

# ***Interactive comment on “Dust deposition and ambient PM<sub>10</sub> concentration in central Asia: Spatial and temporal variability” by Xiao-Xiao Zhang et al.***

## **Anonymous Referee #2**

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The authors analyze dust deposition and concentration data collected in the Xinjiang Province in northwest China over a time period of 13 years. Spatio-temporal differences in annual and 13-year averages for 14 stations are investigated.

The manuscript is overall well written and organized and the presented data and analysis are valuable and interesting. However, my impression is that interpretation of the data is relatively shallow and I think that further in-depth analyses, background information, and more detailed interpretation would be needed before the manuscript can be considered for publication in ACP. Please see my specific comments below.

1) P. 2, l. 10-11: The authors motivate their study by stating that Pye (1987) suggested a lack in reliable dust deposition data. This reference is almost 30 years old. I

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would suspect that more data has been collected since. In fact, the authors list several newer references for data on dust deposition in their Table 1. In my opinion a more comprehensive overview and discussion on currently available dust deposition data is needed.

2) P. 2, l. 25-26: What are the processes governing dust emissions, transport, and deposition in Asia? And what data was used by Shao et al. (2011) and Groll et al. (2013) as the basis of their findings (note that the reference should be Shao et al. (2011) rather than Shao (2011))? I think these questions need more attention in the paper, especially to support interpretation of the spatial and temporal variability observed in Xinjiang Province. How is the data presented in the manuscript on hand different/better than the data used in earlier studies? What drives the trends and spatial variability? These questions appear mostly unanswered in the present paper.

3) Are the 14 environmental monitoring stations the same as the air-quality monitoring stations where the API is obtained from?

4) P. 4, l. 3: “Dust-in-suspension constitutes days...”. Present weather reports refer to the time of observation, not to the whole day. What category is used for a day in p. 7, l.4 if the 3-hourly data shows two different reports on the same day, e.g. blowing dust and dust in suspension? The authors have chosen to not show the results of their present weather report analysis (p. 7, l. 6). However, it would be interesting to see the outcome and compare to earlier studies using a similar method (see first reference in my comment 8).

5) P. 5, l. 2: It is stated that high dust depositions in the industrial belt were caused by “industry, coal burning and vehicle exhaust”. What are the underlying references or data used to support this statement? Or is this a hypothesis? Does the API data used later in the paper provide any evidence in that regard?

6) P. 5, l. 31: Based on their data, the authors “suggest a positive relationship between dust deposition and PM10 concentration”. This is to be expected as – apart

from wet scavenging through precipitation – dust deposition is caused by gravitational settling and turbulent diffusion. Both processes are dependent on dust concentration, i.e. the higher the dust concentration, the higher the dust deposition. I think a more detailed discussion of the observation results on the basis of the physics underlying dust deposition would be needed here. Correspondingly, I would suggest to present dust deposition as a function of PM<sub>10</sub> concentration in Fig. 7 (and discuss results accordingly) rather than vice versa.

7) P. 6, l. 20-21: The authors state that “weather appears to be a dominant factor” driving dust concentration and deposition in arid regions. This is very vague and only very few details are discussed in the following. It seems clear that atmospheric and land-surface conditions are decisive for local dust entrainment and that atmospheric flow determines dust transport. A more detailed discussion of the predominant regional circulations in Xinjiang province would also help interpretation of the spatio-temporal variability of dust deposition in the area.

8) Section 3.4: How do the results obtained in this paper (e.g. trends) compare to earlier studies on dust variability in central Asia (see for example Shao et al. (2013, doi:10.1002/jgrd.50836) and Xi and Sokolik (2015, doi:10.1002/2015JD024092)). I think more consideration need to be given to previous works, even though they might not deal with the exact same area.

9) P. 7, l. 19-20: “These results suggest that dust source[s] in central Asia affect regional air quality and [are] a potential contributor of global dust.” Other studies (e.g. Shao et al., 2011, doi: 10.1016/j.aeolia.2011.02.001, and references therein; Huneus et al., 2011, doi: 10.5194/acp-11-7781-2011; Ginoux et al., 2012, doi: 10.1029/2012RG000388) have shown that some of the world’s major dust sources are located in central Asia. Please rephrase the statement so that it becomes clear in what way the present results support earlier findings, and in what way they may differ.

10) P. 7, l. 31: “. . .this work can aid in adjusting model parameters. . .”. While measured

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dust depositions can certainly be used to evaluate dust model outputs, it does not seem that this is a direct follow-up on the present manuscript or an objective of this study. In my opinion, this work can rather – if further detailed discussions are added – support understanding of dust deposition along with its spatio-temporal variability in the study area (of course this could then also support model development and evaluation in the future) and I would suggest to motivate the paper as such.

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