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Interactive comment on "Comment on the paper "Insignificant effect of climate change on winter haze pollution in Beijing" by Shen et al. (2018)" *by* Run Liu et al.

Anonymous Referee #1

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This comment paper raises doubt on the recent paper by Shen et al. (2018), which draws the conclusion that the effect of climate change on winter haze in Beijing is small and uncertain. The authors point out three issues with Shen et al. (2018), which I think are reasonable arguments. The authors well addressed the questions posed by the Shen. The thoughtful debate on this controversial topic is worth publication at ACP, though I don't think the comment alone could nullify the conclusions of Shen et al. (2018). I have a few comments:

1. A major disagreement between Liu and Shen is whether CMIP5 models can capture the observed trend of RH. Given the large inter-annual variability of RH, the derived

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trend may differ with the starting year. Shen points out that there are a lot of missing data in meteorological stations before 1973. Missing values could potentially lead to sampling biases and therefore biases in the trend. Shen argues that CMIP5 models can reproduce the trend between 1973 and 2016. To address this argument, I'd suggest the authors calculate the trends in RH between 1973 and 2016, and evaluate if CMIP5 models can capture the observed trend.

2. The second argument raised by the authors is that the good correlation between PC1 and PM2.5 derived from monthly data may not hold for other time scales. While I agree the correlation may vary with time scales, I don't think this analysis could really nullify the predictability of PC1. The correlation coefficient for annual mean is based on only eight data points, which is likely to be unstable. Qualitatively speaking, I could tell PC1 can capture most if not all the inter-annual variability of PM2.5. I tend to disagree with the statement that the yearly values are 'significantly smaller' than monthly values.

3. The authors pointed out that PC1 should not be used as a single proxy for PM2.5. Admittedly, a statistical proxy has uncertainties, but I don't think it's realistic to have a proxy that could perfectly simulate all the observed temporal variabilities. Shen et al. (2018) explain that their results differ from Cai et al. (2017) because Cai et al. (2017) does not include RH as a predictor, but such difference is not discussed in the comment. The different conclusions drawn from Shen et al. (2018), Cai et al. (2017) and Pendergrass et al. (2019) actually reflect the effect of climate change is uncertain and controversial. I don't think the conclusions of Shen et al. (2018) would be invalid just because of the inherent uncertainties of the chosen proxy.

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