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Interactive comment on "Identification of dust sources and hotspots in East Asia during 2000–2015: implications for numerical modeling and forecasting" *by* Xuelei Zhang et al.

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Received and published: 9 March 2017

This paper makes some potentially useful contributions to our knowledge of dust sources over Asia, but the important findings are significantly obscured by a lot of rather distracting and sometimes superfluous text, much of it including unsubstantiated statements. The scientific effort in this paper is commendable. It is a thorough analysis of dust sources right down to the scale of very localised hot spots, but in its current form the paper doesn't deliver the main points that are summarised in the abstract.

The abstract states that "we reassess the accuracy of previous predictions of trends in dust variations". This is a difficult statement to understand, and I was not convinced that the paper really does this. First of all, what is meant by the "prediction of a trend"? A

historical trend is quite different from a prediction. Secondly, what is meant by "trends in dust variations"? Trends and variability are two different aspects of a dataset. In the paper itself, Figure 3 is the only place where trends and variability are presented, and the associated text makes what appear to be statistically unreliable statements about the changes. Vague statements like "slightly increasing trend overall" and "clear decreasing frequency" seem not to be supported by the data. Are the trends really statistically significant? Have you considered the uncertainty on each datapoint? The statistical analysis is very weak. Later you state that "Due to our limited data (16 years), this long [11 year] cycle cannot be identified in our power spectrum analysis; however, an obvious, long-period cycle can be seen through visual inspection, with troughs in 2003 and 2014". Please either perform a proper statistical test to confirm your subjective view, or remove the statement from the paper. Finally, to what extent do you really reassess the accuracy of previous studies. I was not convinced that you really do this.

In the abstract you also state that "Recent changes in land use associated with anthropogenic activities (mining and excessive exploitation of water resources) are revealed as one of the major factors leading to an expansion of dust source regions..." I was not convinced by your analysis on this point. You analyse a small number of the hotspots and point to anthropogenic activity, but you don't make a convincing case that changes in activity are related to expansion of dust source regions. It seems logical that they might be associated (dust is being uplifted from regions with human activity), but your data do not show that the changes are linked. This link needs to be either substantiated with evidence or you would be advised to remove any reference to the anthropogenic contribution to changes in dust events in your record. One confusing aspect of this statement is that your paper deals with dust events, while this statement refers to the expansion of source regions, which one would expect to be linked to the amount of dust uplift rather than the number of events.

The abstract states that "Trajectory analysis also shows that dust can even be trans-

ported northwards by the Mongolia Cyclone, to the Far East region and even the Arctic Circle, potentially affecting the climate and ecosystem of the Arctic region." The extent of your analysis here is very limited. It is not disputed that mid-latitude aerosols can be transported to high latitudes, and your study doesn't add much to this knowledge, so I suggest to delete this statement in the abstract.

You state that "After detailed comparisons and evaluation of different algorithms to detect our pre-selected moderate dust event (May 11, 2011) over East Asia, Method 4, which combined visible bands and infrared bands, was selected as being the most suitable algorithm for differentiating dust from land and clouds in this study (Figure 2)." This seems to imply that the whole methodology for 462 events is based on the study of one of them. If you have done "detailed comparisons and evaluation" then please provide at least some summary statistics or evidence to convince readers that the other methods were inferior.

The paragraph on line 237 was confusing: "For the majority of events and algorithms, the published or indicative thresholds under-perform and the values vary from event to event. This makes it difficult to suggest appropriate regional scale thresholds and each event was manually adjusted in this study. While some of this variation is due to factors specific to the algorithms or individual events, other factors such as diurnal and seasonal variations in surface temperature/dust contrast (which affect BTD) will affect all the methods." Does this mean that you are not using the simple threshold stated in the table, but are adjusting each event? If so, your classification system needs to be defined better so that we can understand the subjective approach better. Otherwise it will be impossible for someone to reproduce or compare with your study.

On line 255 you say you compared four trajectory methods and found one was better. Please show the comparison and state more clearly what was superior about Hysplit. I am also missing an analysis of the accuracy of source region identification. How accurately can the sources be pinpointed? It's not at all clear how you actually use the trajectory analysis. Are all the dust events essentially plumes that can be tracked back

continuously (from the MODIS images) to the source? If some dust events are isolated from the source, which part of a large blob of dust do you initiate your back trajectories from? Surely some of the events must be spatially very extensive.

To be useful for modelling, the hotspots on Figure 8 need to be connected much better to your back trajectory analysis. Please show the location of the starting points of all your trajectories on such a map. Presumably they cluster on these hotspots. At present the reader has to take on trust that this is the case. It also appears as if there is some kind of subjective step involved in drawing these very neat hotspot regions on a map, so the methodology and the evidence needs to be presented so that readers can assess the accuracy of your analysis.

On line 304 you state that "The variations in dust events appear to be controlled by atmospheric dynamics (temperature, precipitation, wind velocity, Mongolia cyclone, polar vortex and Arctic Oscillation), land surface characteristics (soil moisture, desertification, vegetation) and human activities." I could find no firm evidence for any of these links. Please provide this evidence. At present your evidence seems to come from an embedded review of the literature.

Figure 9 is a useful comparison of your hotspots against long-term AOD measurements. Is this an original figure? Please state more clearly how it was generated and also improve the quality.

Your policy statement on line 625 is not well substantiated. If at all, I would mention such things only in the summary.

I could not find the Appendix table listing the dust events.

Section 4 on implications for dust modelling does not clearly define the implications. It's quite vague, and again seems to draw more from previous literature than your own data. Overall, this is a long and rather verbose paper that would be greatly improved if it was much more focused on the hotspot dataset and didn't stray so much into literature

review and unsubstantiated claims. I recommend to reduce the paper, provide much clearer evidence for your results and conclusions (as defined above and in the other reviewer's comments), and compare more rigorously with previous analyses. Any conclusions about the implications for dust modelling need to be based on clear differences between your results and previous studies.

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Discussion paper