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Interactive comment on "Seasonal Prediction of Winter Haze Days in the North-Central North China Plain" *by* Zhicong Yin and Huijun Wang

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General comments:

By using the year-to-year increment as the predictands, the authors established a statistical model to predict the winter haze pollution over the North-Central North China Plain (NCP), in which seven predictors are selected and two schemes are employed. Cross validation shows that such model can successfully capture the interannual and interdecadal variabilities of winter haze days over the NCP and the extremums as well. The model based on the new approach of year-to-year increment is very skillful for the seasonal prediction of winter haze days in the NCP, and has greatly potential applications to the environmental pollutions. However, more discussions are needed for the predictor selections and their possible physical processes in the successful seasonal prediction of winter haze days, so that the readers can better understand and apply this seasonal prediction model. Minor revision is required before it is accepted

for publication.

Several major issues need to be addressed:

1. Line 45: "climate change" usually indicates a long-term climate variation, but DY is more like interannual variability, so it's not appropriate to use the phrase "climate change" here.

2. Line 73-74 and Line 77: Why are the MLR and GLM called model-driven and data-driven methods, respectively? A brief description is highly encouraged.

3. Line 87, Line 97, Line 103 and many others: it should be noted that Pacific Japan (PJ) pattern is a summer teleconnection identified by Nitta (1987), not a winter one.

4. Line 91-92: Why is the water vapor transportation enhanced?

5. Line 95: Region of the Japan Sea to the Stanovoy Range is chosen for the pre-autumn TS DY, however, from Figure 2, we can see the region from the Japan Sea to the south of Lake Baikal has larger correlation coefficient. Maybe it is better to use this region for predictor x1.

6. Line 97: As defined by Wallace and Gutzler (1981), EU pattern has three major nodes with their locations at (55N, 20E), (55N, 75E) and (40N, 145E). However, Figure 3 doesn't cover the whole area of EU pattern, so we cannot obviously see the negative EU features from Figure 3. Besides, Figure 3 has rather different features from Figure 1 except for the anticyclone anomalies over the South Japan.

7. Line 103, Line 107: As for the PJ and negative EU pattern, they are not very clearly and significantly seen.

8. Line 110-113, Line 128-129: The correlation coefficients near the NCP are almost insignificant, how can the predictors x3 and x6 be so important?

9. Line 143: Figure 16 is a little complicated, it's better to explain it briefly.

10. Line 154: How to define the predicted bias?

11. Line 180: The center of positive geopotential anomalies is actually located over the Japan Sea.

12. Line 182-184: The consistence of SPMMLR and SPMGAM indicates that the linear part dominates the WHDNCP predictions. At the same time, the failure of predicting the rapid rising trend after 2010 also implies that the DY method has some deficiencies in dealing with the large abrupt change. The authors should point them out.

Technical corrections:

13. Line 25-26: The sentence is a little awkward, and it needs modification.

14. Line 38: The citation of Huang et al. 2015 is not present in the reference list.

15. Line 48: The citation of Huang et al. 2014 is not present in the reference list.

16. Line 91: “Asia” should be “Asian”.

17. Line 105: As for the “Prior studies”, some citations should be given.

18. Line 118-119: it needs to make clear what this sentence is talking about, geopotential height?

19. Line 119: “EAJS” should be “EASJ”.

20. Line 160: “processing” should be “process”.

21. Line 165: “results that are” can be changed to “and the results are”.

22. Line 171: “simulative” to “simulated”.

23. Keep the tense consistent in the whole paper.

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