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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Growth in mid-monsoon dry phases over Indian region: Prevailing influence of anthropogenic aerosols" *by* Rohit Chakraborty et al.

Anonymous Referee #2

Received and published: 17 March 2019

The manuscript describes possible correlations between absorbing aerosols and drought over parts of India and Lucknow in particular to increase in aerosol concentrations over the past sixty years and more appropriately for the past 30 years when satellite datasets of aerosol loadings are available. There is a lot of hard work and analysis that went into the study and appreciate the effort put in by the authors. Unfortunately, they are challenged by the lack of specific data sets that will help tease out any such effect and the possibility that this may not be an easy trend to distinguish with the current available data. In the end this manuscript goes in so many directions that it is hard to follow and some of the relationships discussed are thinly supported and probably contradictory. The essential problem is that when there is no rain the aerosol

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Discussion paper

loading will increase due to the absence of washout. May be this is bit debatable when there is a non-hydrophylic BC is present, but when using AOD as an indicator washout is a fact. Essentially, most of the India with anthropogenic aerosol pollutants that AOD can be expected to recover quickly after a rainfall (washout) event. This implies that the dry days will have more aerosols(AOD) in the atmosphere and there will be a pretty good correlation between dry days and AOD over much of India during monsoon in places with significant anthropogenic pollution. Then to argue that more AOD leads to more dry days, we will need a testable hypothesis. I didn't see any such hypothesis in this manuscript or I might have completely missed it. For example, separating the data into high AOD days of few days and the next rain event, holding all other factors constant, to days with low AOD days with similar set of meteorological conditions and show that the next rainy day is extended by x number of days may prove the authors point. As the manuscript stands now, it doesn't. A second issue I have is the definition of DDF, it described as a frequency. The way it is computed seems like it is simply a daily count of number of days with less than 1mm/day rainfall. When these types of duration statistic is used in hydrology or climate statistics the frequency is given a period 3 day or 5 day (example a heat wave is a 3 day event with greater than 95F maximum temperature in the USA). One day of less than 1mm rainfall does not lead to drought, there has to be a sequence of such days to create this event and this statistic should be modified to prescribe a consecutive number of days over which this is calculated. Third issue is that the DDF and DI as described here are essentially precipitation derived and not independent. It just happens that DDF is high frequency description of DI and one would expect them to be correlated when the precipitation is in the same direction as DDF(low). I don't see the value of correlating DDF to DI in this context. Again, I may be missing something. Fourth big issue is the cloud analysis. It seems like the cloud analysis is used to suggest that there is a decrease in drop size an increase cloud lifetime (more cloudiness) and a decrease in rainfall (drizzle suppression) all lending credence to aerosol second effect. I think this is an interesting insight coming from the data. However, I couldn't figure out where all this data is coming from (MERRA2?).

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This needs more supporting data than presented here, I am not even sure what to make of figure 7, so the cloud droplet radius (I am assuming that is what this is? it cloud particle radius for low level clouds?) has decreased by >11% over Lucknow over the past 10 years in MERRA data. Can you also discuss what uncertainties are there is MERRA data estimates of cloud droplet size? This requires a separate paper and much more detailed analysis and support than presented here. Fifth, why sunspots? Is there a physical reason why sunspot activity effects precipitation over India? Last but not least the dust transport analysis seems irrelevant and adds no information to the paper. Also, let us not forget population density thrown into the mix, how does that effect precipitation and dry days and through which physical mechanism?

Overall, this manuscript describes a jumble of statistics trying to connecting what could probably a flawed 'duration' statistic to a number of randomly selected parameters to make a point that is not well supported.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2019-1/acp-2019-1-RC2-supplement.pdf

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