

Authors' response to reviewers' comments on article:

"Low-level jets in the southern North Sea: implications for wind turbine performance using Doppler lidar observations" (Manuscript 2025-183).

First of all, we would like to thank the reviewers for their review and valuable comments, which contributed to further improving this manuscript.

Reviewer 1:

Major remarks:

1. Response to my original major remark 1: " We acknowledge that the coastal location of the Dunkerque site presents challenges for the interpretation of the climatology of the LLJs, as the measurements can be influenced by both land and sea depending on the wind direction. This is indeed a specificity of our site compared to the purely offshore or inland sites mentioned in our comparison. This question was already addressed in a previous work from Maynard et al. (2025) in Dunkerque, who studied the turbulent coherent structures in the surface layer using Doppler lidar horizontal sweeps. The results showed that, in the instrument range, onshore air masses kept their turbulent characteristics when advected seaward, while offshore air masses rapidly transitioned to onshore turbulence characteristics within a few hundred meters of the coast. This rapid adjustment indicates that the measurements from both the ultrasonic anemometer and the Doppler lidars mostly capture onshore atmospheric conditions, even for sea wind sectors. We think that this supports the validity of our LLJ climatology interpretation".

This explanation is acceptable, but the explanation/reflection has not been incorporated in the revised paper. The reader can benefit from it.

[You are right. A paragraph to include this explanation was added in the discussion part \(new section 4.2\)](#)

2. Response to my original minor remark:" Following your remark, we computed the annual cycle by correcting the number of LLJ profiles based on data availability within each Lamb weather type category, and we found similar results, as it is shown on the figure below. Therefore, we kept the original figures in the paper."

The same holds here: the insight that the analysis of synoptic patterns was confirmed by Lamb weather types is insightful for the reader since it underlines the robustness of your findings. Please add a sentence about this to the paper.

[We added a sentence on this method in Section 2.1 and a reference to the work of Jones et al. \(2013\), which explains the methodology to compute the Lamb Weather types.](#)

3. Response to the minor remark on section 3.3.1. Section 3.3.1: it is surprising that so

many LLJs occur under Unstable conditions, since the theory says many LLJs occur under stable conditions after decoupling of the atmosphere from the land surface, and a subsequent inertial oscillation that starts. So what is the quality of your Pasquill classes categorisation? The sign of L will largely depend on the footprint of the sonic anemometer, which is located at the coast. So with wind directions between NE (45 deg)and SW (225 deg) it feels land in the footprint and sea otherwise (roughly). But the sign of the surface fluxes can depend very much on this footprint, and as such the Pasquill class an event gets assigned to. All of this should be clarified in a revised version of the paper.

We agree that the footprint of the sonic anemometer could have impacted the classification into the Pasquill stability classes. This is a question that was already asked in the framework of another of our recent studies, dedicated to the organization of turbulence into coherent structures within the surface layer (Maynard et al., 2025). Horizontal sweeps of the Doppler lidar were recorded from the same site as in this study, and their manual observation showed that, within the range of the lidar, turbulence organization in onshore air masses kept its characteristics while being advected over the sea, while turbulence in offshore air masses transitioned very quickly (within a few hundred meters) to onshore organization. This supports the fact that observations from the ultrasonic anemometer are close to onshore conditions even when the wind comes from the sea, implying that the important part of jets occurring under unstable conditions is not due to an incorrect Pasquill class categorisation. Besides, LLJs occurring under stable conditions and formed by decoupling of the atmosphere from the surface are typically nocturnal, while in Dunkerque, an important proportion (45%) of the LLJs is diurnal and occurs during the warm season, which explains the proportion of the jets falling into the moderately and extremely unstable classes

The same holds here that the clarification need to be included in the paper since not everyone is familiar with the work of Maynard et al. (2025). The justification offered helps to generate confidence in the results of your paper.

The justification for the important part of jets occurring under unstable conditions was added in section 4.2, together with the discussion on the instruments' footprint.

Minor remarks:

Ln 49: Link <https://parc-eolien-en-mer-de-dunkerque.fr/project-en/>. I have nothing against the content behind the link, but perhaps it is good to think about the fact that this link will disappear from the www at some time. Is there any more permanent link to this project? Also holds for the link in Ln 149 and Ln 494.

We replaced this link by a governmental one that should be more durable:
<https://www.eoliennesenmer.fr/facades-maritimes-en-france/facade-manche-mer-du-nord/dunkerque>

In the legend of Fig. 1, the link to “geoportail” is from a parapublic agency and should remain stable, same for the link to the Copernicus Climate Data Store.

Ln 112: consecutive. Perhaps reword since consecutive can be also consecutive in time (and this is how you use it in Ln 117). Please avoid confusion.

The sentence was modified following your remark.

Ln 114: “decrease below the jet core”. Maybe reword. It should decrease from the jet core towards the ground? This means and increase with height. Please avoid any possible confusion here.

We meant a decrease below the jet core down to a minimum (or to the ground, if no minimum). That was added to the text.

Ln 126: Monin-Obukhov stability parameter. Here it should “Obukhov length” or “Obukhov stability parameter” only. Indeed in the literature we speak about the “Monin-Obukhov similarity theory”, but the length scale L was invented by Obukhov solely.

We changed to “Obukhov stability parameter”.

Ln 130: The used Von Karman coefficient is rather low, usually 0.40 or even 0.41 is used. Please justify your choice in the manuscript

The value of 0.37 for the von Karman constant was chosen for consistency with the previous work of Dieudonné et al. (2023) and with the Pasquill stability classes defined by Xiang (2011), in which this specific value was used.

Ln 133-135: the unit of L is wrong 6 times, should be m and not m^{-1}

The parameter defined in Eq. 1 corresponds to the inverse of the Obukhov length, which explains the unit m^{-1} . To avoid any confusion, we renamed this parameter to Λ and clarified its definition as the inverse of the Obukhov length in the revised manuscript.

Ln 139: (regridded from the native resolution of 1°). I do not understand this. Why would one regrid from 1° to 0.25° ? It does not add significant information. The native grid of the ECMWF model is not 1° . Do you mean 0.1° .

You are right. This information was incorrectly added following a comment from the first round of review. We removed it from the manuscript.

Figures 5-10, captions: “Fractions indicated on the right y-axis”. The right y axis presents percentages, not fractions. Of course I understand what you mean, but formally it is not correct now. In Fig 7 the caption does even not say what is one the right y axis at the moment.

We choose to retain the current notation for the right y-axis, as fractions are dimensionless quantities and can therefore be expressed equally as percentages. However, we added “expressed as percentages” to the captions of figures 5 to 10 and figure 12 to avoid any confusion. The caption of Fig. 7 was updated to include information about the right y-axis.

Bibliography:

- Maynard, P., Dieudonné, E., Sokolov, A., Delbarre, H., Augustin, P., Fourmentin, M., & Dmitriev, E. Exploring the level of organization of turbulent coherent structures in the atmospheric surface layer through supervised classification of Doppler lidar observations. *J. Geophys. Res.: Mach. Learn. Comput.*, 2, e2025JH000652. <https://doi.org/10.1029/2025JH000652>, 2025.
- Jones, P., Harpham, C., and Briffa, K.: Lamb weather types derived from reanalysis products, *Int. J. Climatol.*, 33, 1129–1139, <https://doi.org/10.1002/joc.3498>, 2013.
- Dieudonné, E., Delbarre, H., Sokolov, A., Ebojie, F., Augustin, P., and Fourmentin, M.: Characteristics of the low-level jets observed over Dunkerque (North Sea French coast) using 4 years of wind lidar data, *Q. J. R. Meteorol. Soc.*, 149, 1745–1768, <https://doi.org/10.1002/qj.4480>, 2023.
- Xiang, Y.: Analyse dynamique en champ proche de la contribution des sources de composés organiques volatils, en région urbaine sous influence industrielle, Ph.D. thesis, Université du Littoral Côte d’Opale, <http://www.theses.fr/2011DUNK0408>, 2011.

Reviewer 2:

Remarks:

Lines 147-150: “Three wind turbine models ... (Chirosca et al., 2022, <https://www.4coffshore.com/>), ... (Global 150 Wind Energy Council, 2024, p. 53, years 2023 and 2030).

Citations in this paragraph are problematic:

- There are two references combined in the in-text citation (Chirosca et al., 2022, <https://www.4coffshore.com/>). The format of this single in-text citation is incorrect. In Copernicus style (and most author-date styles), multiple references at the same position are placed in one set of parentheses and separated by semicolons (e.g., Smith, 2009; Mueller et al., 2010). The authors should confirm this in the official Copernicus guidelines. Additionally, the web link belongs in the reference list at the end of the document (with a proper entry, including access date if applicable), while the in-text citation would typically use the corporate author (e.g., 4C Offshore, year).

[That was corrected.](#)

- <https://www.4coffshore.com/> links to the homepage of a commercial website (now rebranded as TGS | 4C). The information on the homepage is volatile/dynamic (e.g., real-time project statistics and market data that change frequently) and currently does not support the information/claims presented in the manuscript. The authors should consider whether this is a suitable reference or if they can provide a more direct/stable link (e.g., to a specific archived report, dataset entry, or permanent URL).

[Since Chirosca et al. \(2022\) is sufficient to support the information on wind turbine models given in the paragraph, we removed the link to the 4C Offshore website.](#)

- By contrast, Chirosca et al. (2022) is a legitimate scholarly reference, with Table 1 listing wind turbine types operating in the North Sea.
- The citation supporting all the claims in this paragraph (and thus in Table 2 of the manuscript) is (Global Wind Energy Council, 2024, p. 53). Indeed, the figure on page 53 of the report allows derivation of all the values in Table 2. It should be noted, however, that the correct citation format is (Global Wind Energy Council, 2024, p. 53), not (Global Wind Energy Council, 2024, p. 53, years 2023 and 2030).

[We removed “years 2023 and 2030” from the citation.](#)

Bibliography :

- Chiroasca, A. M., Rusu, L., & Bleoju, A.. Study on wind farms in the North Sea area. *Energy Rep.*, 8, 162-168, <https://doi.org/10.1016/j.egy.2022.10.244>, 2022.