

Earth Syst. Dynam. Discuss., referee comment RC2
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Comment on esd-2022-35

Anonymous Referee #2

Referee comment on "Regional dynamical and statistical downscaling temperature, humidity and windspeed for the Beijing region under stratospheric aerosol injection geoengineering" by Jun Wang et al., Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2022-35-RC2>, 2022

General Comments:

This paper looks at statistical and dynamical downscaling for the Beijing region under three future climate scenarios. It also appears to be the first time that dynamical downscaling has been used with a geoengineering scenario. The paper is fairly straightforward but seems to be a good starting point for future work on geoengineering modeling. Probably the biggest question I have is regarding the choice of analyzed variables. The authors compare temperature, humidity, and wind speeds under the different scenarios; temperature is of course important, but, at least to me, the latter two are not so much. I wonder if the authors thought about analyzing precipitation instead.

Another general question I have is: how is the ISIMIP spatial downscaling done? The two steps in Section 2.3 address bias correction and temporal downscaling, but how do you go from the coarse ESM grids to the ERA5 grid?

Finally, why is quantile delta mapping not done for the historical WRF simulations (in which case I note that it would simplify to the regular quantile mapping method)? Wouldn't it be fairer to compare ERA5 with bias-corrected ISIMIP/statistical downscaling and bias-corrected WRF/dynamical downscaling?

Specific Comments:

Abstract, line 20: It is not clear to me what "larger spatial ranges" means. I also do not see further reference to this in the main text. While I can see from Figure 9 that the range (maximum minus minimum) of temperature and of humidity is larger in WRF than ISIMIP, it's not obvious that this is the case for wind since although WRF has lower minima, ISIMIP has higher maxima.

pg 3, line 60: Give the full name of ISIMIP at least once (perhaps in the abstract as well).

pg 12 / Figure 4: Are the authors concerned about RH values exceeding 100%?

pg 14, line 277: ISIMIP seems to increase error and variance in wind speed in the case of HadGEM2-ES.

pg 16, line 309-310: Assuming I'm looking correctly, it doesn't look like WRF-QDM-MIROC and WRF-QDM-MIROC-CHEM are wetter than ERA5 from June to October. If anything, they look drier, especially the former.

pg 18, line 335-336: Wind speeds in ISIMIP look highest (as opposed to lowest) in the southeast of the domain.

pg 20, line 360: To me, only the G4-RCP8.5 anomalies for ISIMIP look anti-correlated. I might suggest removing this sentence.

pg 24, line 417: I think the authors meant to say that WRF relative humidity exhibits a dry bias in winter.

pg 25, line 429-430: This sentence is confusing as written, and the first part is not obvious since WRF-QDM-ESM shows opposite trends in roughly equal parts of the domain when comparing G4 and the 2010s. I might suggest removing this sentence.