Response to Anonymous Referee #1

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-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).

Response: We thank the reviewer very much for his/her constructive comments/suggestions on this manuscript, which are very helpful for us to improve the quality of our paper. We notice that there are many studies on LLJ or NLLJ, however there is not an universal method for LLJ detection. Our method adopted the most commonly used criteria for maximum wind speed and height, considered an inversion condition, and made a slight change of the criterion for the decrease of wind speed above the NLLJ. Following the review's suggestions, we further examined our NLLJ detection method (we find that the method works well over the TD region) and added more related references. We also improved the relevant statements for clarification. The "emission" in the title was replaced with "activity". Our responses to the specific comments are presented below.

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"

Response: References have been added.

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.

Response: We have indicated the Tianshan mountain, Tibetan Plateau and TD in Figure 1. We do not show the seasonal wind distribution over the TD region in this manuscript, because it has been shown in our former study (Ge et al., JGR, 2014).

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

Response: References have been added.

L. 77: "highly sensitive to wind speed" also include some of the earlier studies that highlighted the sensitivity of dust emission to wind speed.
Response: Some earlier studies have been cited.

L. 77: "incursions" choose another word and note that cold outbreaks are also associated with synotic-scale weather so that it is redundant.
Response: "incursions" and other superfluous words are removed. The sentence is changed as "Synoptic cold fronts are known to lead to strong surface winds".

L. 91: the listing of "frontal dynamics" is not clear in the context of NLLJs.
Response: Yes, the "frontal dynamics" is not a clear expression. We change this sentence as "temperature gradients over sloping terrain, coastal area and across weather fronts".

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.
Response: Thanks for this comment. The friction layer depth refers to the thickness from the surface extending to the height above which the frictional force is negligible. The expression of the diurnal variation of the inversion depth may cause some confusion. We removed both friction layer and inversion layer depth to make this sentence short and clear.

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

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.
Response: Yes, turbulence can be caused either mechanically by vertical wind shear or thermally by surface heating. Here we mean that the strong radiative cooling at the surface after sunset can stabilize the surface layer and provide a favorable condition to trigger NLLJ. The relevant part has been revised for clarification.

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.

Response: Thank you for pointing this out. Due to the coarse temporal resolution of the ERA data, we did not plot hodograph to examine if there is a circulation oscillation over this region. However, we found that the wind speed at 18 UTC is larger than 00 UTC. We also examined the total cloud cover (TCC) and clouds occurrence as you suggested. It is found that both the TCC and clouds occurrence on NLLJ days are much smaller than those in no jet days. This in turn may indicates that the strong radiative cooling, which is a critical condition in IO mechanism, plays an important role in the formation of NLLJ. The wind directions can also be controlled by pressure gradient, orography and decoupling period. So, yes, we notice that a strong jet structure occurs in the vicinity of mountain slope at the eastern entrance of the basin (i.e. 88° E). We changed this sentence to "We anticipate that the frictional decoupling after sunset with a subsequent inertial oscillation may play an important role in the formation of NLLJ for this area."

L. 182: "captures the elevation" better: reasonably well approximates the height
Response: "captures the elevation" have been replaced with "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.
Response: This statement has been revised as "The ERA-Interim underestimates the NLLJ winds at Ruoqiang, but overestimates them at Korla".

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.
Response: We use the ERA temperatures (Celsius) on model levels to identify an inversion. The inversion top height is determined by following the Kahl's (1990) protocol. This reference is also added.

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.
Response: We analyzed the height difference between the maximum and minimum wind speed layers. As shown in the following figure, the median height difference is

about 1 km. The extreme height difference with a thickness larger than 4km is only about 3.3% of the total data (the lower and upper boundaries of the blue boxes are the 25% and 75% percentiles, red line locates the median, the whiskers represent the upper and lower fence).

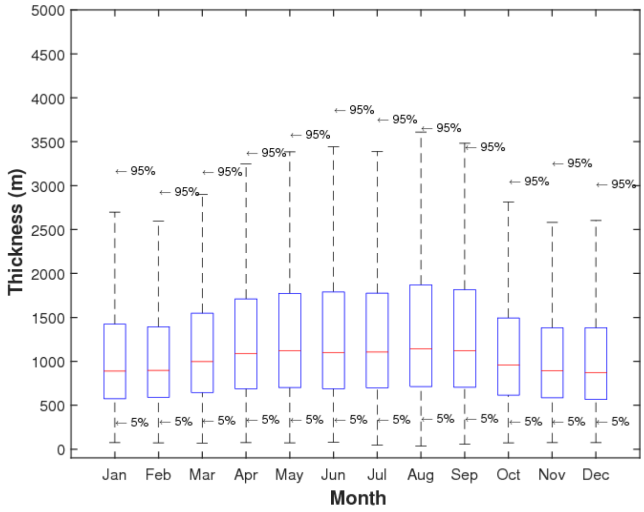

Figure 1. Annual cycle of the height difference between the maximum and minimum wind speed layers

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.

Response: This sentence has been deleted.

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).

Response: In Figure 5 we can see that the large frequency occurrence of NLLJ appears highly related to surface types (arid and desert regions) and orography. That also makes us feel confident about the NLLJ detection method. We modify this sentence as "It can be seen that there is a rough co-location of maxima in NLLJ wind speed and frequency. Figure 5 indicates that the jet detection algorithm is successful."

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.

Response: Blackadar theoretically assumes a complete decoupling from frictional effects. Yes, in reality, the frictional force does not vanish and the idealized circulation oscillation will be changed. However, when the frictional force significantly decreases during night, the balance of forces will be disturbed. This may trigger the formation of NLLJs and could be a reason for why NLLJ does not have to be formed on the top of inversion layer.

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?

Response: MISR passes over the TD region at about 10:30 AM. It would be better to compare the wind speed near the time when the satellite passes over.

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)

Response: We add this reference.

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.

Response: We change this sentence as "solar insolation that 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?

Response: We examined both the occurrences of clouds and total cloud covers (TCC) over the TD at 00 and 06 UTC for January and July from 2000 through 2013. As the following figure and table shown, both the occurrence of clouds and TCC on NLLJ days are much smaller than no-jet days, especially in July. Thanks for this comment. "Cloudless" is not an accurate word. We modified this statement in the revised manuscript.

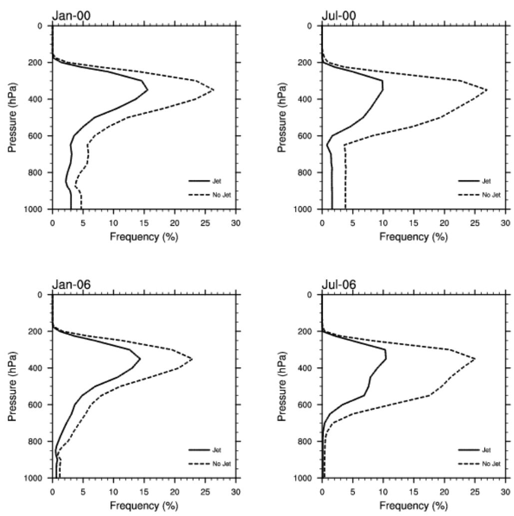

Figure 2. Vertical structure of cloud occurrence for January and July.

	Jan		Jul	
	Jet	No jet	Jet	No jet
00 UTC	0.18	0.30	0.11	0.29
06 UTC	0.16	0.25	0.12	0.28

Table 1. Total cloud cover at 00 and 06 UTC for January and July.

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.

Response: “suppress” is not an accurate word. We totally agree that the downward mixing process is not suppressed in cold season. We examined the inversion occurrence at 00, 06 and 12 UTC shown in the following figures. The inversion occurs frequently at night over the TD all year long (Figure 3a), and always breaks after sunrise at 06 UTC in all seasons except for December, January and February. This may indicate the vertical mixing is weak in cold season, but not suppressed since the frequency of inversion at 06 UTC is smaller than 00 UTC over the TD. Figure 3c shows that the inversion has been established at 12 UTC over the TD region during cold season. So, as the reviewer commented, the downward mixing in winter, which could occur later, cannot be well visible in the 6-hourly data. We corrected our statements in the revised manuscript.

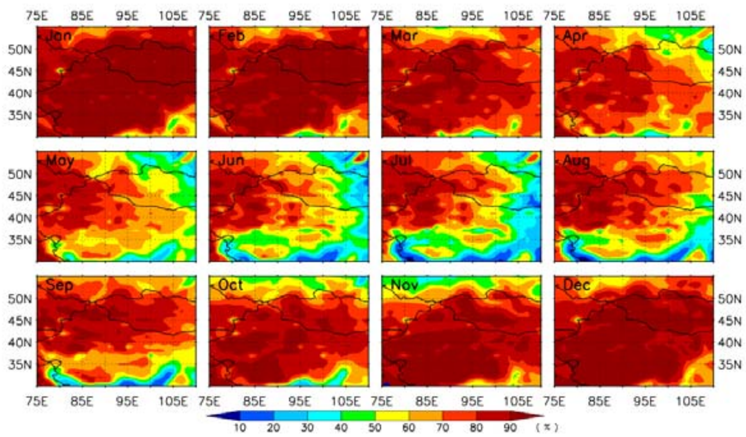

Figure 3a. Monthly mean occurrence of inversion at 00 UTC.

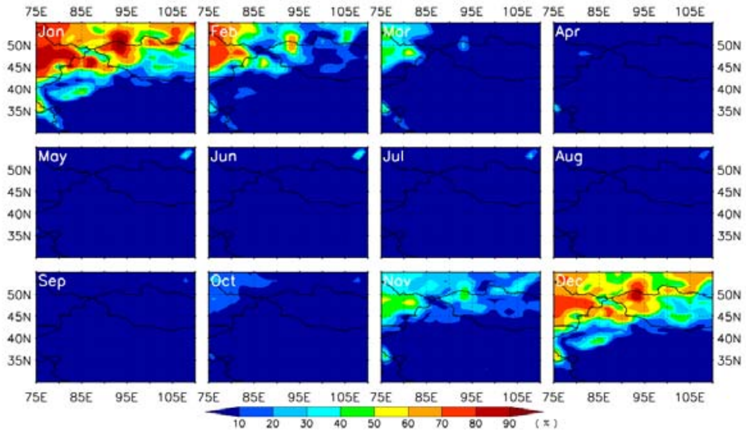

Figure 3b. Monthly mean occurrence of inversion at 06 UTC.

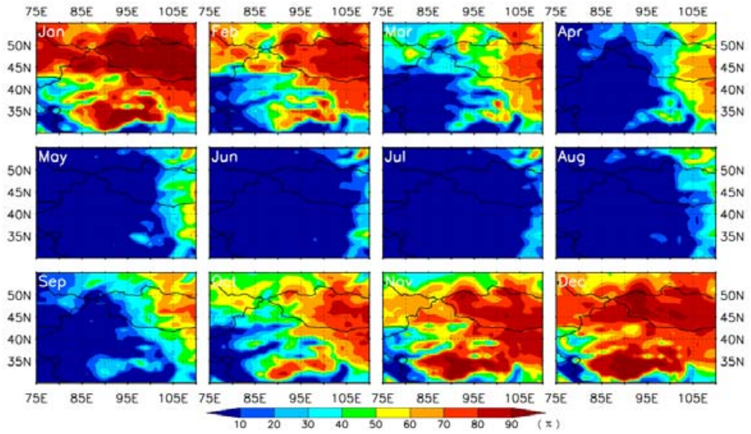

Figure 3c. Monthly mean occurrence of inversion at 12 UTC.

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.

Response: Yes, AOD is a measure of total extinction of light by all aerosol species in the atmosphere. Our former study (Ge et al., JGR, 2014) shows that the AOD value over the TD region is much larger than surrounding area. The AOD over the TD region is mainly contributed by local dust particles which are largely confined in the Tarim Basin. So transported AODs form remote region may have little influence. We agree that aged dust from previous events could obscure the link between dust and NLLJ. We change this sentence as "In order to explore a link between NLLJs and 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.*

Response: Yes, it is difficult to entirely avoid the effects from other processes on the AOD variation. We did a composite analysis with all time-matched AOD and wind profile data. As shown in the following figure, significant enhancements of wind speeds in the lower atmosphere are obvious in all seasons. Comparing the following figure with the Figure 10 in the manuscript, we can see that after selecting the data only with the appearance of NLLJ, high dust loading dose not significantly correlate with an increase of wind in spring and winter. We may expect that the risk is largely avoid.

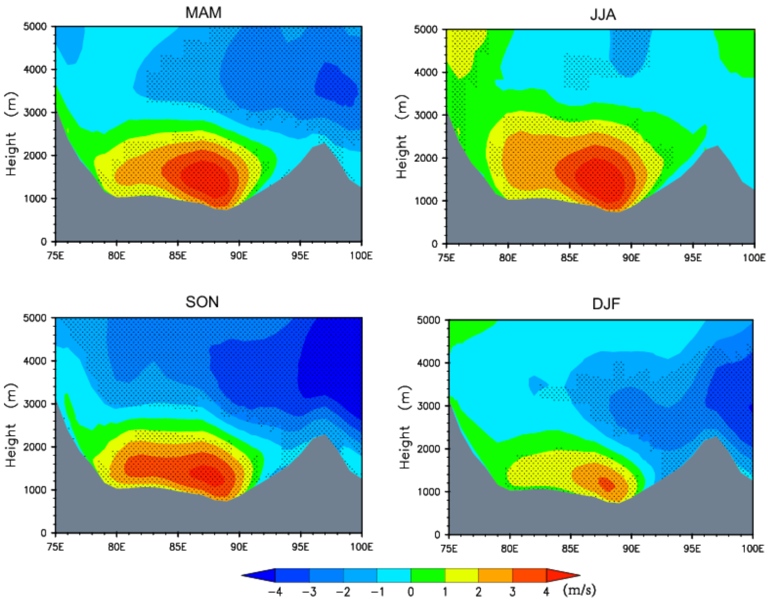

Figure 4. Same as Fig. 10 in the manuscript, but all time-matched AOD and wind profile data are used.

L. 386-388: Please add reference.

Response: Reference is added.

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?

Reponse: Technical corrections have been made in revised manuscript.