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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Contributions of meteorology and emission to the 2015 winter severe haze pollution episodes in Northern China" *by* Ting Ting Liu et al.

Anonymous Referee #2

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Summary:

This study uses a combined observation and modeling analysis approach to investigate the possible impact of meteorological conditions on surface air quality in Northern China. Since 2013, China has implemented stricter controls on emission sources to improve air quality, however observations of PM2.5 in early winter 2015 were higher than those observed the same time the year before. From the analysis of the observations and a simple model study were emissions were held constant, the authors conclude that changes in meteorological conditions are the main driver of the higher PM2.5 in December 2015 compared to December 2014.

However, I find this study lacks robust methods to come to such a bold conclusion.

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The methods for analyzing the meteorological observations are unusual and it is unclear how the conclusions were drawn from the analysis. The hypothesis is interesting and worth investigating, therefore I recommended the major revisions and additional analysis laid out below before this paper can be accepted for publication.

Major Comments:

This study would benefit from putting the 2014 and 2015 observations in perspective with pre-2014 observations. Figure 1 would benefit from additional monthly averages showing a climatology for the period pre-2014 with error bars or shading indicating the standard deviation to see which, if any, of the observations in 2014 and 2015 have actually improved since the stricter regulations were put in place. Without evidence of how 2014 and 2015 compare to prior to the emission reductions, the statements that emission reductions implemented in the area was effective in reducing major pollutant concentrations on Pg 7 lines 2-8 cannot be stated so categorically.

On Pg 2 line 18, the authors state “a steady decrease in major air pollutants was observed compared to 2014”. Again on Pg 4 line 15, “the annual mean concentrations of PM2.5 are overall in a decreasing trend”. Are the authors referring to Figure 1 monthly values from January to September? I agree that overall the monthly values in 2015 are lower than the corresponding monthly values in 2014, however the pollution levels in spring months are about the same values in the two years. If the authors refer to a trend then description of linear fit should be included.

On page 4 line 20, the authors suggest removing November and December of 2015 from the analysis to show that the difference between the annual means would increase at all four stations (note, Pg 5 line 2 only references December.). Did the authors also remove November and December 2014 from the analysis? This argument ignores the seasonal cycle since the monthly PM values in the January and February 2015 in Baoding and Shijiazhuang were similar to the values of November and December 2015. There is a lack of discussion of the seasonal cycle of the air pollutants shown

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in Figure 1. Is the decrease in PM2.5 part of the seasonal cycle? Is there a reason that PM2.5 peaked in October in Beijing, Langfang, and Shijiazhuang in 2014 and not in Baoding and not at any of the stations in 2015? I suggest expanding on the details introduced on Pg 3 Lines 1-3. In particular, what are the meteorological conditions – such as humidity, clouds or sun light, temperature, and wind – and how do they impact haze pollution? (could introduce sentence Pg 10 lines 10-12 earlier).

Pg 3 lines 13-17: Can the authors describe how El Nino and La Nina seasons usually impact the meteorological conditions in Northern China. Stating "Unusual climate and extreme weather happened everywhere" does not tell the reader how extreme or unusual the weather can be in Northern China and how the weather conditions can lead to more or less haze events. This also needs to be addressed in the conclusion (Pg 14 line 8-9), the authors have not shown the synoptic conditions are characteristic of an El Nino phase by comparing to previous El Nino winters. Also, the authors have not shown temperature therefore how can they state 2015 was a warm winter.

In the modeling section, on Pg 11 line 17, the authors state the comparison was made for November and December, however the figure captions state difference between December only. Is this correct, or are the figures November and December composites? And if the Figures are for December only, why did the authors not include November in the modeling results? Can the authors provide more details in set up of the model. Was the model run for the full two years of 2014 and 2015? Was there any spin up necessary? Is the emission data from pre-2014? Are meteorological observations assimilated in the GRAPES_meso? If so, please state what kind of assimilation is used.

The modeling section would again be more robust if a comparison was made to 2013, a year before stricter legislation, or possibly an ensemble of years to see if the differences are statistically significant and not just noise in the model results. Alternatively, a comparison of model runs where the emissions did change (if emission data is available for 2014 and 2015) to show how much change is related to emission changes and how much was related to meteorological variability.

The following list improvements that should be made to the individual figures:

In Figure 2, it is not clear which values belong to Langfang and Tianjin. The values in Figure 2 do not match up with the PM2.5 values in Table 1. For example, Beijing monthly mean PM2.5 in Dec. 2015 in Figure 1 is 154 while in Table 1 it is 151.

In Figure 3, the wind speed convergence lines are shown over the top of weather maps which indicate meteorological conditions at stations at some point in time. Are these conditions monthly averages or instantaneous observations? Also, I am not familiar with the use of wind speed convergence lines to indicate stable atmospheric conditions and it is unclear as to what the blue arrows on Figure 3a and 3b are and why they vary in length. I have searched for other papers that have used this method, however I was unable to find a reference in papers or meteorological textbooks where it did not refer to convergence along a front or surface low pressure system. More traditional method would be to composite mean sea level pressure and winds. A quick look using NCEP reanalysis data (www.esrl.noaa.gov/psd/cgi-bin/data/composites/printpage.pl) shows areas of high pressure over Northern China and low pressure off to the northeast for November and December for both 2014 and 2015 where the extent and location of the pressure systems shift from year to year. Therefore, I can understand that a shift in high and low pressure systems would impact the accumulation of pollutants, changes in wind speeds and directions and changes to relative humidity but it is not clear to me how "pollutants easily accumulate along the lines" (Pg 8 line 4) or how RH changes with shifts of the WSCL (Pg 10 lines 8-10). Can you provide more details on the methods to calculate the WSCL, how it may relate to pressure and frontal systems, and possible references where this method has been used before?

The WSCL lines of Figure 3 are referred to in the discussion of Figures 4, 6, and 7. Specifically Pg 8 lines 7-8, Pg 10 line 8-14, Pg 12 line 6. Also, the changes at the specific four cities are referenced from the figures. It would make the concluding statements from these three figures "clear" if the WSCL lines were added to the figures as well as possibly color coded open circles around the four city locations to make it

easier to identify which dot belongs to which city. From my attempt to draw the line and identify the cities on Figure 6, I do not understand how the shift in the WSCL impacted RH at SJZ more than BJ (Pg 10 line 13). It would also improve Figure 3 if the latitude and longitude axis ticks to match Figures 4 and 6 were included.

Figure 5 caption states that the winds are SWF winds yet the figure shows winds up to 7 m s-1. The authors need to more clearly separate the discussion of SWF on Pg 9 lines 7-14 from discussion of winds between 1 to 7 m s-1, which can possibly impact the transport of pollution in and out of the region.

Figures 7 and 8 only have one longitude and one latitude labeled. The figures would benefit from more regular axis ticks. It is not clear in the text why the two figures would not be for the same region; Figure 8 is shifted slightly or possibly zoomed in, which would be easier to see if the figures had more axis ticks. The titles on Figures 7 and 8 are not informative and should be changed or removed. The key title is not in English. On page 12 line 14 the authors state a range of 40-180%, however Figure 8 key shows percent differences from 0 to 1.8%. Either the legend or figure caption or the text on Pg 12 needs to be changed.

Minor Comments:

Pg 1 line 22: spell out versus and change meteorology to meteorological

Pg 2 line 2: remove “an” before elevated

Pg 2 line 5: change to read “pollution problem in the cities of Northern China”

Pg 2 line 6: change “and has a” to “due to the”

Pg 2 line 9: From this work do you expect to provide more effective control measures to the government? If not, stop the sentence after “China”.

Pg 2 line 10: Can you expand in more detail and include references on the strict control measures that were enforced in 2013. Was there any control measures of air pollutants

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in China before? If so, change “strict” to “stricter”.

Pg 2 lines 14-16: This final sentence sounds like you have a result you want to prove (“but largely should attribute to the control measures taken”) instead of a hypothesis you are looking to test in this paper. It would be better if the sentence identified that both changes in emission sources and changes in meteorological conditions could impact the changes in PM2.5 observed at these stations.

Pg 2 line 17: remove “situation”

Pg 2 line 20: What is the time resolution on the PM2.5 concentration that reached 1000 μg m-3. Is it an hourly maximum or a daily average?

Pg 3 line 7: “where the scientific control measures can be formulated”. Do the authors mean where the scientific control measure can be implemented?

Pg 3 lines 9: Add “used” prior to “to analyze the contributions from”

Pg 3 line 16: Remove “et al.” and add first name of Zhao, unless there were more than one person in the personal communication. I am surprised there are no references available which discuss the conditions for haze pollution that can be linked to the meteorological conditions associated with ENSO.

Pg 4 line 4-5. What are the strict software engineering requirements?

Pg 4 line 13-14: PM10 and O3 are not referred to in this study.

Pg 4 line 17: reference Figure 1.

Pg 5: Figure 1 could be improved by switching the colors such that the bolder color indicates 2015.

Pg 5: There is a missing μcharacter in the units in the y-axis in each subplot as well as another missing character referring to the percent difference in each subplot in Figure 1.

Pg 5: The dashed lines are missing from the legend in each subplot in Figure 1 and a description of the dashed lines should be included in the Figure 1 caption. There is no reference to the dashed lines in Section 2. The authors should clarify if the dashed lines are an average for the full year or the dashed lines represent a three month average (Oct-Dec) for each year?

Pg 5 line 12: Similar increases were seen for the other observed pollutants except for SO2 which decreased. Can the authors explain why this would happen? Did the authors expect SO2 to decrease?

Pg 6 lines 7-8: The Table 1 caption should explicitly state that the values are monthly mean PM2.5 concentrations for December 2014 and 2015 with the percent difference (Diff (%)).

Pg 7 line 10: What is the time resolution of the surface meteorological data?

Pg 7 line 13: change “northerly” to “northward”. Also, “southern” Hebei and “central” Hebei are not capitalized but “North Hebei” is.

Pg 8 line 2: change “northerly” to “northward”. There are further instances where northerly is used, however it is confusing since in meteorology a northerly wind is southward flow. Consider changing in all cases to use “-ward”.

Pg 8 lines 10-11, 13: changes to sentences to clarify that Table 2 does not show trends but a difference in static wind frequency: “unusually high”; “the other cities all experience an increased frequency of stable weather”; and “decreasing trend for SWF” to “decreased frequency for SWF”.

Pg 8 lines 13-14: Can you clarify what you mean by “Shijiazhuang had a similar WF with other cities with more than half of the days under static stable environment”?

Pg 8 line 15: Is this result indicative of coastal conditions having less SWF?

Pg 8 line 18: Have you considered adding a percent difference to Table 2 since you

reference it in the text on line 12.

Pg 9 line 7: “2105” change to “2015”. Also, Figure 5 top panel has “2104” instead of “2014”.

Pg 9 line 14: remove “was even”

Pg 10 lines 3-5: Remove “(SWF)” since windspeeds over 1 m s-1 are included on the figure. Add (top) and (bottom) after Beijing and Shijiazhuang, respectively.

Pg 10 line 14: change sentence to start with “A previous study by Chang et al. (2009) has shown that . . .”

Pg 10 line 14 to Pg 11 line 7: The authors need to more clearly relate sulfate and nitrate to PM2.5 to strengthen their argument. Also, I suggest these sentences are separated into a new paragraph.

Pg 13 line 6: change “of the un-favorite” to “the unfavorable”

Pg 14 line 7-8: switch order of percentages from low to high, 17% to 163%

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