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Interactive comment on "The Effects of El Niño-South Oscillation on the Winter Haze Pollution of China" *by* Shuyun Zhao et al.

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In this manuscript, the authors found observational evidence about the regional dependency of the relationship between winter haze days of China and ENSO SST, and tried to verify the mechanism about the effect of ENSO on the winter haze pollution of China based on the composite analysis for El Nino cases and La Nina cases which are simulated by using the aerosol-climate coupled model BCC_AGCM2.0_CUACE/Aero of the NCC, focusing both transport effect and scavenging effect which are related to the atmospheric circulation change and associated precipitation change induced by different SST forcing. The methodology is sound and the paper appears to be relatively well-organized. I believe that the paper provides a further insight into the regional dependency of the ENSO effect on haze pollution of China. However in this version

there are some scientific comments to be clarified.

Specific comments

1. Significant test: the most important procedure in composite analysis is to provide readers with the significant test for the difference in target variables between two groups, at least 5% significant level. So, the authors should add the significant level for every difference field. The simulated changes caused by El Nino and La Nina forcing relative to climatology are not always symmetric each other. So, it is very difficult to understand the difference between El Nino effect and La Nina effect. In my opinion the other approach to stress the effect of El Nino on the winter haze is to see the difference between two extremes (El Nino and La Nino). In this way, there will be a clear difference by offsetting the same sign and magnitude. Anyway the authors should revise the manuscript based on the significant test. In addition the authors should add some discussions about the dynamic link between circulation change and ENSO forcing through telelconnection pathway based on the significant test.

2. Filtering of the monthly haze days: In Fig.4 the authors calculated the correlation coefficients between the winter-average monthly haze days and SST in winter, using data after applying a linear-trend removing and 2∼8 years' band-pass filtering. Basically 2-8 years' band-pass filter also eliminates a linear trend because periods less than 2 years and over 8 years including a linear trend are removed. So, using both filters is redundant. Applying a linear-trend removing at first is obviously not valid because time series is not linear. Moreover sudden jump in last year can make linear trend distorted toward large trend. Thus the author should clarify this point. If it is so, the authors should re-calculate the correlation coefficients in Fig. 4 by considering the point suggested above.

3. Haze days and surface aerosol concentration in Southern China: Is less haze days in observation during El Nino winter shown in Fig. 3c matched with less surface aerosol concentration in model during El Nino winter shown in Fig. 9a? If it is true, why are

surface aerosol concentrations over southern China decreased in both El Nino and La Nina winter? Please clarify this point. In addition, the authors should check whether Fig 9a and b are consistent with Fig. 12 over southern China. In my opinion, two pdf curves for El Nino and La Nina should be located below CLI curve on average because changes in both Fig. 9a and b are negative over southern China. Are pdf results dependent on the definition of southern China?

Technical corrections

1. Line 9 page 8: “the weakness of EAWM” should be “the intensification of EAWM” because Siberian high is stronger than normal.

2. Line 5-8 page 9: Over northeastern China winter CONCsur of aerosols are decreased in both El Nino and La Nina, indicating that the decrease over northeastern China is not irrelevant to ENSO. So the authors should describe this point carefully.

3. Line 1 page 24: “zonal flux” should be “horizontal flux”.

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