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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Understanding meteorological influences on $PM_{2.5}$ concentrations across China: a temporal and spatial perspective" *by* Ziyue Chen et al.

Anonymous Referee #3

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General Comments

This is an interesting paper that applies an exciting and fairly new statistical method (convergent cross mapping) to quantify the relationship between local air quality and local meteorology (wind, temperature, precipitation, etc.). The authors argue that unlike a simple correlation analysis, this method is able to demonstrate causal relationships between variables. My understanding is that this method is quite new, and it is central to this study, so I think this paper would benefit from a clearer discussion of why it is better than correlation at determining causal relationships. Would the authors' findings have differed significantly if they used correlation instead of CCM?

There are some aspects of CCM that were not clear to me. Does CCM account for relationships between meteorological factors? E.g. if wind is affecting both precipitation and PM, how is the affect of wind on precip on PM counted? Another question I have is what produces a large value. For example, are the values higher in winter simply because there is more PM available to be effected? Or as another example, is precipitation more effective at removing PM along the coasts because it rains more? If there were a way to normalize by the amount of total rainfall, would precip still be more important along the coasts than in the drier interior?

My second major concern is that this study uses a single year of data to make general comments about PM-meteorology relationships. This gives us little sense of how much year-to-year variability may exist in these relationships and generally weakens the conclusions. If CCM is too computationally expensive to use on multiple years, perhaps a different method could be used to supplement it.

Finally, I would like to see a deeper discussion of the scientific significance of this work. As it is written currently, this paper is almost purely descriptive. The new method is interesting, but the paper could do a better job of articulating what we are learning from it. Perhaps some discussion about why different meteorological factors are more/less important in different regions/seasons, for example, would help give the paper more depth. I would also suggest spending more time discussing the implications for modeling, especially in the introduction and conclusions, as that was what I took away as the most important implication in this paper.

Specific Comments

pp 3, ln 93-95 - Can you elaborate further on how your previous study showed that CCM is better than correlation for the benefit of readers who have not read that paper. It is important for your results here to make as clear as possible why CCM is a better method/provides new insights.

pp 8 ln 221 – Are the results sensitive to the choice of parameters?

pp 9 ln 237 – how is the value of determined? Are you calculating the limit or taking the value at a specific time series length? What about cases such as PM2.5 xmap minTEM, which (at least by eye) does not appear to be converging? Wouldn't that suggest that minTEM was not influencing PM2.5 at all?

pp 9 ln 244 – It's not clear here how PM is changing wind speed

pp 10 ln 270 - I would guess that (out of the 189 cities) those clustered regionally would show similar maps. Is that true? I.e. are the 37 cities shown representative of the cities not shown?

pp 13 ln 292-296 – This only seems to be true in some seasons.

pp 13 ln 299-300 - Can you quantify this more rigorously? By eye, it seems like there are enough outliers to call this in to question

pp 14 ln 337-339 – This seems true for winter vs. summer, but what about spring vs autumn?

pp 18 Fig 4 caption – Is there a particular argument for only including the dominant factor in each city?

Section 5.1 of the discussion feels out of place. This is more of a discussion of what we already know about aerosol-meteorology interactions than a discussion of the implications of the work done in this study. I would recommend either rewriting it so that it builds more on the results from this paper, or cut it and integrate the important information into earlier sections.

pp 24 ln 562-566 – as per my earlier comment, I am not sure that you have shown this. If Beijing were to receive the same amount of precipitation that a coastal city does, is it possible that precipitation would become more important in Beijing? Does looking at how a specific factor changed PM in a year tell us about how effective changes in that factor would be?

Technical Corrections

pp 1, ln 17 (and later occurrences) - "causality influence" is redundant. "Influence" already implies causality.

pp 2, ln 35-36 - "Amongst these environmental elements, ... concerned social and ecological issues." The wording of this sentence is unclear.

pp 2, ln 42 - "Serious haze not only influences peoples daily life," this wording is vague. How does haze influence peoples daily lives?

pp 2, ln 57 - "controversy" should be changed to "controversial"

pp 3, ln 68 - "... fractions of three different sizes..." this is unclear. Authors should indicate that they are talking about aerosols, and specify the sizes.

pp 3, ln 7 - what region is this study referring to?

pp 3, ln 86 - word choice: I would suggest "well studied" instead of "massively studied"

pp 4, ln 119 – what does API stand for?

pp 5, ln 136-137 – I'm not clear on what small and large evaporation are.

pp 5, ln 144 – sunshine duration for the day is a less widely used term and should be defined here

pp 5 ln 146 – what qualifies as extreme wind speed?

pp 5 ln 146 – how is max wind direction defined?

pp 6 ln 174 – "Two time series" is unnecessary. Suggest changing the sentence to "{X}=[X(1),...X(L)] and {Y}=[Y(1),...Y(L)] are defined as the temporal variations of variables X and Y."

pp 6 ln 175 – It's unclear what r and S are.

pp 8 ln 217 – why can E be 2 or 3?

pp 14 ln 331-332 – the phrasing here is unclear.

pp 18 ln 374-376 – is there a way to test if these values are significant?

pp 20 ln 419 – this wording is unclear

pp 20 ln 426 – Wikipedia is not an appropriate source. Better to cite a scientific paper that defines SSD.

pp 20 ln 440 – Temperature inversion is certainly important, but none of the metrics in this study measure it directly.

pp 21 ln 447 – what about horizontal transport (advection)?

pp 21 ln 446 – change “social economic” to “socio-economic”

pp 22 ln 492 – change “negative causality on” to “decreases” and “positive causality on” to “increases”

pp 23 ln 527-528 – do you have citations for this?

pp 24 ln 544 – can you give more details/citations about the controversy?

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