

## Comment on wes-2022-26

Anonymous Referee #1

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Referee comment on "Spatiotemporal observations of nocturnal low-level jets and impacts on wind power production" by Eduardo Weide Luiz and Stephanie Fiedler, Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2022-26-RC1>, 2022

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### General Comments:

The paper provides a detailed characterization of NLLJs based on LiDAR and meteorological measurements from two nearby sites in Germany. The subject is of broad interest since it investigates atmospheric processes poorly observed and highly variable in space and time. The methods are appropriate to support the findings, which are in accordance to previous studies. There is an extensive discussion on related literature. The main goals from the paper were the concise and objective approach, exploring from the data screening, handling and framing of the analyses. The evaluation of additional parameters such as stratification and weather patterns made the results more robust. The language is technically good and did not impair the understanding.

### Specific comments:

Some minor issues that could be improved:

1. Adequacy of title and scope: The scope of the paper is mostly focused on NLLJ characterizations and impact on wind profiles (shear and veer). Wind power production is assessed for NLLJ / Non-NLLJ periods, but this information is part of local wind climatology and the results were somehow expected. I suggest further examination on the unfolding questions that arise: How the shear and veer affect the wind power curve? What fraction of the wind power potential can be attributed to NLLJs? How it could support variability studies in longer time scales (even climate change)? How the weather patterns could help?

2. Assumption of a mesoscale process: The signals from NLLJ in the nearby sites are

similar what makes the assumption of a mesoscale process robust (and I tend to agree) but I'm not sure if this (6 km apart observation) is enough evidence for this statement. Surely there is a broad literature supporting these findings, and I suggest to revise the text (mostly discussion and conclusions) and rely more in this literature and not only on the concurrent observations.

### 3. Points to clarify:

Line 87-90: Does terrain discretization affects wind detection in reanalysis?

Line 155: The vertical resolution apparently did not change (see Line 135). Was that a temporal smoothing?

Line 158: Can we affirm that nocturnal speed-ups in the wind with less than 1 hour are noise?

Line 192: Limits for Ri depend on height evaluated?

Line 256: How were the nights without NLLJ? Does the RI and DT developed differently from Fig.11a?

Line 284: Is there any reference supporting the slackening geostrophic winds?

### **Technical corrections:**

Line 41: Is "single" necessary?

Line 67: Revise sequence of citations;

Line 81: Revise "With heights";

Line 187: Standardize Ri formatting;

Line 228: Can not see "duration" in the Table 5;

Line 275: Not clear what relative and absolute means

Line 324-325: Revise units kW/h;

Line 329-333: Not clear paragraph. Differences are expected due to hub height and rated speed. Consider improving;

Line 343-345: Not clear the shear/productivity gains. Consider improving;