

Earth Syst. Dynam. Discuss., referee comment RC1
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Comment on esd-2022-35

Anonymous Referee #1

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-RC1>, 2022

General comments

Wang et al. presents an interesting study about comparison between two downscaling approaches i.e. statistical downscaling (ISIMIP) and dynamical downscaling (WRF) combined with bias correction of the future projection of ESM scenarios (i.e. RCP4.5, RCP8.5, G4) over Beijing provincial region. The idea of comparing statistical with dynamical downscaling is a novel path of research. The authors focus on mean temperature, humidity, and wind speed, which are all relevant for climate impacts. The manuscript is generally clearly written and well structured, and the analysis is done in an organized way. However, some parts need to be clarified to easily understand some contents of the manuscript. I think the manuscript is scientifically sound and merits publication after some minor revisions based on my specific comments below.

Specific comments:

- In table 1, assessment on the performance of ISIMIP and WRF with bias correction is done only through RCP4.5 scenario. Is there any reason why RCP8.5 is excluded in this part?
- This research used ERA5 reanalysis data as the proxy of observation. The authors need strong justification of choosing ERA5 reanalysis in this research as reanalysis data itself might contain some degree of bias.
- Step1 of ISIMIP statistical downscaling and bias correction is simplified and make it unclear. Please provide a more detailed description of it.
- The statement "QDM is similar to QM but is non-stationary". In what sense it is non-stationary?
- From figure 3i we see that the WRF simulations have spatial patterns very different from ERA5. Moreover, figure 4 also shows that the pdf of WRF deviates from ERA5 significantly. Are there any reasons behind these findings?

- Authors use three different terms for ensemble i.e., “ensemble-mean”, “multi-ensemble mean”, “multi-model ensemble mean”. Are they referring to the same metric?
- In Figure 5, the caption states “labelled as ERA5”, but in the figure we cannot find the label “ERA5”. Do you mean the “observed” label? Figure 8 seems correct. Please make it consistent.
- It is stated that “wind speed of all four ESMs outputs have correlation coefficients <0.1 with ERA5”. I would suggest that authors describe in the discussion section the consequence of this low value to the results of the analysis.
- The statement “Humidity and windspeed anomalies from ISIMIP appear somewhat spatially anti-correlated, while for WRF there are no particular patterns” need to be clarified. What does the term “anti-correlated” mean? Is it a negative correlation?
- In Table 3, I would suggest using Asterisk sign to indicate that the differences are significant, instead of writing the number in bold.