



EGUsphere, referee comment RC2
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Comment on egusphere-2022-780

Anonymous Referee #2

Referee comment on "Superimposed effects of typical local circulations driven by mountainous topography and aerosol–radiation interaction on heavy haze in the Beijing–Tianjin–Hebei central and southern plains in winter" by Yue Peng et al.,
EGUsphere, <https://doi.org/10.5194/egusphere-2022-780-RC2>, 2023

General comments:

The authors of the manuscript entitled: "Superimposed effects of typical local circulations driven by mountainous topography and aerosol–radiation interaction on heavy haze in the Beijing–Tianjin–Hebei central and southern plains in winter", try to investigate the link between aerosol, local vertical circulation, and heavy haze pollution in the Beijing–Tianjin–Hebei plain in winter, by implementing the atmospheric chemistry model GRAPES_Meso5.1/CUACE in January 2017. In my opinion this is an interesting manuscript suitable for Atmospheric Chemistry and Physics journal, however some important issues need to be addressed before it can be further considered for possible publication.

Specific comments:

Lines 20-21: This phrase is not clear. Please revise.

Line 33: Please define PM_{2.5} based on their diameter.

Line 43: The expression "high concentration aerosols" is not clear. Please revise

Line 89: Why was this time period selected? This must be explained.

Figure 1: The abbreviation OBS is not defined in the legend. Please revise.

Line 127: Please define the type of these correlation coefficients.

Lines 128-129: The difference of the correlation coefficients (0.03) between the two numerical scenarios (CTL and EXP) is really low to support this statement. Please revise.

Lines 131-134: The justification for the selection of the three studied pollution periods (January 5–7, 16–18, and 23–26) is not adequate. Other extreme pollution periods are also indicated in Figure 2. In addition, the selection of January 6, 17, and 24 as the representatives of the three pollution periods is also not adequately justified. This is an important issue because biased results can be implied. Please include more convincing explanations. Moreover, in my opinion the authors should expand the implementation of their method in other time periods besides January 2017 to enhance their manuscript.

Figure 4: The quality of these images is very low and has to be improved. In addition, some elements are not defined the legend. More specifically: a) which images correspond at 500 hPa and which at 700 hPa? b) the red square indicates the BTH region?

Figure 5: A red square is also needed here to indicate the BTH region. Please add it.

Lines 231-232: This sentence is incomplete

Lines 238-242: This section is not clear especially for January 24. Please revise.

Line 248: The enhancement of northerly winds in the lower level on January 24 is not clear in Figure 9c. Please revise.

Line 253: A reference to Figure 10 (b, e) would be useful here

Figure 9: (g–i) is missing from the legend. Please include it.