

Interactive comment on “Global aeolian dust variations and trends: a revisit of dust event and visibility observations from surface weather stations” by Xin Xi

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Thanks very much for this interesting work. I welcome the revisitation of the global dust studies as satellite and modeling improve, it is always needed to reevaluate the subject. Also, big picture analysis of dust activity in the context of climatic indexes as shown here are always welcomed.

Without providing a long review, I'd like to add a few comments and clarifications which I believe are in order and if the author (or editor) thinks so, I think it would strengthen the paper.

C1

My main comment is that the title of the paper conveys an idea that it is not quite consistent with the evidence shown in the paper. Largely missing in this paper is the subject of dust activity at high latitudes (HLD for short) and with this regard, this is an important omission of an important development in the last few years with regards to global dust characterization. Here I list a few sources of information regarding HLD that illustrate my points:

A.1 A major review on the subject was published by Bullard et al (2016). In addition, a large database of publications on the subject can be found here <https://icedustblog.wordpress.com/publications/> (mostly references related to Greenland and Iceland dust activity)

A.2 The satellite data used in this study (MODIS data from the Voss and Amato database) is only characteristic of observations from 45S to 50N, thus it does not include any of the high latitude dust sources reported in the studies from the previous section.

A.3 Surface Visibility and Synop codes have been successfully used to characterize high latitude dust activity in Iceland (see above blog for references) and in Patagonia (Gasso and Torres, 2019, Gasso et al, 2010, Gasso and Stein, 2007)

I believe that just an appropriate adjustment of the title is needed in order to reflect that this study is not global.

In addition, where are a few clarifications that jumped out when reading the paper:

B.1) What is the time resolution in the model-satellite comparison? specifically is the model sampled at the same time of the model overpass? This information would be useful to guide future research based on your analysis.

B.2) What is the density of stations used in this study? what regions are not well captured by the surface and satellite data?

B.3) In figure 4, there is a singular dot in South America, possibly in Chile or Argentina.

C2

From my own work, I am familiar with the station Tinogasta in Argentina (by the Andes mountains where this dot is located). this is station is consistently biased to report more dust activity than actually is. I found this out by talking to Argentina's weather bureau central data archive manager. I suggest removing such point.

B.4) Is there a consideration for the fact that a large amount of dust activity occurs in cloudy conditions? Satellite polar observations are biased low not only because 1-2 obs per day in a given cite but also because cloudiness, which tends to be more pervasive towards higher latitudes. See Gassó and Torres (2019) for more on this.

I believe that all these issues can be easily addressed and turn this fine work into a more complete study.

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C3

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C4