

Interactive comment on “Assessing the formation and evolution mechanisms of severe haze pollution in Beijing–Tianjin–Hebei region by using process analysis” by Lei Chen et al.

Anonymous Referee #2

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Review of: “Assessing the formation and evolution mechanisms of severe haze pollution in Beijing–Tianjin–Hebei region by using process analysis” by Chen et al.

The authors quantitatively examined the cause of a severe haze event over Beijing–Tianjin–Hebei (BTH) through decomposing contributions from emissions, physical and chemical processes, using the WRF–Chem model equipped with an improved integrated process rate (IPR) analysis scheme. The IPR scheme also reveals the dominant role of aerosol radiative effects in haze formation is physical rather than chemical. Such an IPR scheme merits application in future relevant studies. Overall, the manuscript is well structured and should be of great interest to ACP readers. I recommend it is

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publishable after my minor comments/suggestions are addressed.

General comments: -Local vs. regional contributions. From the sensitivity simulations, local emissions and regional transport account for 80% of total PM_{2.5}. What does the contribution from “others” (Fig.7) mean? is it because the non-linear chemical formation for secondary aerosol or contribution from aerosol precursors outside of BTH?

-Advection contribution. A negative value for advection is diagnosed by the IPR scheme. From my understanding the advection means horizontal transport, which should not be always a negative contribution, and instead it may contribute a lot to PM_{2.5} if taking the value of regional contribution as an equivalent.

-Aerosol radiative effects. It is considerate to include the aerosol indirect effect, though this process contributes marginally in less-cloud wintertime. But the authors failed to show/discuss how aerosol indirect effect is expressed from IPR result. For example, in Fig. 11 the CLDC (1.5 $\mu\text{g m}^{-3}$) and WETP (1.2 $\mu\text{g m}^{-3}$) can be taken as the result of aerosol indirect effect. It needs to clarify.

Specific comments:

-The presentation of this work would be greater if some editorial aspects are improved.

Use of %: P1L27. Here and elsewhere in the text, please round off 250% instead of 250.0%. The decimal doesn't make sense in terms of model bias.

Please check the appropriate usage of hyphen (-) (e.g., near-surface) and en-dash (–) (e.g., December 20–22).

Fig.1: The purple dot (?) for PBLH is hardly to see. Reduce the size of green triangle or increase its transparency.

Fig.4: I suggest the use of NMB and correlation coefficient are good enough for model evaluation. Reader gets lost in so many numbers.

Fig.6: I can't see any different for (k) and (i), and there is also no discussion in the text.

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Remove one of them.

Fig.8: Move the middle panel (b1 and b2) towards right. Is there any difference in Y-axis of (b1) and (b2). I suppose they are the same.

-P1L24: there is any special meaning for “absolute” PM2.5. If not, please “absolute” when it is unnecessary.

-P2L6 & P5L8: remove “and so on”.

-P3L7: I don't think severe haze frequently occurs in wintertime over PRD region, and neither of your two references support this.

-P310-12: Health threat by PM2.5 is the most important thing people care about.

-P5L1: remove “(SPM)”. You don't use it in the following text.

-P5L3: haze is not actually caused by “the interactions between...”. It's a synergy effect by these factors.

-P5L12-13: is there any reference saying “substantial efforts since 2009”? Zheng et al. (2018, ACP, Trends in China's anthropogenic emissions since 2010 as the consequence of clean air actions) shows emissions dropped substantially only after 2013.

-P7L2: which year of anthropogenic emission from MIX?

-P8L20: sub-grid convection (SGCV) is found to be zero in the simulation. It can be possibly due to no precipitation in this case. But why gas-phase chemistry is also zero, or what it specifically means? Nitrate formation is dominantly through gas-phase oxidation of NO₂+OH.

-P9L14 & L16: You haven't defined what “two stages” are. Maybe don't need to mention this here.

-P10L12-19: please delete unnecessary “marked in” in this paragraph.

-P11L4-L6 & P12L10-11: The NMB (IOA) and R are the only metrics you used in the
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text and they are informative enough. I suggest remove other metrics if possible. Or define them in Table 3.

-P11L10: change “options” to “parameterizations”

-P13L24: add “air quality” before “threshold value”

-P14L8: I suggest change “internal” to “dominant” or “leading”

-P16L2-4: please delete “Suspended...during winter haze periods”. You have done this in the Introduction section.

-P16L15-16: Is this process important in your case? If not, saying this here reads misleading.

-P16L10: I suggest to move Fig. S4 in the main text. It is interesting to show the important role of absorbing aerosol on regional circulation change. This result is consistent with the simulation by Qiu et al. (2017).

-Conclusions and discussions. The “conclusions” part can be shortened and concise, which should make room for more insightful discussion. I came up some ideas. (1) how the IPR scheme can be further improved? (2) the authors could discuss the possible application of IPR scheme in future haze study (both winter and summer), because the work provides a quantitative analysis of how aerosol radiative effects change PM2.5 through physical and chemical pathways.

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