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Comment on acp-2021-889 by Liu et al.

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Referee comment on "Evolution of turbulent kinetic energy during the entire sandstorm process" by Hongyou Liu et al., Atmos. Chem. Phys. Discuss.,
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In this paper, Liu et al. investigated the turbulence structures and scales evolving during the rising, steady-state, and declining stage of a sandstorm. Investigation of a real sandstorm through a lens of wall-bounded turbulent flow dynamics is interesting and meritorious. However, in my opinion, a number of major issues need to be addressed before publication.

General Comments

1. The paper fails to investigate local meteorological and synoptic conditions associated with the case of the sandstorm studied herein. This investigation is crucial as weather features are expected to directly impact large and very large-scale motions (LSMs and VLMSs) of turbulence. Specifically:

1.1. The sandstorm event studied here must be described in details in Section 2.1, including the date/time, weather conditions, potential meteorological drivers, etc (see for example Gasch et al., 2017). Without this information, all the discussion of results regarding the onset of the sandstorm and the link between LSMs/VLMSs and synoptic conditions is questionable.

1.2. Throughout the text, authors referred to the study by He et al. (2020) to describe the physics and meteorological drivers of a sandstorm. This is problematic, because He et al. (2020) investigated a mesoscale convective dust storm generated by cold pool outflow (AKA haboob), which is drastically different that a synoptic-scale dust storm. (see Knippertz (2014) for more information). More concerning is that the paper describes 'synoptic events' and 'cold front' in a sandstorm on the basis of the study of He et al. (2020), who looked into a haboob sandstorm.

2. The structure of the paper should be improved. Specifically:

2.1. The paper should be shortened:

- Remove Figure 3 or move it to a supplementary information document as it is simply a repetition of the text (lines 162-174).
- Figure 4 and the discussion around it (lines 175 -192) seems to be out of place and should be moved to a supplementary information document.
- The spectral method (section 3) is a well-established approach in the study of turbulence, and the contribution of this work in terms of methodology development is not clear. Therefore, I suggest this section to be shortened and the text to be moved to a supplementary document.

2.2. Lines 198 to 213 should be presented earlier in the paper together with the discussion around Figure 2.

Specific Comments

- The segmentation method described in figure 3, involves a number of subjective criteria including the IST threshold (30%), the time window used for initial time-averaging (1 hr), and dt (5 min). The uncertainty of these choices in final results should be studied and discussed. Specifically, after applying the data processing procedure the size of all segments ended up being very close to or exactly 1-hr which was the initial choice for time-averaging and removing the time-varying mean. One may ask whether the 1-hr initial choice could basically govern the whole procedure and making the entire segmentation algorithm irrelevant. A sensitivity test should help answering this question.
- Figure 2(a): Can authors comment why the time-varying average velocity obtained by the EMD method contains low frequency fluctuations in the rising stage, which are absent in the other two methods (moving windows and adaptive wavelet transform)?
- The studies of Kim and Adrian (1999), Guala et al. (2006), and Balakumar and Adrian (2007) have been referred to throughout the text to describe and identify LSMs/VLSMs. All these studies investigated turbulent channel and pipe flows (internal), rather than a true turbulent boundary layer flow as relevant to a sandstorm (external). Monty et al. (2009) concluded that VSLMs in boundary layers are different from those in channel and pipe flows (e.g., as in Kim and Adrian (1999)). Therefore, there is a concern in

using results/criteria from internal flows in the case of a sandstorm with very high Reynolds number.

- Figure 6: What is the difference between subfigures (a) and (b) , (c) and (d), (e) and (f) ? It was neither mentioned in the caption nor discussed in the main text.
- Figure 7(b) and lines 330-333: The sharp decreases in the declining stage were attributed to the exhaustion of energy at this stage, but why there is a maximum right when the declining stage is started and before this sharp decrease?
- Figure 8: The two fraction numbers contributed by VLSMs of 75% and 40% reported throughout the text were obtained from this figure. As this fraction is changing with height, it is crucial that either the location where the fraction is reported be mentioned everywhere in the text or an average value below a certain height be reported. It seems that the two reported fraction values (75% and 40%) are simply the limit of measurements in terms of height.
- The Taylor's hypothesis of frozen turbulence has been used throughout the text. Does the level of turbulence intensity (i.e., fluctuations compared to the mean wind value) justify this approximation?
- Figure 11 and the text around it: How are the "small-scale motions" defined? (This point may be linked to point 3 above questioning the criteria to define VLSMs).
- Figure 11: Including an inset in (b) and (c) are quite confusing. I think the plots for all the heights can be presented as the main figure instead of being included as an inset.
- Line 481-486: This statement seems to be an overgeneralization of the lifetime of a sandstorm based on observations of a single event (This point is directly linked to my first point under general comments).
- The data provided in the Zenodo data repository has no metadata, data header, or any information to help using this data.

Technical Comments

- Line 14: check for grammar correction
- Line 19: use "humidity" instead of "dampness"
- Line 24: "A kind of power": sounds awkward
- Line 27: "... impact on sandstorm more intensively, significantly, contributively than other..." : sounds awkward
- Line 49 and throughout the text: use "transport" instead of "transportation"
- Line 85: "necessary": Do you mean "ideal" or "suitable" ?
- Throughout the text: I suggest using "surface" instead of "wall". I understand that "wall-bounded turbulence" is an established term, but the word "surface" or "ground" seems to better suit an atmospheric application.

References

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Monty, J.P., Hutchins, N., Ng, H.C.H., Marusic, I. and Chong, M.S., 2009. A comparison of turbulent pipe, channel and boundary layer flows. *Journal of Fluid Mechanics*, 632, pp.431-442.