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**Interactive
comment**

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.

Anonymous Referee #1

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This paper investigates multiple aspects of dust sources in East Asia, using satellite data to locate the hot spots, and compares them with other source inventories. They did also some trajectory analysis to determine long-range transport of dust from these sources. Finally, they make some recommendations for modeling dust in East Asia. This would appear as an interesting scientific objective worth publication in Atm. Chem. Phys. Unfortunately, the analysis is constantly staying superficial, while touching aspects (see below for examples) irrelevant to their objectives. As they do not describe and present properly their methodology and results, some of their conclusions seem unsubstantiated, speculative or false. Scientifically, this paper is below standard and I would not recommend its publication in Atm. Chem. Phys.

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Discussion paper

Their main contribution is to have analyzed satellite data over 16 years with a few kilometers resolution, although this not original for the region. They provide a schematic of the workflow of their satellite analysis, but they never show that it works with one case study for example. Also, in their schematic some datasets are not described (MODIS D08), used wrongly defined data (contrary to what they wrote MODIS Deep Blue on Terra has been accessible for at least a year), described in the text but does not appear in the flowchart (MODIS Level 1B), or using old data (Collection 5 MODIS aerosol products instead of collection 6). It seems as if this algorithm was developed 5 years ago and have not been updated since. For "validation" of the different aerosol-retrieval algorithms, the authors provide a figure comparing the retrieval of one dust plume with two other methods and one snapshot of a true color image. This is a purely qualitative comparison, and not a proper validation. Without going further in term of presenting their results they show annual and seasonal trends of dust events. They compare their total count for the entire China with ground-based data measured in the Tarim basin, which is misleading. They also compare with another study (Zhang et al., 2008) using the same MODIS satellite products, but their results differ by 50%. Instead of spending time trying to understand the origin of such discrepancy, they link the trends with ENSO, and even the solar cycle! They pursue by discussing over one full page the importance of landuse change to dust trends by reviewing the literature. But the link between landuse and their results is tenuous if existing. The next section is related to the regional distribution of dust sources. They start by showing an image previously published without adding one of their own results. I am not sure why they would do this. Often they make unsubstantiated assertion. This section is no exception, and wrote that a difference between African and East Asian dust sources are the former are heterogeneous regarding soil properties. Contrary to what the authors wrote, a large number of studies indicate that African sources are heterogeneous as well. After this transgression, they come back to their main objective and divide East Asia into sub-domains. For unclear reasons (they just wrote that it is based on regional characteristics) their sub-domains don't match previous studies, which will restrain fruitful

comparison. Then, follow several pages of a literature review relative to the geomorphology of the sub-regions. They provide a long list of local names, which are never geolocated on a map. The interest of such list is unclear, but is certainly a frustrating distraction. They include in this section, a little bit of forward trajectories. But the methodology is poorly describe or shown with Figures. The only shows some arrows, apparently draw by hand, and only three trajectories! It is dubious that 3 trajectories could help provide any interpretation of lon-range transport. A bunch of Figure shows true-color images of dust plumes coming out of dry lakes. Unfortunately, they are not geolocated (time and space) which limits their usefulness but on the other hand fuel our frustration. Then they discuss again geomorphology using MODIS landcover dataset. There is no information on geomorphology in MODIS landcover dataset (MCD12Q1) but landcover types. This is troubling because they provide in Table-3 the erodibility features of dust sources. So, where is this geomorphological information coming from? Finally, they show a Figure (Figure 8) with some of their results (spatial distribution of hot-spots over a map of East Asia). The choice of colors could be improved as it does not allow to differentiate between medium and intense hot spots. Follow a poor figure (Figure 9) showing the long-term mean (2000 to 2015) aerosol optical depth retrieved from MODIS at 1-degree resolution. With this Figure, they make dubious discussion about dust transport to the Tibetan Plateau, making make reference to the controversial theory of 'elevated heat pump". The circulation over the Tarim Basin is not related to the existence of aerosols over the Tibetan Plateau but the blocking of surrounding high mountains. As an additional distraction, they include some discussion about the development over the years of a saline factory in the Tarim Basin. They pursue with more discussion on landuse using a plethora of local names. They finish this section by providing some recommendations for policy makers! The last section before conclusions is supposed to compare different source inventories. Once again the subject is lightly brushed (one paragraph) with unsubstantiated conclusions. Their description of the inventories contains false information (e.g. Table 4) or inaccurate summary of previous studies. Sometimes, it is unclear if they even read the papers they cite. This

is unfortunate as this could have been the most interesting part of the paper.

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