Replies to Referee Comments

OSD manuscript number os-2018-160 "Characterizing ERA-interim and ERA5 surface wind biases using ASCAT" by Maria Belmonte Rivas and Ad Stoffelen

We are thankful to the reviewers for the time they took to read the manuscript and the judicious comments they made, which helped to improve the manuscript. Below, comments from referees are reproduced in black, our responses in red, and the changes introduced in the manuscript in green.

Referee #2 (Mark Bourassa)

Summary: The paper makes great use of a time honored type of analysis to examine wind in satellites and models. The results are scientifically interesting as well as illuminating strengths and weakness of the ERA-interim and ERA5. The quality and clarity of the work are largely excellent, although there are a few places where more cautious conclusions should be drawn. The link to currents is remarkable.

Major Comments:

1) Abstract: the word 'defective' carries very negative impressions, and is not very descriptive. Please use a more effective word.

OK. We have replaced the word 'defective' by 'insufficient' or "too weak".

2) Page 2, line 20: How are the satellite and model winds collocated? What is done to make the locations and times match? This is particularly important for metrics such as the one at this part of the ms and on page 3, line 16.

Agreed. The original sentence in page 4 last paragraph (section 2.2 ERA surface winds):

"The ERA-Interim first-guess winds, featuring a spatial grid of 79 km, come from 3-hourly forecasts based on 12-hourly analyses centered at 0 and 12 UTC. The ERA5 first guess winds come from 1-hourly forecasts based on 12-hourly analyses centered at 6 and 18 UTC, with an improved spatial grid of 31 km"

Is extended with:

"The model wind vector components are quadratically interpolated in time and linearly interpolated in space to match the ASCAT satellite observations."

3) Given that satellite winds tend to sample twice a day, the sampling of the diurnal cycle is regionally biased to certain times of day, which might not be representative of the other times of day.

Agreed. This issue of limited representativity of ascending measurements is very relevant, and it is now introduced in the manuscript (section 2.1, see reviewer 1 response, and last paragraph in Section 4 Discussion).

"In all cases, we note that the model-to-satellite wind differences are limited to ascending ASCAT measurements, which correspond to nighttime (approximately 9.30 pm) conditions. The diurnal variability of surface winds certainly limits the representativity of the nighttime differences that we observe, since the ERA diurnal cycle may not be perfect. Nevertheless, the main conclusions are not expected to change for daytime conditions. Actually, the boundary layer destabilization that generally takes place during daytime is expected to increase the amount of higher frequency wind variability, which is generally underestimated by ERA, and thus enhance the magnitude of the model-to-satellite differences reported here."

4) Page 6, line 14: PBL stability is one of the two popular explanations for SST-related variations in wind vectors. Theory and observations indicate that this explanation is insufficient, and at least one other mechanism must be important (O'Neill et al. 2012)

Agreed. The original sentence:

"The SST-gradient effect describes how surface winds vary in response to SST modification of atmospheric boundary layer stability via PBL destabilization with heat fluxes."

Is now replaced by:

"The SST-gradient effect describes how surface winds dynamically respond to SST modification and associated ocean heat flux changes (O'Neill, 2012; Skyllingstad et al., 2007)."

O'Neill, L. W., 2012: Wind speed and stability effects on coupling between surface wind stress and SST observed from buoys and satellite, J. Climate, 25, 1544-1569, doi: 10.1175/JCLI-D-11-00121.1

Skyllingstad, Eric D., Vickers, Dean, Mahrt, Larry, Samelson, Roger, 2007, Effects of mesoscale sea-surface temperature fronts on the marine atmospheric boundary layer, Boundary-Layer Meteorology 123 (2), 219 – 237, https://doi.org/10.1007/s10546-006-9127-8

5) Page 9, discussion of Figs. 13& 14. Please also discussion EKE changes in the Southern Ocean.

The original paragraph:

"Figures 13-14 show the balance between observed and model mean and eddy kinetic energies before and after the ocean current correction (cf. Fig.2). The alleviation of zonal

mean wind errors reduces the MKE differences in the mid-latitudes by about one half, but increases the MKE differences in the tropics, indicating that the model mean wind speeds in the trade regions have become weak relative to observations after the ocean current correction, which is to be mainly attributed to defective model meridional inflows into the ITCZ (see right panel in Fig.12). We observe that EKE differences increase globally, particularly in the extra-tropics. "

Has been extended with:

"In the Southern Ocean, the increase in EKE differences is accompanied by the largest decrease in MKE differences."

6) Page 11, around line 10: A more careful hypothesis is that Monin-Obukhov parameterizations are insufficient to explain the mixing in the lower atmosphere. While they are observed to work well near the surface, additional processes might be needed at in the mid and upper boundary-layer.

Agreed. The original paragraph has been extended with:

"The Monin-Obukhov parameterizations are observed to work well near the surface, but additional processes might be needed at higher levels in the boundary layer."

Minor Comments:

1) Page 1, Line 24: separate references with a comma.

Done

2) Figure 5 caption: Should 'differences to ERA Interim' be 'differences from ERA Interim?' The same question applies to the following figures.

Done

3) Page 10, line 4: change portray to portrait or portrayal.

Done