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Interactive comment on "Low-level jets over the North Sea based on ERA5 and observations: together they do better" *by* Peter C. Kalverla et al.

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Dear reviewer,

First of all, we would like to thank you for the time and effort you have invested in reviewing our manuscript. We appreciated your kind words and constructive feedback. In your specific comments, you highlighted numerous points in the manuscript where the formulation was unclear or imprecise. We have carefully addressed your concerns and prepared a revised version of the paper in which most of your feedback is implemented. Below, we have copied your specific comments and inserted our detailed response to each of your suggestions, including the modifications we made to the manuscript. With that, we trust we have adequately addressed your concerns.

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Kind regards,

Peter Kalverla, James Duncan, Gert-Jan Steeneveld, Bert Holtslag

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Specific comments

p1,ln8: 'bias of 1m/s' Ambiguous. Clarify that this is compared to the long-term mean.

Response: We agree and implemented the suggestion in abstract.

p4,ln10: Specify on what grid the ERA5 data was retrieved for the present study.

Response: The ERA5 data was retrieved on a 0.3 degree lat/lon grid. We added this information.

p4,ln2: Given the large variability in data availability, it would be worthwhile to have Fig A1 here in the main text.

Response: We agree with the reviewer and placed this figure in the main text.

p4,ln14: 'gridpoint closest to meas. location'. Is this the case for the entire manuscript? I guess spatial gradients in ERA5 cannot be a priori ignored off the Dutch coast, in particular when plotting subtle differences as in Fig 2C. From suppl. inf. I note that interpolation is used, but this is good to mention this explicitly in the main text.

Response: The text is correct here, we used the nearest grid point. Only vertical interpolation was applied. Our rationale was that since we're considering heterogeneous terrain (especially if the next gridpoint is a land point with very different surface characteristics), interpolation might have adverse effects, and could even 'contaminate' the physical consistency of the model output. However, the reviewer brings forward a valid argument that spatial gradients can be strong, and therefore we checked whether interpolation would improve the results. We attach a figure illustrating the results. It appears that interpolation slightly improves the result, but the overall (rms) difference between

the two methods is only 0.04 m/s, or 3%. In figure 2c (now 3c), this error margin is within the size of the marker. Therefore, we prefer to stick with the 'pure' model output, and we added a comment about this comparison exercise at the end of this sentence.

p4,ln23: 'the same ... technique': unclear, please clarify

Response: We have modified this sentence to "and a similar seasonality filter would result in ..."

p4,ln30: 'The sites with . . .' This is a too strong statement since is depends totally on the HKZ lidars: ignoring them would lead to an opposite statement. Scatter is large.

Response: This is a fair point, we may have been a bit too eager in the description of this figure. We modified the text so that it now reads: "For example, the HKZ lidars show a strong bias (i.e. systematic error), but have a relatively small standard deviation (i.e. random error).

p5,ln1: Please add a (few) lines on the observation uncertainty. To what extent could this contribute to the scatter in Fig 2C? Is it all the fault of ERA5?

Response: We have added the following: "Uncertainties in the observations can also contribute to overall error statistics. Based on the manufacturer information and previous validation (Poveda, 2015), the uncertainty in the observations can only account for about 2% of the errors. Finally, displacement in space or time, as well as discrepancies between point-based measurements and modelled control-volumes can contribute to errors, although we've done our best to minimize these effects, e.g. by using appropriate time-averaging of the observations (see SI-II)."

p5,ln5: Valuable observation!

Response: We agree that this needs to be communicated.

p8,ln6: 'average ... time steps'. This adds up to only 50 minutes of data, not an hour. Typo? Also, in 1 line explain the third representation of Fig4 in the text

Response: Good point. Actually we used 50 minutes, because we wanted to center the moving average with equal weight before and after the full hour, but indeed... the observation data are 10-minute averages over the past 10 minutes, so we need to include one more record on the 'right' side. We made this modification in both the text and the analysis. The results are not affected. With respect to the second part of the comment: this is actually the third representation, but we didn't say that explicitly. Therefore, we modified the text to "A more permissive evaluation (the third representation) is based on ...."

Sect 3: Nice section clearly illustrating the non-trivial character of LLJ detection!

Response: thanks.

Figure 3: For clarity reasons, I suggest not to plot every single LLJ event. Suggestions: apply some form of colour coding depending on the number of events for a number of falloff bins per height interval, 2) distinguishing between the various sites has little added value in this plot.

Response: We acknowledge the concern about clarity and appreciate the suggestion to plot the data in a different format. While a 'hexbin' or 'density' kind of visualization provides a more quantitative view on the height-falloff distribution, it also hides certain features (e.g. it is no longer obvious that the underlying data is aggregated over multiple sites). Moreover, since the 'point density' in this figure has a broad range, the sparser (perhaps most interesting) areas become almost invisible (unless a non-linear colormap is used, which is perhaps even more confusing). Finally, from a practical point of view, we can no longer 'jitter' the data, which will result in a much more 'banded' and less clear figure. Considering that our main goal is to illustrate the jet detection procedure and not so much the exact number of individual low-level jets that are present in the data, we prefer to stick with the original formatting of this figure. However, to address the concern about clarity and to facilitate quantitative interpretation, we added the number of jets in each panel as well as the number of jets above the falloff threshold

in the top left corner of each panel.

p9, l26: ‘calculated the ratio . . . ERA5 data’. Specify if this is done for each location (and each month) separately. Line 27: ‘months’: I don’t understand the plural form in relation to ‘this factor’ in the same sentence. Do you mean ’Months for which this factor is much smaller/larger than 1 are characterized by etc.’? Please clarify.

Response: We modified “for each month” to “for each month and each location”. And yes, this is exactly what we mean, and we agree that this formulation is much clearer, so we adopted it.

p10, ln 7. ‘fixed’ clarify: the same for all stations, for all months, or both?

Response: Two modifications. The first is “fixed scaling factor that minimizes their difference” is modified to “that minimizes the difference between each pair of monthly observed and simulated low-level jet frequencies.” The second is that we added “We do this for each platform individually and also for their combined signal.”

p14, ln13: Mention the low LLJ frequency off the British coast, even for offshore wind directions. Seems to behave differently that the continental coast.

Response: This is partly true, but if we consider figure 1b, the overall jet frequency here is not exactly low. It seems it is just less dominated by a certain weather type. Thus, we added that “The British isles are different in this respect, since for westerly flows, we do not observe an increased low-level jet rate off the eastern coast of the UK.”

p14, ln 13: Refer to the work of Ranjha et al. 2013, who demonstrate that this increased LLJ occurrence along coasts is a global phenomenon. Ranjha et al. 2013: Global distribution and seasonal variability of coastal low-level jets derived from ERA-Interim reanalysis, TELLUS A, https://www.tandfonline.com/doi/full/10.3402/tellusa.v65i0.20412

Response: In the revised manuscript we have extended the literature review in Section 1, which now includes the study of Ranjha et al. At this point in the manuscript, we

added “In general, we see that low-level jets concentrate along the coastlines. This extends and refines the global findings of Ranjha et al. (2013) and Lima et al. (2018) for the North Sea domain.

p16, ln23: ‘1%’ ambiguous in case of frequency of occurrence. I guess, also given Fig 9B, it should be 1 percent point, meaning a relative difference of âĹij10%. Please clarify.

Response: The reviewer is absolutely right, it should be percent point and we corrected this.

p18, ln 19: see comment p16, ln23

Response: corrected

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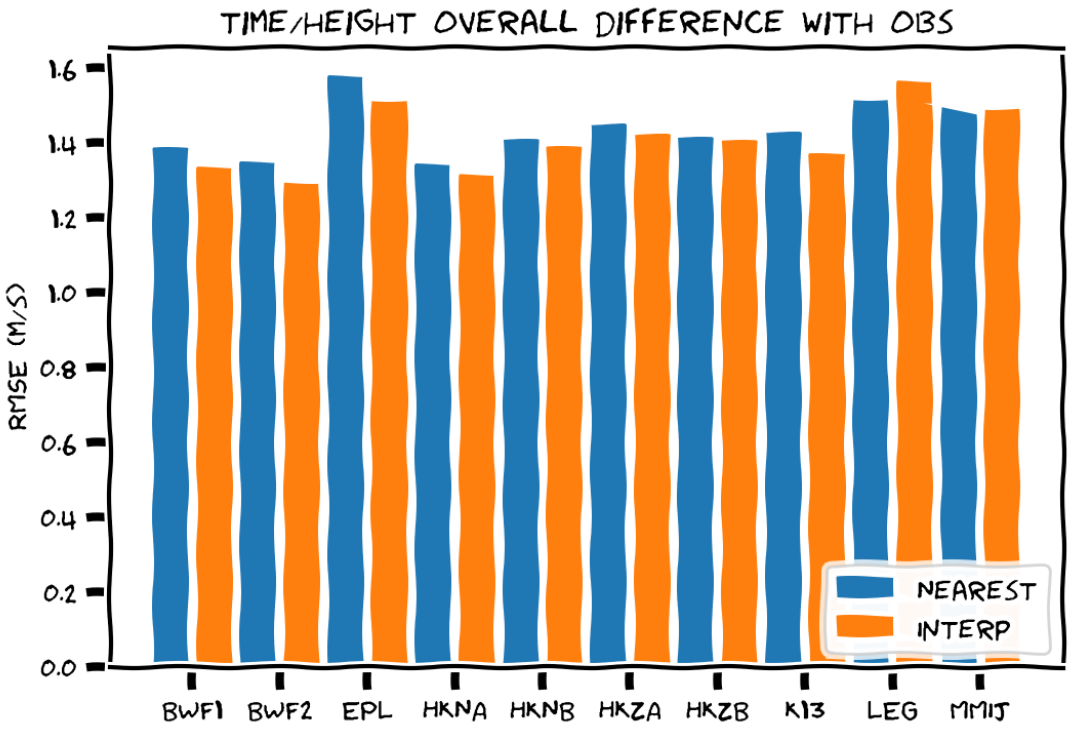

Fig. 1.

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