Review of the manuscript wes-2025-183

Low-level jets in the southern North Sea: implications for wind turbine performance using Doppler lidar observations

By Pauline Haezebrouck and co-workers

Summary: This manuscript reports on a study using a Doppler lidar in Dunquerke at the French North Sea coast to develop a mini-climatology (3.3 years) of low-level jets (core speed, height, shear, Pasquill class) in the context of wind energy studies. The paper is in general well written, with clear figures, and is put appropriately in the context of earlier studies in the study area. Despite the figures are clear, with 15 figures they are with many. The main new insight that is provided is that based on the wind speed profile climatology with turbines of the future will suffer less from extreme positive wind shears in the LLJ-profiles, since they will "grow beyond them". More work is needed on the manuscript on the aspects of the understanding of the LLJ mechanisms.

Recommendation: Major revision.

Major remarks:

-Although the sec derivation of the LLJ climatology from the Doppler lidar data is thoroughly done, the paper is relatively silent about the challenging measurement site, i.e. close to the coast. The paper compares their findings with sites at FINO and Ijmuiden which is over sea, and Cabauw which truly over land. However the Dunquerke site is at the coast and the sensors will feel influences from land and sea depending on the wind direction and trajectories of the airmasses. This hampers the interpretation of LLJ climatology and should be addressed more in the paper. Some more concrete concerns are in the minor remarks below.

-Section 4.2 should be strengthened. This section provides physical explanations for the LLJ climatology, but many of them are suggestions (plausible suggestions, I agree), but I would like to see more evidence for the statements that are made in section 4.2 on the LLJ groups at high and low elevations, and the LLJ formation mechanism in the different wind directions.

-The paper has a relatively large amount of figures, and some are discussed not in much detail, so please consider ad deeper explanation and/or merging some figures.

Minor remarks:

Ln 90: To address this issue, the number of LLJ wind profiles was adjusted by dividing the observed values by the corresponding fraction of available data. This is a bit risky since the synoptic patterns might not be the same in the sample set. Do your results remain the same if the number of LLJs is not scaled with time/available data, but by available data per weather pattern (Lamb weather type, GrossWetterlagen)?

Figure 2: please add to the x axis it represents wind speed and to the y axis it represents height.

Ln 112: In this study, a longer criterion was considered as it improved the jet detection by excluding isolated jets. Please justify how you measured it was a "improved jet detection". What is your ground truth? Also, isolated jets are also jets, and they can affect wind turbines, and as such should be included in the statistics.

Ln 119: please add which value of the Von Karman constant was used. I have seen values between 0.35-0.41 in the literature. Just for reproducibility reasons.

Ln 119: turbulent sensible heat flux: at which height. I suggest to reword it to turbulent sensible heat flux at the surface (if it is sufficiently close to justify this). Same for friction velocity.

Equation 1: The Greek capital L to denote the Obukhov length suggests that this Obukhov length is the local Obukhov length that is based on local fluxes at height z (see Nieuwstadt, 1984). If you intend to use the Obukhov length based on surface fluxes, then I suggest to move to regular capital L for its notation.

Ln 126: I would like to see a sentence or two here that says something about the quality of ERA5 "to derive the conditions favoring the formation of LLJs". What is the quality of this product for your study area? The ERA5 product has a good reputation, but is not perfect in LLJs (see e.g. https://rmets.onlinelibrary.wiley.com/doi/10.1002/qj.3748). Also, ERA5 is relatively coarse with 0.25 deg grid spacing. Since Dunquerke is at the coast of the British Channel, is the ERA5 gridcell you used a land grid cell or sea grid cell? This will make a big difference and need to be addressed.

Ln 144: The assumption of the air density being constant at 1.2 kg/m3 seems to me as a very crude assumption, since others use 1 kg/m3 as well. For didactic reasonings it is okay, but here you aim to estimate real production values, so a density of 1 vs a density of 1.2 means 20% difference/bias/error in the estimate of P. So it is better to calculate and use the actual density.

Ln 206: It okay to report the mean LLJ core speed. But perhaps also add the median if the distribution is not very close to a normal distribution.

Ln 230: around this line you discuss the climatology of the fast jets being at higher elevation, and weaker jet being closer to the surface/lower elevation. Could you link this to their mechanism of formation? I can imagine the lower jets to be triggered by the land/sea contrast in surface roughness and/or surface temperature difference, rather than by the traditional decoupling in stable boundary layer conditions. This would bring added value to the manuscript.

Section 3.3.1: it is surprising that so many LLJs occur under Unstable condition, since the theory says LLJs many 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 if 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.

Section 3.3.1: in addition, to what extent is it possible to quantify whether the detected LLJs have been produced/triggered elsewhere (e.g. over the North Sea) and were just advected to your study site. It would be interesting to have this information to clarify the LLJ statistics behavior.

Ln 400: "implies that the second mechanism contributing to their development may have been the channeling of continental air masses into the Dover Strait under anticyclonic conditions. The jet formation process in this case has been related to the orography and land-sea roughness contrast". I agree that there must be multiple processes active, but more justification is needed for this statement. Running an illustrative case like this with a high resolution weather forecasting model like WRF would help to secure the understanding and proposed mechanism is correct.

Ln 404-: These jets generally presented lower velocities, occurred most frequently during summer afternoons, and were absent in winter, which suggests that they were mostly composed of sea breezes. Here I would disagree with the statement (or at least not enough support is given to justify the statement), since the current paper does not make a climatology of sea breezes to link the two phenomena. It can also be that in summer the North Sea SST is relatively low compared to its overlying atmosphere, which promotes a stable boundary layer, decoupling of the atmosphere from the surface and inertial oscillations can trigger LLJs. If these are then advected to your study site with a NW wind, they will not be triggered by a sea breeze. I also say this since my experience says the amount of pure sea breezes over the North Sea coast is less than our intuition might say.