

## ***Interactive comment on “Response of Early Winter Haze Days in the North China Plain to Autumn Beaufort Sea Ice” by Zhicong Yin et al.***

**Anonymous Referee #2**

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General comments: Haze pollution is for the time being a serious problem for China. The prediction of haze pollution is highly-relevant to the society. The manuscript explored the linkage between the number of haze days in China and the change in autumn sea ice extent in the Beaufort Sea and analyze the potential mechanism. I find the manuscript is scientifically interesting and fits the scope of ACP. However, major revision is needed before it can be accepted for publication in ACP.

Major comment: 1. the cause and effect are not convincing in this manuscript. There are quite some places authors used ‘induced’. Correlation/regression can not tell what is cause and what is the effect. 2. Why can not directly link the SST anomalies in the Bering Sea and the number of haze days over NCP? Detailed comments: 1. What is the difference between ‘Arctic region’ (Line 23) and ‘Arctic area’ (Line 25)? 2. Line 23, I

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do not understand why authors highlighted ‘February 2018’ since no data from 2018 is used in the manuscript. 3. Line 24, Does the authors mean the Arctic amplification intensified only during past few years? 4. Line 26, What the authors mean by ‘Recently’? 5. Line 26, ‘Arctic sea ice decreases rapidly since the satellite era, in particular, after year 2000’. 6. Line 27, ‘the change of ASI’ 7. Line 30, remove ‘variability’ 8. Line 30, sea ice is a component in climate system, is not an external driver 9. Line 40, it is better if the authors can provide a brief definitions for dust, sandstorm and haze. 10. Line 45, ‘long-term trend of haze’ is not clear. Long-term trend of number of haze days, or intensity of haze, or periods of haze? 11. Line 46, the same as above ‘the trend of haze pollution’ 12. Line 50, the same as above, ‘correlation with the haze’ 13. Line 51, the same as above, ‘different variations in haze days’ 14. Line 52, ‘between the autumn sea ice cover in Beaufort Sea and the number of haze days in winter’ 15. Line 54. ‘number of haze days varied differently during early (December-January) and late (February) winters’ 16. Line 54, ‘suggesting a potential different driving mechanism’ 17. Line 56, similar as No. 14 18. Line 80, ‘The HDJ was stable during 1979 to 2012 and decreased during 1993 to 2009’ 19. Line 80, ‘The HDJ showed a strong upward trend after 2009’ 20. Line 85, ‘what is the threshold for pollution in China’? 21. Line 84, what is the meaning of ‘synoptic process of haze were weaker’? How to judge this? 22. Line 90, there are number of places the authors used haze days. I believe that authors mean ‘number of haze days’. 23. Line 97, the correlation can not tell which causes which. 24. Line 101, ‘correspond’ instead of ‘induce’. Again, correlation can not tell which causes which 25. Line 102, can authors perform sliding correlation to indicate the enhanced connection after mid-1990? 26. Line 103, ‘response’ is not accurate here 27. Line 108, SST and sea ice concentration in general co-varies. Correlation can not tell which causes which. Authors can also check the surface heat flux. 28. Line 108, ‘induced’ is not correct here. 29. Line 109, Do authors have any idea why negative SST anomalies disappear in November? 30. Line 110, correlation can not tell ‘change of BS sea ice’ can lead to SST anomalies over the BS and GA. ‘induced’ is not correct here. 31. Line 118, why authors can not directly link the SST anomalies over BS and

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GA to HDJ? 32. Line 123, how authors can conclude the change in atmosphere circulation is a response to change in sea ice by correlation? 33. Line 125, 'induced' again 34. After line 125, authrs used correlation to conclude the sea ice change-leading to atmosphere change-leading to SST change in number of places of the manuscript.

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