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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Taklimakan Desert Nocturnal Low Level Jet: Climatology and Dust Emission" *by* J. M. Ge et al.

Anonymous Referee #1

Received and published: 5 April 2016

The article by Ge et al. presents an interesting investigation of NLLJs over the Taklimakan Desert, which has not be comprehensively analysed before. Using satellite-retrieved AOD the study attempts to establish a link to dust activity for evaluating the importance of NLLJs for dust emission in that area. My main concerns are the adopted method and physical background of the article. Some statements in the article are not physically clean and not always adequately referenced. Even though NLLJ detection tools exist, the present study defines another method that is not sufficiently motivated and leaves open questions due to a rather short validation and lack of naming some threshold criteria. This makes the evaluation of some of the results and desired comparison to NLLJ statistics from other regions difficult. Moreover, the connection of NLLJs to dust emission is not well represented by using AODs. I recommend that the physical explanations, method description, and critical discussion of the results are im-

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proved prior to publication in ACP. Since dust emission is not explicitly analysed, the title should be altered, e.g., Dust Activity (instead of Emission).

specific comments:

Add citations for the following statements: L. 49 “most intense dust aerosol source in Asia” and L. 50 “its large contribution to the global dust emission”

L. 55-57: Could you indicate the major mountains and the TD in Figure 1? I would also recommend to show the winds per season since the seasonal conditions are important for the results.

L. 73-74: Add citations for the dust emission process

L. 77: “highly sensitive to wind speed” also include some of the earlier studies that highlighted the sensitivity of dust emission to wind speed.

L. 77: “incursions” choose another word and note that cold outbreaks are also associated with synotic-scale weather so that it is redundant.

L. 91: the listing of “frontal dynamics” is not clear in the context of NLLJs.

L. 93-95: “due to diurnally varying eddy viscosity and friction layer depth that accompanies changes in inversion layer depth driven by surface thermal radiation emission and solar heating” Your difference between the friction layer depth and the inversion layer depth is not clear. Please revise. Maybe you could include a sketch for explaining what is meant.

L. 99-106: Add citations, e.g., studies carried out for other world regions than Asia.

L. 111-112: Also mechanically induced mixing due to the wind shear can disturb NLLJ development. This implies that, even though the radiative cooling might be strong, the decoupling from the surface must not necessarily be. Please revise. Also L. 255-260 need to be revised for the same reason since it proposes a similar explanation.

L. 112-113: Can you underpin the assumption that the IO is the dominant NLLJ formation mechanism? I am sceptical since Figure 3 shows that a strong jet structure occurs in the vicinity of mountain slopes. Moreover, L. 261-264 state that the wind directions are confined by the topography and find no large directional differences for NLLJs. In an IO, however, one would expect circular oscillations of the wind at jet level.

L. 182: “captures the elevation” better: reasonably well approximates the height

L. 183: “underestimates the wind speed in the lower and middle atmosphere for the two sites” The figure shows that ERA-Interim underestimates the NLLJ winds at Ruoqiang, but overestimates them at Korla. The statement should be revised.

L. 200-201: “temperature inversion condition is identified and the inversion top height (Hi) is determined by scanning each temperature profile” Please add which kind of temperature data and thresholds you applied for the presence and height of the inversion.

L. 211: “NLLJs always have jet-like profiles” that is not necessarily ensured with adopted criterion. If a wind minimum would occur, say just below 5000m, and the NLLJ at 1000m, the NLLJ would have a rather slow wind decay aloft and not a typical NLLJ profile as seen in the observations. From the observation (Fig. 3), it seems that this is not often the case, but to be sure one would need to add some validation, e.g., the usual height difference between maximum and minimum in the observations and the re-analysis, and how often extreme height difference occur. This would allow to better estimate the actual wind shear above NLLJs over TD and made your results better comparable to studies with other detection tools, like you do later in the manuscript.

L. 211-214: I would recommend to delete this sentence. The definition of the wind shear in your detection does not provide more consistency than having a fixed threshold. It is rather less certain what exactly you detect (see previous comment), thus gives you less consistency in the results.

L. 229: “Figure 5 reveals that our jet detection algorithm is reliable” It would be more

precise to say what is seen, namely a rough co-location of maxima in NLLJ wind speed and frequency indicating that the jet detection algorithm is successful. Note, however, that maxima in speed and frequency are not perfectly correlated as one can have rare, but strong NLLJs. That is also why a large NLLJ frequency does not necessarily imply that it is important (see L. 230-231).

L. 247: “frictionless” It is difficult to transfer the conceptual model by Blackadar to the re-analysis and observations where frictional effects persist in the nocturnal boundary layer, although substantially weaker than during the day.

L. 275-277: “Ideally this would have been calculated for 10:00 AM local time to observe the maximum effect but only 6-hourly ERA data were available.” Is 10 am the time of the maximum you have identified from observation?

L. 304-316: The Richardson number and method to determine the top of the boundary layer using it has a rather rich history and should be acknowledged, e.g., Richardson et al. (Boundary Layer Meteorology, 2013)

L. 321-322: “solar insolation which drives the local thermal forcing and the terrestrial cooling” More precise would be to say that solar insolation is the primary control of near-surface heating.

L. 326-331: Could you show the analysis of the occurrence of clouds to underpin your explanation?

L. 354-356: “This process is suppressed during cold season when the inversion depth is greater and consequently results in less downward momentum transfer that occurs over a longer period of time.” and also conclusions in L. 421. It must not necessarily be true that the process of downward mixing is suppressed. The downward mixing in winter could just occur later when the boundary has grown sufficiently deep that is presumably occurring after a longer time period than in summer. Also the mechanism must not be well visible in the 6-hourly data. You could simply test whether the NLLJ

is not mixed by comparing nighttime with mid-day wind profiles. If you still see a jet structure of the same magnitude, your statement would be right, but in that case the jet structure would not be a classical NLLJ.

L. 363-364: “In order to find a direct evidence of NLLJs effects on dust emission” AODs can not be used as direct evidence for dust emission. AOD is not only influenced by emission but also by transport and deposition, including aged dust from previous events that are not necessarily linked to NLLJs. Moreover, other aerosol species than desert dust affect AOD and the optical properties also play a decisive role. One could say that increases in AOD are an indicator for dust activity.

L. 370: “To avoid this risk” This risk cannot be entirely avoided. The results can be affected by emission and transport caused by other processes, e.g., daytime winds (not connected to NLLJs) increasing AOD. These AODs than coincide with NLLJs in the following night, such that the AOD is also an indicator for dust transport instead of pure emission linked with NLLJs. In fact the last paragraph states that in spring synoptic-scale events are more likely than NLLJs.

L. 386-388: Please add reference.

technical corrections:

L. 47: omit “extremely”

L 63: “earth-atmosphere” replace with Earth

L. 76: “resuspension” better emission in general, also in other sentences of the manuscript

L. 116 “LLJs” replace with NLLJs, also later in the manuscript

L. 347 no big or not a big

L. 369 “to evaluating” of evaluating

L. 559: “Monthly mean occurrence of the NLLJ frequency”, Use Monthly mean occurrence of NLLJs or Monthly mean NLLJ frequency

L. 572: Are these means?

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