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Comment on acp-2021-750

Anonymous Referee #2

Referee comment on "The relationship between $PM_{2.5}$ and anticyclonic wave activity during summer over the United States" by Ye Wang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-750-RC2, 2022

In this manuscript the authors explore one aspect of the known connection between meteorology and pollution, focusing on statistical correlations between anti-cyclone wave activity (AWA) and fine particulate matter (PM2.5). This is a useful topic, and one which has attracted a great deal of attention in recent years in the hopes of better understanding variability in observed pollution levels as well as improving and constraining projections of future climate impacts on pollution. While I see some promising steps and interesting results here, I also see serious omissions that make it hard for me to understand and accept the final conclusions being drawn. I would like to see significant improvement and clarification in several areas before I can recommend publication in ACP.

General questions and comments

- **PM2.5 speciation**
  Unlike ozone, previously explored by the authors using a similar methodology, PM2.5 is composed of a wide array of compounds derived from many different precursors. The treatment of PM2.5 here, though, seems to completely ignore this detail and handles all particles as identical. Without any individual treatment of speciated PM2.5, or even discussion of how variability in composition across the United States may be affecting relevant formation/loss mechanisms or final correlations, it's very difficult to accept any of the results or conclusions. This aspect of the analysis needs serious work to address this dimension.
- **PM2.5/AWA mechanisms and correlation with other local meteorological metrics**
  While at times it is mentioned that AWA has ties to local meteorological variables such as temperature and stagnancy, these correlations are never shown, quantified, nor discussed in any detail, and their potential covariance is crucial in terms of explaining the mechanisms underpinning the presented relationships. If it is being argued that variability in AWA can explain "up to 75% of interannual PM2.5 variability across the US", it behooves the authors to go into greater detail on the exact mechanisms from which this predictive power stems. Are higher temperature driving increased emissions? Is it transport related? Absence of wet deposition? As it stands this open question is the elephant in the room of this manuscript's discussion and conclusions, and needs to be addressed (ideally with conclusive and quantitative analytical tools).

Specific section questions and comments

- Section 2.1: Unless I am misunderstanding some key detail, 1000 observations between 1988 and 2014 seems like a very low threshold for data availability, with potentially very significant biases due to missing data. Were these observations counted year round, or only from summers? Were there any notable biases or differences between selected stations in terms of whether the observations occurred early or late in the temporal domain? Also, why does this year range (1988-2014) differ from the date ranges used in other data sources (for example 1991-2010 for ERA-Interim data)?
- Section 2.2: I'm concerned about the inclusion and comparison between free running and specified dynamics model output, as these two types can differ in very significant ways, even for otherwise identical years. In particular, Figure 3 (and related analysis) shows a very strong difference between REFC1SD and GCM2000. How much of that difference could be an artifact of the differing dynamics? Do the overlapping years between them (2006-2010) show good agreement in AWA? If not, this has major consequences for the interpretation of 3b and 3d.
- Section 2.6: This description needs some serious clarification. I was not able to clearly understand this description of "composite methodology" until piecing it together from later sections and figures.
- Figure 2: With exclusive focus on summer conditions in this paper, what is the relevance of including full annual cycles of PM2.5? Furthermore, what is the relevance of agreement between monthly average values year round (9 months of which are completely ignored in the rest of the paper) rather than daily values within the chosen JJA domain? Far more useful, (and legible) in my view, would be box or column plots showing summertime means/ranges for each of the different sources of data or model output, with correlation statistics provided for daily comparison to observations.
- Section 3, site selection: The LAVO1 site seems like a problematic choice, relative to the other two. Not only is PM2.5 not particularly high during the summer, there appears to be a very high degree of variability during those months. Is there a reason for its selection? Also, considering the final conclusions focused on the importance of the AWA in the Midwest and Great Plains regions, wouldn't it be beneficial to have at least one site within one or both of them?
- Figure 4: What is the meaning of the contour lines in this figure?
- Section 3.1: "averaging together all AWA corresponding to daily PM2.5 above the 90th quantile shows a similar strong correlation between PM2.5 and AWA" Unless I'm missing something, I don't think "correlation" is the correct word here (and elsewhere in this section). It suggested to me that an additional regression was being performed for days with PM2.5 > 90th percentile, but I don't think that is correct. Please clarify -- without additional information on behavior of other PM2.5 percentiles, we can't really define correlation from this subset of daily conditions.
- On a related note, I don't really see the benefit of including/discussing both regressions (Fig 4) and high PM2.5 filtered subset (Fig 5). What is gained from this comparison, beyond noting that they are somewhat consistent? It seems neither surprising nor useful to me.
- Figures 6 and 7b: The diverging colorbar used in these figures does not appear to be appropriate, as the neutral/lighter shades and color split does not occur around zero. The choice creates an artificial split dividing the maps between red and blue that does not seem to have any purpose. Please change to a single color option, or equivalent. Figure 7b also needs more work and cleanup, especially with its legend and units.
- Section 3.3, quantile regression: The increasing sensitivity for higher percentiles is

interesting and potentially important, but it is also important to acknowledge the huge range and overall poor predictive power in this correlation. An r of 0.36 implies that the vast majority (nearly 90%) of all variability is being driven by factors other than AWA at the AREN1 site. Alongside the examination of increasing quantile sensitivities, it must be noted that this lack of overall correlation implies other (and likely MUCH more important) drivers of PM2.5 variability at sites such as this. This is one area in particular where introducing PM2.5 speciation may prove crucial for understanding differences in bulk aerosol behavior.

- Section 3.3, quantile regression differences: This comparison and examination of Figure 7b seems a little unclear and potentially misleading, as a very high fraction of sites are in the [0 1) bin, suggesting a very small (and potentially insignificant) difference between 50th and 90th percentiles. A better choice of colors is needed to distinguish tiny differences (of either sign). More robust tools to evaluate and compare quantile regression coefficients are available, and should be used to establish significance here.
- Section 3.4: This section all became very hand-wavey to me. I'm not clear on many of the decisions, methods, and conclusions being made here, and more explanation and discussion would be appropriate. In particular, I don't understand the switch to "interannual variance", nor how that was calculated. Previous examinations looked at daily variability -- why the switch to interannual? How many years were included in this calculation? 75% explained variability seems VERY high to my eye, given previous figures and results. I would need to see more time devoted to explaining how this regression was put together before I could accept it at face value. The spatial inconsistency with other studies is also of concern. How robust can we assume this result to be, given the apparent inconsistencies? What does it mean to say "where meteorology dominates", and how is this defined?