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Interactive comment

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 invested in reviewing our manuscript. We especially appreciated your general comment about the figure style. In a very considerate manner, you expressed almost exactly our own thoughts about the use of this style, including the hesitation. As suggested, we added a note at the end of the introduction stating that the consistent use of this style is in line with one of the main messages of the paper, i.e. to convey a notion of uncertainty. Your specific comments helped to improve the manuscript further and we have prepared a revised version in which most of your feedback has been implemented. To illustrate what we have done with each of your suggestions, we have copied your specific comments below and inserted our response to each comment, explaining the modifications that we made to the paper. With that, we trust we have adequately addressed your concerns.

Kind regards,

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

....

Specific comments

Page 2, lines 5-11: The literature review on previous LLJ studies, in particular LLJ statistics, is a bit short and should be extended.

Response: We already considered this for the initial submission, but at that time we decided to condense this paragraph in order to “get to the point”. However, the fact that a reviewer now raises this point makes us come back on our initial decision, and therefore we extended the literature review again.

Page 3, line 32: “Observations are available from seven sites (Figure 1B).” –> Make clear that only LiDAR observations are used, not met mast data.

Response: this is not completely correct as in fact, met mast observations are included, but only at MMIJ. We added this information immediately after the first sentence of section 2.

Page 4, line 1-2: “More information on the quality control and post-processing of the LiDAR data can be found in Appendix A.” Add “data availability” to the sentence –> “More information on the data availability, quality control and post-processing . . .”

Response: Another reviewer suggested to include the appendix figure in the main text. So instead of referring to the appendix, we now refer to this figure. With that, we think we have also addressed the underlying concern of this reviewer, namely that some information about data availability is appropriate at this point.

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Discussion paper

Page 4, line 20: “At MMIJ (. . .) this representativity bias reaches upwards of 1 ms-1” –> In Fig. 2A the lines for MMIJ are only 0.4 m/s apart, not 1 m/s, and the bias for many of the other sites is much larger. Please correct or clarify what you mean.

Response: Indeed, this must have been mixed up. We have modified the text to express that “for some stations, this bias reaches up to almost 2 m/s, and for MMIJ, for which the longest record is available, it still reaches up to about 0.5 m/s.”

Page 4, line 30: “An error diagram of the wind speed in ERA5 versus observations” –> Which ERA5 dataset is meant: The full 10-year dataset or the subsets? Please clarify also in the caption of Fig. 2.

Response: this can only refer to the subsets, as we can only compute error statistics when observations are available. We have added the specification “(subsets)” in both this sentence and the figure caption.

Page 4, lines 31-34 and Fig. 2C (error diagram): By definition and as also obvious from the figure RMSE and STDE are the same, aren’t they? Your description of the figure and the figure itself suggest that there is a difference.

Response: No, they are not the same, but they are related through (RMSE)ˆ2 = (STDE)ˆ2 + (BIAS)ˆ2. The standard deviation of the error distribution is sometimes referred to as a ‘centered’ RMSE. We have added this relation to the text, since it is apparently not immediately obvious.

Page 5, lines 8-9: “We hypothesize (. . .)” –> Is there any literature available that could support your hypothesis?

Response: We agree that this would strengthen our argument, but unfortunately such papers are hard to find. While they don’t support our hypothesis explicitly, we included the following references: “the difficulty of appropriately assimilating observational data within the (stable) boundary layer is discussed in Reen (2010) and Tran (2018)”.

Page 6, lines 4-18 and Fig. 3: Can you add numbers to Fig. 3 (and/or to the text)? By how much is the data reduced from A to C or B to D? It seems as if even below 300 m much more than 50 % of the data is removed.

Response: That's a good suggestion! We added the total number of jets as well as the number of jets exceeding the falloff threshold in the top left corner of each panel. This allowed us to make some quantitative statements in the text (e.g. "in going from panel A to C, 93% of the jets above falloff threshold vanished")

Page 6, line 20: "Simple visual inspection indicates that ERA5 does not perform well." –> Give more details, e.g. similar height distribution but much smaller falloffs.

Response: We agree that this statement was a bit vague. We based it mostly on the (underestimation of the) amount of jets (the height distribution is shown later). To clarify this, the sentence was changed to: "Judging from the figure, it seems that ERA5 does not perform well. Much fewer jets are found above the falloff threshold in the ERA5 data as compared to the observations. Indeed, a more quantitative comparison in the form of a contingency table ..." and then, after "was filtered out" we added a note that "even though the falloff is typically much smaller (to the extent that it falls below the falloff threshold), the height distribution of the ERA5 jets seems similar to the observations (also see Section 4)".

Page 6, lines 20-22: "A contingency table (. . .) shows a very low critical success index (. . .) and probability of detection (. . .)" –> Explain what this means, maybe also show the table.

Response: We added that "In other words, only 20% of low-level jets are correctly represented by ERA5". We considered showing the table here, but the number of choices involved in creating this table that would then also have to be explained and justified in the text would distract too much from the main focus of the paper. Note that this table is included in the supplementary information. Thus, we added a cross-reference instead.

Page 6, line 27: “other characteristics appear to be captured quite well” –> Add “e.g. the distribution of LLJs with height”.

Response: See previous comment Page 6, line 20, where we already added statement about similar height distribution. With that, we think the current suggestion has also been addressed.

Footnote 2: “In contrast to model level data (. . .)” Elaborate on this: why do the model level heights vary (is clear to me but maybe not to every reader)? How does adding jitter work?

Response: Quite challenging to explain this in a footnote.. Here’s our best try: “The ERA5 model levels are specified in terms of pressure rather than height, and can therefore exhibit small height variations in time. The observations, in contrast, are at fixed height, and to improve ....” However, see next response.

Figure 3: Explain why the points are organized in these “bands”. I assume this is due to the discrete model levels which vary in height.

Response: Indeed. So we combined this comment with the previous suggestion and move the extended footnote to the figure caption. Hope this makes things clearer.

Page 9, line 14: “the ERA5 data” –> The full dataset (A) or the subset (B)?

Response: See next comment/response

Page 9, line 10-18: Please clarify this procedure a bit more. It is hard to follow.

Response: Indeed, we struggled a bit to formulate all these steps succinctly. To clarify, we have rewritten a few lines here. The new text is: “To distill a more robust signal from the observations, we combined the data from all sites before computing the monthly means, and smoothed the resulting signal with a moving average of three months. The result is the dashed black line in panel E. We then repeated these steps for the ERA5 data (panels A-D), but before plotting these lines, we scaled them with the observations, using a fixed scaling factor that is simply the ratio between the mean low-level jet rate in the respective representation of ERA5 (panel A-D) and the mean of the observations (panel E)."

Figure 5A: Is is unclear if the dashed line in Fig. 5A is derived by the procedure described on p.9, l.13-14 or by the procedure described on p.11, l.5.

Response: The figure is based on the first procedure, and the confusion is probably caused by our reference to figure 5a on p.11, l.5. We have modified the latter, and it now reads "Applying this factor of 0.44 to the full ERA5 data provides us with a smooth seasonal cycle with reduced amplitude (similar to the black dashed line in Figure 5A, but this time based on an optimized scaling factor)."

Page 10, lines 6-9: Clarify that each pair of monthly observed and simulated LLJ is considered. And clarify that all sites are taken together so that you obtain one single scaling factor for the combined dataset.

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." We adopted this terminology "pairs of monthly ..." also for the other sections: pairs of hourly, etc.

Page 11, lines 23-24: "It appears that the low-level jets occur throughout the day, but with a small dip around 11 UTC." –> The dip is not so small, the LLJ probability is significantly reduced between 8 and 16 UTC. Do you have explanations for this diurnal cycle and what does the literature say?

Response: Well, the dip seems big for the ERA5 data, but it is much less pronounced in the observations. Especially if you compare it with a land point (e.g. Cabauw), the diurnal cycle for the offshore platform is much less pronounced. In the meantime, we have further analyzed the ERA5 data, and it seems that there are at least two mechanisms leading to the low-level jets: one related to the nocturnal jets onshore, and another leading to afternoon jets. This is probably related to the diurnal heating cycle, and especially the difference between land and sea. A third mechanism could be an inertial oscillation triggered by the coastal transition in offshore flows, but this would not have a diurnal signature. There is much more to say about these mechanisms and the literature, but we deliberately avoided going into too much detail here. To address the comment, we added that "From the observations, it appears that ..." and "The diurnal cycle in ERA5 is much more pronounced." And "At this point, we think it is good to stress that several mechanisms can lead to low-level jets in coastal areas (see Section1 and Section 9), and the diurnal signature should not be confused with that of the typical onshore nocturnal jet that is often found over land."

Page 11, line 26 and Fig. 7A-C: "but again, the magnitude differs" –> In Fig. 7A-C the dashed lines have the same magnitude. Please clarify.

Response: Modified to "but again, we needed to scale the ERA5 signals because they differed in magnitude."

Page 13, lines 2-9: Please describe which area you used to determine the LWT – is it the area shown in Fig. 8? So on how many grid points is the LWT derivation based? I assume you are using the ERA5 sea-level pressure field? How do you obtain the streamlines: Are you averaging all situations belonging to one LWT?

Response: We added "To derive these weather types we used the ERA5 mean sea level pressure on a 5-degree grid of 16 points as laid out in the appendix of Jones et al., but centered over the area of interest." And indeed, the streamlines represent averages, we added this to the figure caption.

Figure 8: "Amplitude is off by a factor of 2 (best guess)?" –> What does this mean? It becomes clear from the text, but I would recommend to omit this information in the figure caption.

Response: We understand that this information is confusing; on the other hand, we think it is necessary to warn readers who only scroll through the figures that this amplitude should not be taken for granted. Therefore, instead of omitting this information, we modified the warning to “As explained in the text, the values shown here overestimate available observations and should be interpreted with caution.”

Page 14, lines 23-30: Can you give references for this type of procedure? Response: We included the following references. Carta et al. (referenced in section 2) gives a nice overview of MCP. MOS forecasts are commonplace in meteorological textbooks, so we referred to Wilks, 2006 (chapter 6.5.2), and additionally to two early papers (Glahn, 1972 and Carter, 1989).

Page 16, line 15: “Notice that this seasonal cycle is very erratic” –> That is not surprising as it is only based on two years of observations.

Response: True, but we still want to point it out. To clarify this, we added “Note that this seasonal cycle is very erratic. This can be expected for such a short period, but the question is whether the additional information contained in the predictor variables enables us to predict the other two years despite the unrepresentative training data. Thus, in the next step, we used our trained model ...”

Page 16, lines 18-19: “we reconstruct the predicted seasonal cycle by grouping and aggregating the predicted probabilities for each month” –> Please give more details.

Response: We agree that this is not very clear. Modified to “rather than predicting individual jet events, we used the predicted probabilities directly and computed the monthly mean predicted probability.”

Page 17, lines 1-2: “This is our best estimate of the low-level jet seasonal cycle (. . .)” –> Please link to the results in section 6 (which give a similar results)

Response: We added “Compared to the results presented in section 6, we can conclude that we have adjusted the erratic nature of the short-term observations (Figure

5E), resulting in a seasonal cycle similar to that shown in Figure 5A, but with reduced amplitude. Compared to this final result, the crude amplitude adjustment with which we started in Section 6 now appears far too strong."

Section 9: Very nice summary of the paper!

Response: thanks. We hope that the discussion of mechanisms here also helps to address the reviewers concern about the (literature on) characteristics of the diurnal cycle.

Figure A1A: What does the colour coding mean?

Response: Added a short note "the color coding highlights episodes of high (yellow/green) and low (blue) wind speed."

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