Anonymous Referee #1: 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:

Response: We thank this referee for her/his perceptive comments. We appreciate that the referee found "the thoughtful debate on this controversial topic is worth publication at ACP." We have carefully considered all comments. Listed below are our point-by-point responses to individual comments (Referee's points in black, our responses in blue).

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 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.

Response: We agree that the key to this argument is that whether CMIP5 models can capture the observed trend of RH. Here, we calculate the trends in RH between 1960 and 2017 with data from China Meteorological Administration (CMA) rather than NCDC used in our original comment (acp-2019-193). The RH data from CMA, unlike data from NCDC, do not have the problem of missing values before 1973. The comparison is shown in Figures 1 and 2, which are in good agreement with the original figures. So, we have replaced those figures in the revised manuscript, but left the text essentially intact.

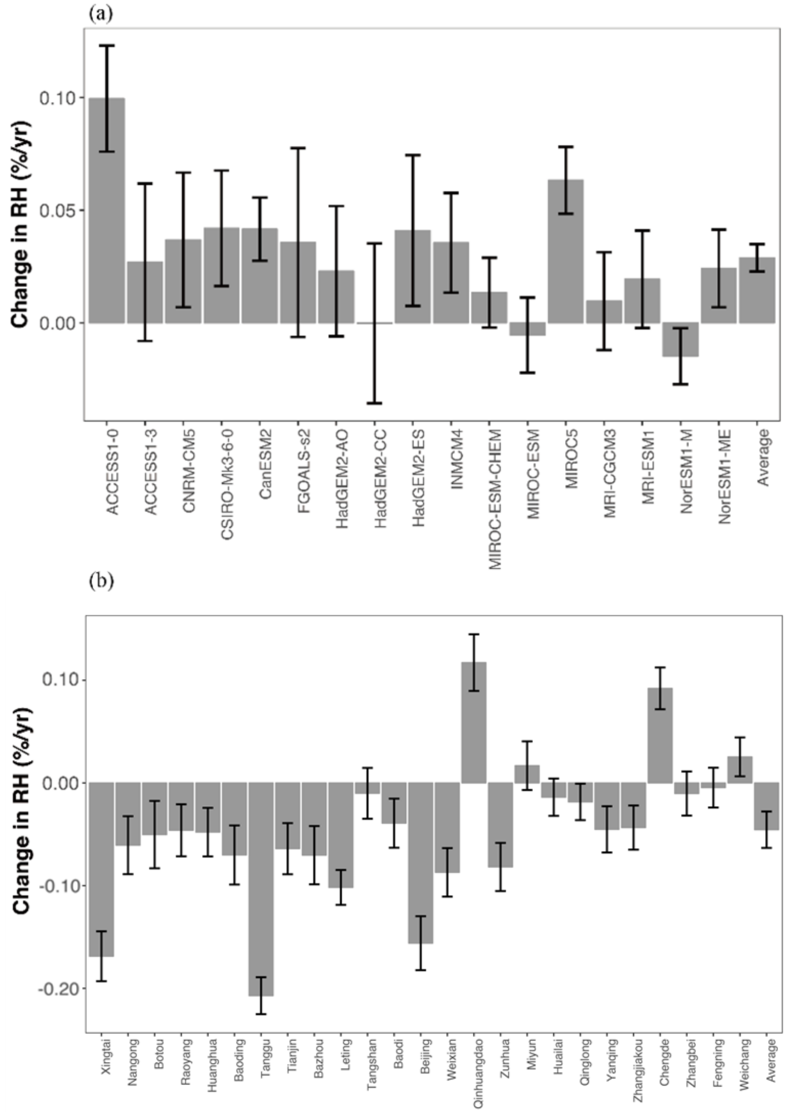

Figure 1: (a) Linear trends of wintertime average RH (in % per year) in Beijing-Tianjin-Hebei (BTH) calculated for 1960-2017 historical simulations by an ensemble of 17 CMIP5 climate models. (b) Same as (a) except derived from 25 meteorological stations of CMA in BTH region.

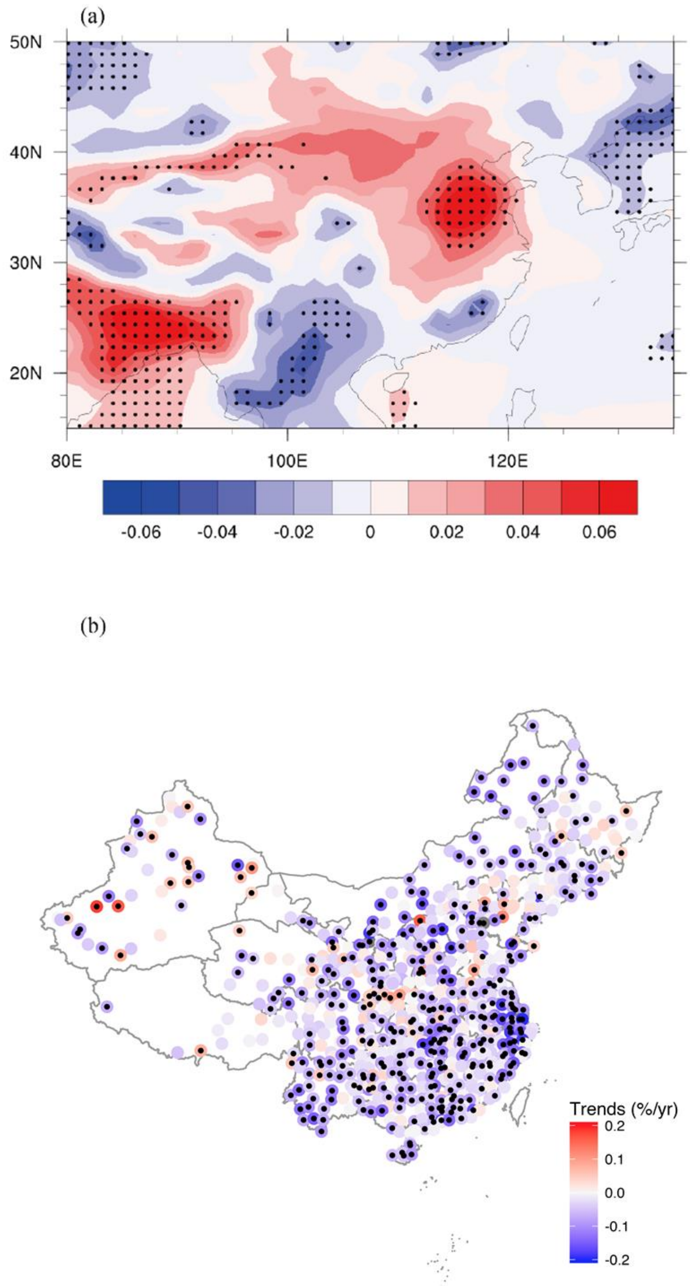

Figure 2: (a) Spatial distribution of linear trends of winter average RH (in % per year) in China calculated for 1960-2017 historical simulations by an ensemble of 17 CMIP5 climate models. (b) Same as (a) except derived from NCDC station data. Small black dots denote those trends significant at 95% confidence level.

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.

Response: In our first reply to the interactive comments, we pointed out the fact that, in addition to annual data, "the correlation coefficient of PC1 with $PM_{2.5}$ on daily basis (more data points than monthly values) is 0.68, which is significantly lower than the value of 0.9 used in the original paper." We made this point in the third section of the original manuscript. Therefore, the burden is on Shen and co-authors to prove that the correlation coefficient of PC1 with $PM_{2.5}$ stays high for the time scale of climate change, which is the time scale of concern for Shen et al. (2018).

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.

Response: We agree with the referee that the conclusions of Shen et al. (2018) would not be invalid just because of the inherent uncertainties of the chosen proxy. However, the conclusions of Shen et al. (2018) are invalid because of a fundamental point raised in our original manuscript: "a parameter such as PC1 should not be considered as a sole/exclusive/sufficient proxy of $PM_{2.5}$ just because PC1 has a good correlation with $PM_{2.5}$"; and a point raised in our second reply to the interactive comments: "even a perfect correlation coefficient (1.0) does not imply any causal relationship, let alone an exclusive/sufficient relationship". We have added the second point to the revised manuscript.

Comment on the paper “Insignificant effect of climate change on winter haze pollution in Beijing” by Shen et al. (2018)

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Abstract. The recent paper by Shen et al. (2018) (referred to hereafter as SHEN) made a sweeping statement on the winter haze pollution in Beijing by claiming “Insignificant effect of climate change on winter haze in Beijing”. We argue that the paper contains three serious flaws. Either one of the three flaws can nullify the claim of SHEN.

SHEN made a sweeping statement on the winter haze pollution in Beijing by claiming “Insignificant effect of climate change on winter haze in Beijing”. While failing to acknowledge the large differences in dataset used, analysis methodology, winter month selected, geographic region chosen, and period and time scale of study from the others, SHEN attempted to invalidate a number of previous studies, including Wang et al. (2015), Cai et al. (2017), Zou et al. (2017), and Li et al. (2018), which have suggested that climate change will worsen haze pollution in Beijing. In this context, our recent study (Mao et al., 2018) also suggested that global warming and other climate changes such as El Nino-Southern Oscillation (ENSO) and Pacific Decadal Oscillation (PDO) contributed significantly to the trend as well as interannual variabilities of winter haze days in eastern China.

We have found three critical flaws in SHEN. First, SHEN did not make any evaluation of the accuracies or uncertainties of the projected changes in surface relative humidity (RH) and meridional~~latitudinal~~ wind velocity at 850 hPa (V850) in the RCP8.5 scenarios calculated by an ensemble of 32 Coupled Model Intercomparison Project Phase 5 (CMIP5) climate models for the 21st century (2080-2099 vs. 2000-2019). Here we evaluate the accuracies and uncertainties of the projected changes in RH of CMIP5 climate models by comparing changes in RH~~ and V850~~ from historical simulations (1960-2017) of these climate models to observed values. Figure 1a shows the values of linear trends of annual average RH in Beijing-Tianjin-Hebei (BTH) calculated for 1960-2017 historical simulations by an ensemble of 17 CMIP5 climate models (Table 1). A few models show significant positive trends, but the average trend is only about 0.3% per decade. This small trend is consistent with the projected insignificant trends in 21st century (2080-2099 vs. 2000-2019) of RH in the RCP8.5 scenarios from an ensemble of 32 CMIP5 climate models as shown in Figure 5c of SHEN. In contrast, the small positive trend is in stark disagreement with the average trend of about -0.~~8~~5% per decade observed at ~~seven~~ 25 meteorological stations in BTH between ~~1973~~ 1960 and ~~2016~~ 2017 (Figure 1b). The disagreement is further illustrated in Figures 2a and 2b where the spatial distribution of trends of annual average RH in China calculated for 1960-2017 historical simulations by an ensemble of 17 CMIP5 climate models is compared to observed trends. The model trends are positive in the north and negative in southern China, while observed trends are ~~consistently~~ uniformly negative and greater in values. These disagreements raise serious doubt on the validity of projected changes in RH in Beijing for the RCP8.5 scenarios by an ensemble of 32 CMIP5 climate models. This result is not surprising because it is well known that climate models have large uncertainties and biases in local and regional projections of trends of meteorological parameters. ~~as~~ In fact, the evaluation of climate models during~~since~~ IPCC AR5 assessed median-and-above model performance only for the projected global average temperature trends (Flato et al., 2013).

Second, Figure 1d of SHEN showed time series of monthly average $PM_{2.5}$ and three meteorological parameters, i.e. RH, V850, and PC1. The correlations among $PM_{2.5}$, RH, V850, and PC1 are very good as reported in SHEN. However, most of the good correlation is contributed by the large monthly variations. Will the good correlation hold true for yearly variations, and more importantly, hold for the time scale of climate change, which is the time scale of concern for SHEN? In addition, will the ratios between $PM_{2.5}$ and the three meteorological parameters of longer time scales remain the same as those derived from monthly data? SHEN did not address these questions. Here we reproduce Figure 1d of SHEN in Figure 3a. Correlation coefficients of $PM_{2.5}$ with PC1, V850 and RH derived from Figure 3a are 0.90, 0.81 and 0.79 respectively, consistent with SHEN. In comparison, Figure 3b shows yearly average values of $PM_{2.5}$, PC1, V850 and RH; their corresponding correlation coefficients are 0.80, 0.66 and 0.46 respectively. These yearly values are significantly smaller than the monthly values, casting serious doubt on the applicability of results of monthly correlation to longer time scales. A further issue is that SHEN did not document what parameters were used in the principal component analysis and how PC1 was derived.

Third, a more fundamental question is that a parameter such as PC1 should not be considered as a sole/exclusive/sufficient proxy of $PM_{2.5}$ just because PC1 has a good correlation with $PM_{2.5}$. Even a perfect correlation coefficient (1.0) does not imply any causal relationship, let alone an exclusive/sufficient relationship. In other words, PC1, V850 or RH should not be used to exclude other proxies such as those suggested by Wang et al. (2015), Cai et al. (2017), Zou et al. (2017), and Li et al. (2018). The exclusiveness (or sufficient condition) of an index can only be established if a mechanistic model that uses the index as a sole proxy, can successfully reproduce the concentrations and trend of $PM_{2.5}$ quantitatively. SHEN did not develop such a model. For example, the variation of severe haze is associated with the daily variation of weather condition as shown in Cai et al. (2017) instead of the monthly PC1 given by SHEN. By using the same data as in SHEN, the correlation coefficient of PC1 with $PM_{2.5}$ on a daily basis is 0.68 (Figure 4b), which is significantly lower than the monthly value of 0.90 in Figure 4a, ~~.~~ ~~Comparing to the monthly value of 0.90 in Figure 4a, it again demonstrates~~ demonstrating that different correlation coefficients are found at different time scales again. Furthermore, the correlation coefficient of PC1 with $PM_{2.5}$ for severe haze days (days with daily mean $PM_{2.5}$ concentration $\geq$ 150 μg m^{-3}, as defined in Cai et al. (2017)) is a small value of 0.34 (Figure 4c). Therefore, it is inappropriate to use monthly PC1 to predict future severe winter haze pollution in Beijing as in SHEN.

Compared to the large uncertainties in regional RH from the climate models in SHEN, haze weather index (HWI) in Cai et al. (2017) is defined by anomalies in large-scale circulation with a 3-dminetional dynamical concept, which can be captured by climate models for the past and future (see Cai et al. (2017) for the justification).

Data availability

The CMIP5 model results provided by World Climate Research Programme CMIP5 (http://cmip-pcmdi.llnl.gov/cmip5/, last access: 31 January 2019) are available. The data of this paper are available upon request to S. Liu (shawliu@jnu.edu.cn).

Author contributions

RL, LM, and SL performed most the analysis. RL, SL and HL prepared the manuscript with contributions from all coauthors.

Competing interests

The authors declare that they have no conflict of interest.

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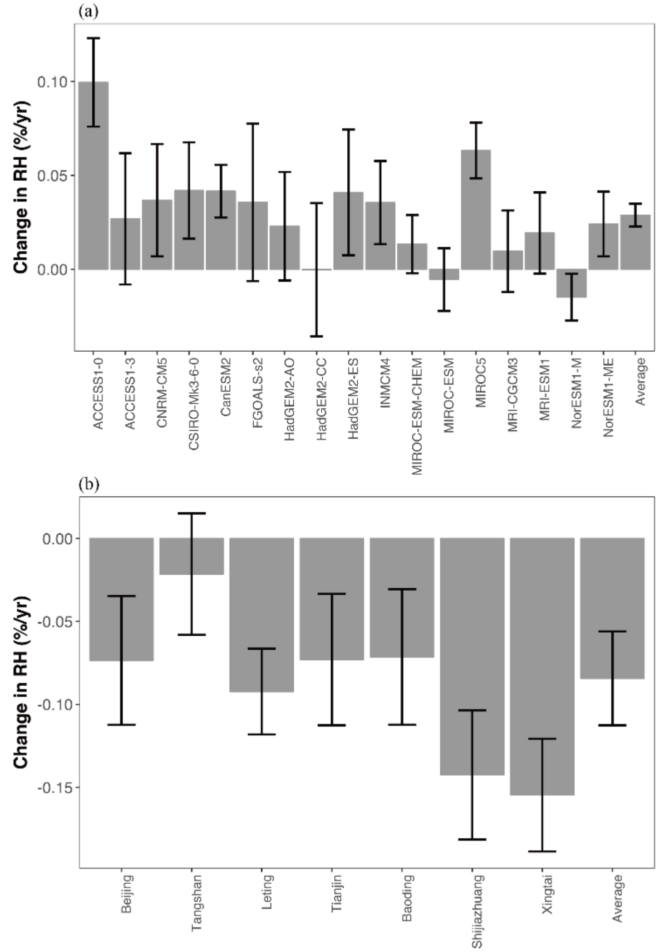
Tables

Table 1. Abbreviation and name of 17 CMIP5 models used in this study

Abbreviation	Expanded model name
ACCESS1-0	Commonwealth Scientific and Industrial Research Organisation Australian Community Climate and Earth System, version 1.0
ACCESS1-3	Commonwealth Scientific and Industrial Research Organisation Australian Community Climate and Earth System, version 1.3
CNRM-CM5	Centre National de Researches Météorologiques Coupled Global Climate Model, version 5
CSIRO-Mk3-6-0	Commonwealth Scientific and Industrial Research Organisation Mark, version 3.6.0
CanESM2	The second generation Canadian Earth System Model
FGOALS-S2	The Flexible Global Ocean-Atmosphere-Land System model, Spectral Version 2
HadGEM2-AO	Atmosphere and Ocean (non-Earth System version) configuration of HadGEM2
HadGEM2-CC	Hadley Global Environment Model 2 - Carbon Cycle
HadGEM2-ES	Hadley Global Environment Model 2 - Earth System
INMCM4	Institute of Numerical Mathematics Coupled Model, version 4.0
MIROC-ESM-CHEM	An atmospheric chemistry coupled version of MIROC-ESM
MIROC-ESM	Model for Interdisciplinary Research on Climate Earth System Model
MIROC5	Model for Interdisciplinary Research on Climate, version 5
MRI-CGCM3	Meteorological Research Institute Coupled Atmosphere–Ocean General Circulation Model, version 3
MRI-ESM1	Meteorological Research Institute-Earth System Model Version 1
NorESM1-M	Norwegian Earth System Model, version 1, intermediate resolution
NorESM1-ME	Norwegian Climate Centre Earth System Model ME

Figures


(a)
Change in RH (%/yr)
0.10
0.05
0.00
ACCESS1-0
ACCESS1-3
CNRM-CM5
CSIRO-Mk3-6-0
CanESM2
FGOALS-s2
HadGEM2-AO
HadGEM2-CC
HadGEM2-ES
INMCM4
MIROC-ESM-CHEM
MIROC-ESM
MIROC5
MRI-CGCM3
MRI-ESM1
NorESM1-M
NorESM1-ME
Average
(b)
Change in RH (%/yr)
0.00
-0.05
-0.10
-0.15
Beijing
Tangshan
Leting
Tianjin
Baoding
Shijiazhuang
Xingtai
Average

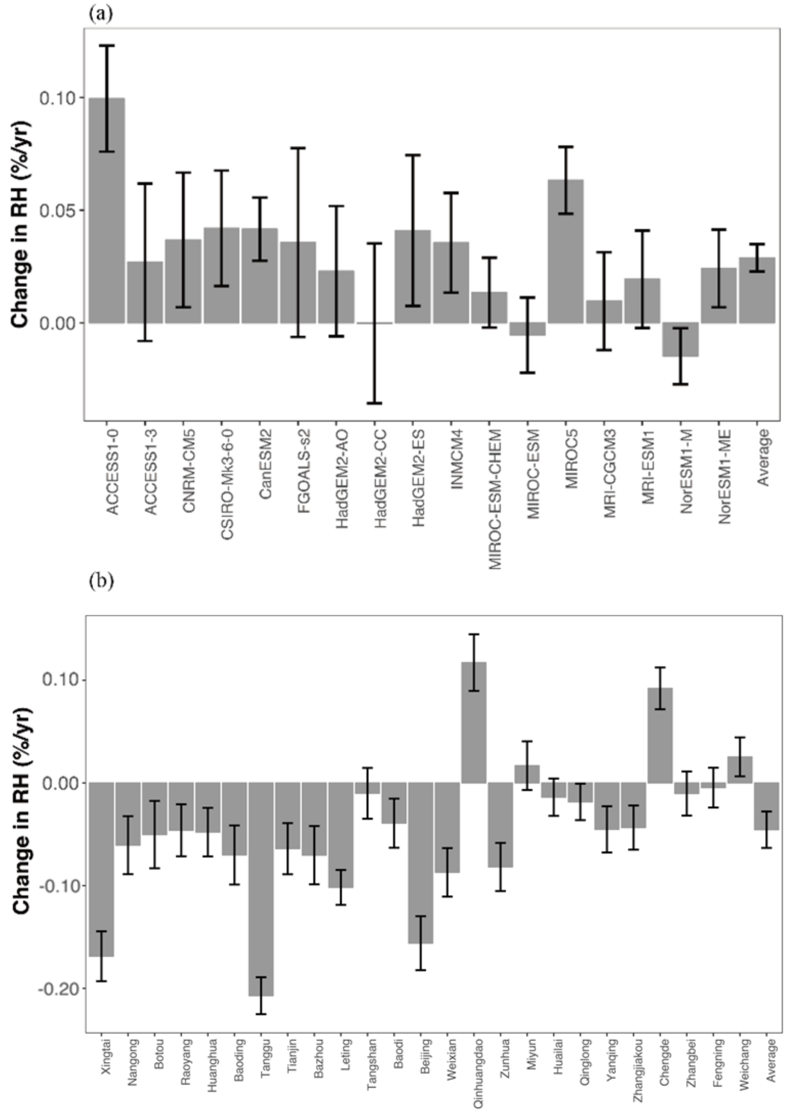
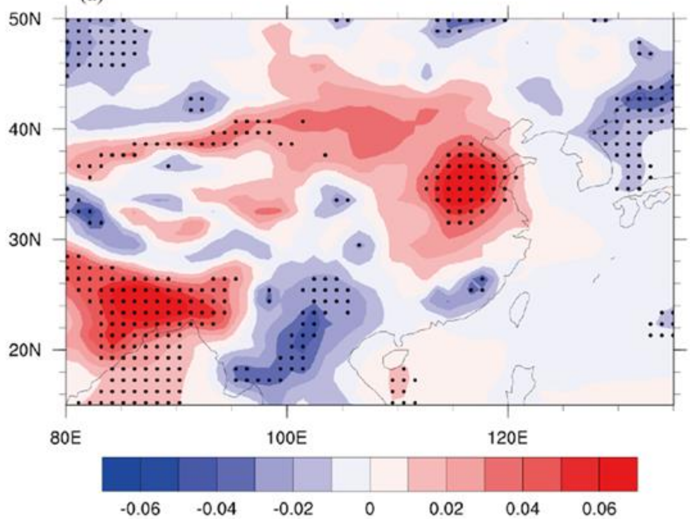


Figure 1: (a) Linear trends of annual average RH (in % per year) in Beijing-Tianjin-Hebei (BTH) calculated for 1960-2017 historical simulations by an ensemble of 17 CMIP5 climate models. (b) Same as (a) except derived from 25 CMA meteorological stations in BTH region.~~NCDC station data.~~

(a)



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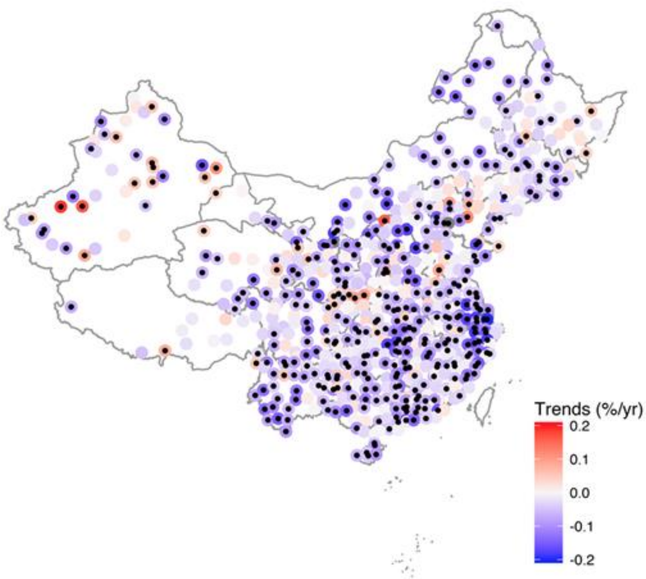

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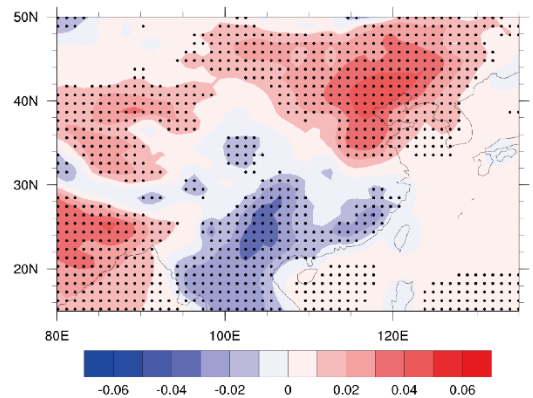

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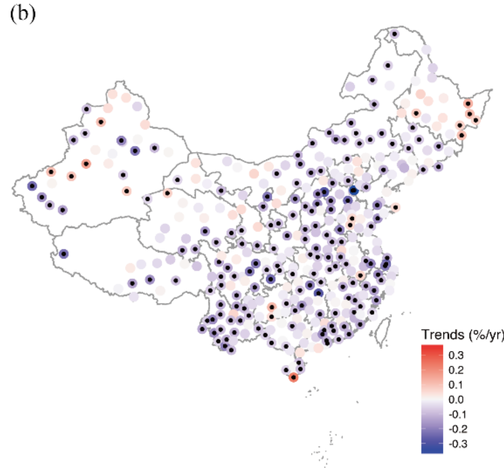

Figure 2: (a) Spatial distribution of linear trends of annual average RH (in % per year) in China calculated for 1960-2017 historical simulations by an ensemble of 17 CMIP5 climate models. (b) Same as (a) except derived from CMA meteorological stations. Small black dots denote those trends significant at 95% confidence level.~~NCDC station data.~~

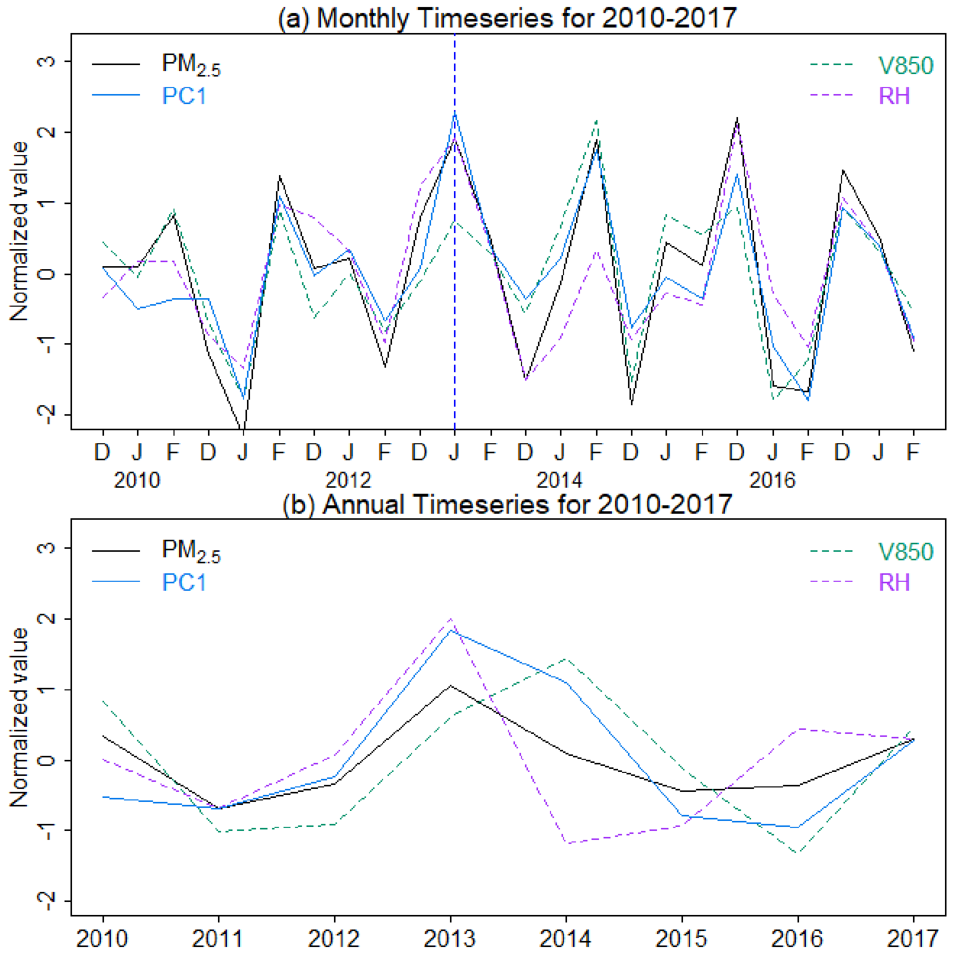

Figure 3: (a) Monthly mean time series for 2010–2017 of normalized PC1, $PM_{2.5}$, V850, and RH, the normalization is relative to the 2010–2017 means. (b) Same as (a) except for yearly means.

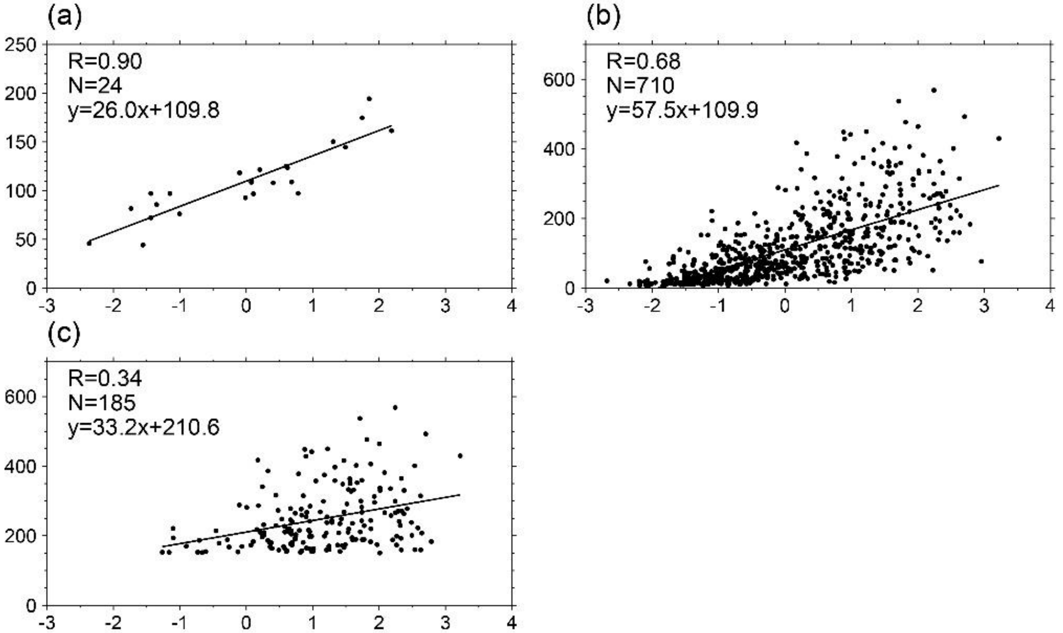

Figure 4: Correlations between PC1 (defined by V850 and RH in SHEN, horizontal axis) with observed wintertime $PM_{2.5}$ concentrations in Beijing (µg m^{-3}, vertical axis) for (a) monthly $PM_{2.5}$ concentrations and PC1, (b) daily $PM_{2.5}$ concentrations and PC1, and (c) daily $PM_{2.5}$ concentrations and PC1 for severe haze days (daily mean $PM_{2.5} \geq 150$ µg m^{-3}). In each panel, N is the number of samples in the studied time period of 2010-2017 as in SHEN, and *R* is the correlation coefficient.