the dropoff zone. f: is Coriolis parameter. Lcu: is the length of the frictional inner shelf zone where surface and bottom Ekman layers overlap. Ldrop: is the scale of wind dropoff. Rho: is water density.

If we consider that Lcu = Ldrop, then:

Wup $\sim=$To/(rho*f*Ldrop).

The total upwelling velocity does not depend on the coastal wind stress (Tc)

18.- Page 3012, line 20: Onshore wind? This discussion has been about the decline of the meridional wind. Is that what is meant?

Yes you are correct, there was a mistake in the manuscript the text was corrected as follows:

"a marked decline coastward of meridional wind component"

19.- Page 3013, line 3: In the previous paragraph, several assumptions were made. Here, is this using the assumption of a constant gradient and that it goes to zero at the coast, or is this the actual curl computed from the model grid?

Please note that the paragraph is rather a "note" to explain some considerations regarding both mechanisms analyzed and zonal wind distribution under certain assumptions. This was added after a suggestion given by the Editor and we found it reasonable to consider

The curl was calculated from the model grid, this was mentioned in the mansucript

20.- Page 3014, line 16/Fig 3d: Since Fig. 3e only goes out to 200 km, perhaps only extend the Fig. 3d out to âĹij500 km. This will also make it easier to see the detail near the shore in the model. Also, caption should be "Distance from the coast (km)"

We agree with the reviewer's comment and extended Fig. 3d until 500km, x-label was changed to "Distance from the coast" and included in the caption

21.- Page 3017, line 6: On average...not every day has equatorward wind, especially in winter.

We agree with this comment and corrected the text

22.- Page 3017, line 25: What do you mean by integrate? It looks like these are just average values in 0.25 degree bins. What wind measurement closest to the coast is

it? From QuikSCAT? Is it from the WRF (not a measurement. . .)? This needs to be much clearer since it is central to your main conclusions.

The misunderstanding most likely is, because the units in Fig. 7e are wrong, it should be m3 s-1. The mistake was corrected

Ekman transport was meridionally integrated every $0.25°$ so the final units are in m3 s-1 as it has been done in other related studies (e.i. Pickett and Paduan (2003); Aguirre et al., 2012)

As the reviewer mentions, WRF are not measurements, we corrected this mistake and specified that Ekman transport and pumping were obtained from numerical simulations.

23.- Page 3018, line 18: The meridional variation of the relative contribution between pumping and transport is important, but is the actual ocean response dominated by processes like upwelling shadows in the Coquimbo Bay?.

Yes indeed, processes such as upwelling shadow can be important in the Bay of Coquimbo, and are affecting the temperature distribution inside the bay, especially in the southern part of the bay close to the coast, where higher temperatures are observed (and higher thermal front) compared to the lower temperature area that extends north from Punta Lengua de Vaca (Figure 10). In fact a study in the southern part of the bay system of Coquimbo by Moraga et al. (2011) shows cyclonic circulation when there are upwelling favorable winds, the circulation is attributed to the separation of oceanic flow in Punta Lengua de Vaca, which is in agreement with the process of upwelling shadow and mainly affects the area indicated above. But we think that this is not inconsistent with the effect of the wind curl in the area, which would favor upwelling north of Punta Lengua de Vaca. The oceanic response in the area clearly needs more attention and research. In the future, we think to use an ocean model forced directly with high-resolution atmospheric simulations to analyze the oceanic response to different mechanisms.

We included a paragraph in the manuscript about the effect of an upwelling shadow and the consequences to our study.

25.- Page 3021, line 14: QuikSCAT is only twice a day at most, which can also impact the average.

Correct, we included this comment in the manuscript

26.- Page 3024, line 17: Since pumping is also correlated to transport, wouldn't that also be highly correlated? It would be good to include that to not oversell the pumping-only relationship.

Yes, in fact both mechanisms are highly related to seasonal scale, as specified on page 3018, lines 5 to 10. However, both mechanisms exhibit significant differences in upwelling transport as a function of latitude, i.e. when one is intense the other is weak (Figure 7). South of 31.25 °S, both mechanisms vary more uniformly.

We agree with the comment and included in the manuscript that due to the high correlation obtained between both mechanisms within the seasonal scale we cannot infer a relationship with SST only from Ekman pumping, especially where Ekman transport dominates.

27.- Fig. 7: Would the ratio of transport to pumping make a better comparison?

We agree with this comment and included the ratio of transport to pumping.
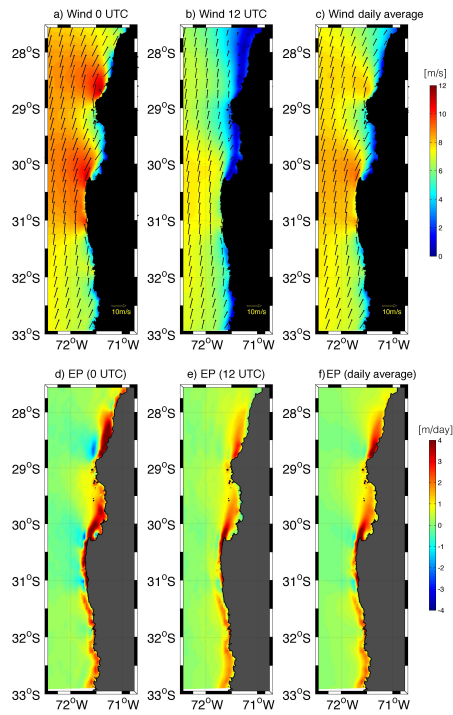
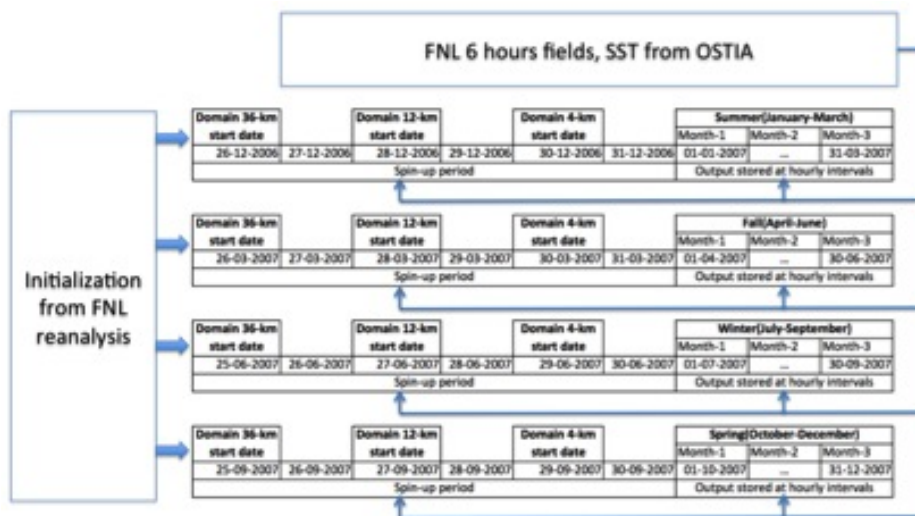28.- Fig. 9: Dec. in the upper left panel.

Text was corrected

Fig. 1. Spatial distribution for spring season of wind velocity (top panel) and Ekman pumping (EP, lower panel) using WRF wind at a-d) 0 UTC, b-e) 12 UTC and c-f) daily average

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Fig. 2. Schematic diagram of the experiments for the year 2007

Sensitivity phases	Nº	Convective parameterization	Planetary boundary layer (PBL)	Suface Model	SST	Nesting	Topography	Soil Categories
Parameterizations and Soil models	1	Kain-Fritsch	Mellor-Yamada-Janjic(MYJ)	Pleim-Xiu	NO SST	two-way	USGS(30")	USGS(24) 1-km
	2	Kain-Fritsch	Mellor-Yamada-Janjic(MYJ)	NOAH	NO SST	two-way	USGS(30")	USGS(24) 1-km
	3	Betts-Miller-Janjic(BMJ)	Bretherton and Park(UW)	Pleim-Xiu	NO SST	two-way	USGS(30")	USGS(24) 1-km
	4	Betts-Miller-Janjic(BMJ)	Bretherton and Park(UW)	NOAH	NO SST	two-way	USGS(30")	USGS(24) 1-km
Sea surface temperature	5	Betts-Miller-Janjic(BMJ)	Bretherton and Park(UW)	NOAH	RTG	two-way	USGS(30")	USGS(24) 1-km
	6	Betts-Miller-Janjic(BMJ)	Bretherton and Park(UW)	NOAH	OSTIA	two-way	USGS(30")	USGS(24) 1-km
Topography - Soil Categories	7	Betts-Miller-Janjic(BMJ)	Bretherton and Park(UW)	NOAH	OSTIA	two-way	SRTM(3')	Modis(21) 1-km
Nesting technique	8	Betts-Miller-Janjic(BMJ)	Bretherton and Park(UW)	NOAH	OSTIA	one-way	SRTM(3')	Modis(21) 1-km

Fig. 3. Different model configurations adopted for each sensitivity experiment. Shaded and bold letters highlight the selected optimal configuration.