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Comment on acp-2021-826

William Collins (Referee)

Referee comment on "Future projections of daily haze-conducive and clear weather conditions over the North China Plain using a perturbed parameter ensemble" by Shipra Jain et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-826-RC1>, 2021

Review of Jain et al.

This study addresses changes in their haze weather index HWI. Much more evidence is needed that this HWI will correlate with future pollution/visibility. As mentioned briefly in the conclusions the actual changes in visibility will depend on other factors, especially emissions of particulate matter. Hence to avoid misinterpretation by readers "hazy days" and "clear days" need to be replaced throughout by high and low HWI.

This study should include sufficient explanation for the HWI in this paper for the reader to understand the underlying principles without having to look into the earlier literature. It is not immediately obvious why these particular parameters should be associated with low haze, and why such specific regions are used to calculate the terms. Presumably it is due to typical synoptic patterns bringing air from a clean or polluted origin (it would be helpful to provide illustrations of typical synoptic patterns associated with high and low HWI). This explanation is particularly necessary because the variables are much less directly associated with pollution than typical stagnation indices which usually based on boundary layer height, surface wind speed etc.

It is also not at all obvious that the same correlations between variables and pollution/visibility will hold in a future climate – as one example any systematic changes in humidity or boundary layer height over NCP with climate change would strongly affect haziness but not appear in the HWI. This may have been addressed in the studies cited, if so the specific examples need to be referred to, if not then the authors need to demonstrate themselves that HWI is applicable in future climates.

A much stronger result could be obtained if any variables relating to pollution (aerosols or passive tracers) in the model were output. The authors should check what is available.

For the analysis in section 5 it is essential for the earlier section 3 to determine how much each of the variables contributes to the correlations with pollution/visibility. i.e. if V850 turns out to be the most important variable determining the correlation with pollution/visibility then the lack of future change in V850 would imply the haziness is unlikely to change in future.

Line 31: The sentence "The future frequencies of winter hazy and clear days in the PPE are largely driven by changes in zonal-mean mid-tropospheric winds and the vertical temperature gradient over the NCP" is very misleading. The study does not assess at all that future frequencies of hazy and clear days are driven by changes in zonal winds and vertical temperature gradients, rather it finds that those are the components of the HWI that change. There is no evidence presented of the effects on haze itself.

Lines 59-67: This paragraph describes important quantities (PBL height, stability, humidity) that are all key for haze formation but don't appear in the HWI. Much more evidence is needed that the particular variables in HWI are related to the quantities responsible for haze. References to literature on other stagnation indices (e.g. Horton et al. 2014, Vautard et al. 2018, Garrido-Perez 2021) needs to be discussed here.

Lines 68-91: More information needs to be given in this paragraph as to what measures of haze are used in these different studies. Do they all use HWI, or do some use a stagnation index, do some use modelled PM or visibility?

Section 2.1: More information is needed here on these observations. It seems that using the US embassy site could lead to extremely localised sensitivities to wind directions/conditions depending on where the embassy is situated in relation to the sources of pollution or even orientation with respect to street canyons in the neighbourhood. Figure 1 would be much more credible if it used a more spatially-averaged measure of PM_{2.5}.

Lines 252-258: This seems a strange comparison, there is no reason why the average PM_{2.5} in two different regions over two different time periods should be the same. It would be much more useful to show a correlation between Beijing and NCP to see whether Beijing is typical.

Figure 5: It would be useful here to look at the absolute frequencies as well as the change wrt historical. For instance there seems to be an outlier with very high changes starting in 2006-2032, but this might be explained if the historical frequency happened to be particularly low. These points all need error bars to determine whether the differences

between members are explained by internal variability. I couldn't see any of the pick/red circles that "suggests a decrease in daily haze frequency".

Line 347-349: I didn't quite follow how the shift in HWI,U500 and dT were "consistent" when different PPE members show large difference in HWI in figure 5.

Lines 437-438: This sentence isn't clear. The different PPE members certainly show different trends, but is the argument that these differences in trends can be explained by internal variability? The outlier member in figure 5 seems to suggest that for this particular set of parameters the trend is significantly higher than the others.

Figure 10. Why does the 2060-2086 circle in panel (c) show a negative trend in frequency when figure 5 shows all the trends are positive?

Lines 465-466. This sentence of the climate change impact only being discernible for specific periods needs to be explained more fully. If it is true that an increase in HWI with climate is not a general feature then that disagrees with the claims of a "clear impact of anthropogenic climate change on future trends".