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Comment on acp-2022-279

Anonymous Referee #2

Referee comment on "Possible influence of sudden stratospheric warmings on the atmospheric environment in the Beijing–Tianjin–Hebei region" by Qian Lu et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-279-RC2>, 2022

Lu et al. studied how stratospheric sudden warming (SSW) events can influence the air quality in Beijing-Tianjin-Hebei region in east Asia using a combination of re-analysis and observational datasets. In particular they study a coupling on subseasonal-to-seasonal time scales and a distinction between split and displacement SSWs. Their work is based on an interesting research question and the corresponding results could indeed be useful for a wide audience. The paper generally follows a reasonable structure and covers a range of analyses covering large and small scale diagnostics in both stratosphere and troposphere. The figures are mostly easy to understand and presented in an overall adequate way. The language used sometimes seems quite cumbersome or unnecessarily complicated.

However, I feel like the content of the paper could be more focused on the new aspects of the study, e.g., by emphasising in more detail the concrete chain of processes involved in the coupling. I am further not fully convinced by the robustness of some of the signals and further discussion and/or analysis is required. In my opinion, the manuscript needs some substantial revision (see comments below), but could afterwards very well form a valuable contribution to current research.

General comments:

1. A large part of the manuscript covers the influence of SSWs on the (large scale) tropospheric circulation, which is a topic well discussed in the existing literature. On top, it seems to me like much of the corresponding results are not very convincing: Fig. 1, for example, is supposed to show differences in the dynamical downward coupling between split and displacement SSWs in several metrics, but one can hardly see any significant or substantial anomalies near the surface in any of the panels. This might simply be an

unlucky choice of diagnostics. A possible approach here might be to extend the literature review in terms of SSW-research rather than "reinventing the wheel" and instead focus more on the connection between large scale circulation and regional circulation/pollution.

2. One of the main points of the study is the distinction between split and displacement events, however, none of the figures show specific difference-plots. Hence, small differences can be masked by large absolute anomalies despite still being important. E.g., Figs. 2b and e or Figs. 6a and b could be easier to interpret if you also show corresponding differences.

3. You should probably extend your discussion about robustness and significance of your results or even extend your analysis to extract more significant signals. Fig. 6 suggests reduced visibility following the first two weeks after split events both overall and compared to displacement events. However, the diagnostic shows high day-to-day variability and I am not fully convinced this plot shows an actual downward influence. The same holds for other figures. One way you could deal with these weakly significant signals is to further emphasise consistency between diagnostics.

4. I feel the chain of processes leading from a polar vortex break down to changes in regional air quality is not made clear enough. Fig. 5 shows differences in regional winds and planetary layer height patterns between split and displacement events, which are supposed to explain the changes in air quality. However, during days 0-10 (when the differences in visibility are largest according to Fig. 6) the magnitude in both are relatively equal, while for days 20-30 (when Fig. 6 suggests almost no differences) you find strong dynamical differences.

Specific comments and typos:

L13: I would move the sentence starting "As the duration of split SSW..." to later in the abstract as you should first address SSWs in general and then make the distinction between different subclasses.

L64-66: You do not necessarily find enhanced wave forcing preceding SSWs (eg: de la Cámara, 2019, JoC)

L67: Maybe make clear this particularly holds for the zonal mean anomalies!

L84: Please add a note introducing PM2.5

L101: m/s is missing for g

L112: 121days seems like a large window size.. are your results sensitive to it?

L118: Please add at least one sentence describing what these diagnostics are.

L135: how many minor warmings do you find? is it worth showing a plot for these events similar to Fig. 1 (in a supplement)?

L140: 2 January 2019 is listed as both displacement and split event, does it enter both composites during your analysis?

L160: This seems to be a statement that should be clear at this point in the paper, especially because it is also mentioned in the introduction. I would much rather like to see a similarly detailed discussion on how the tropospheric circulation might affect regional air quality.

L178-180: This statement also seems a bit misplaced within the results section.

L180: denote -> are consistent with

L194-195: But you just investigated this, right? So do you conclude there is a downward influence or not?

Fig. 3: I suppose the small green box marks the area of interest? Maybe make the box more pronounced and mention it in the caption. Also: the downward influence of stratospheric anomalies is relatively weak in the mid-troposphere and usually strongest near the surface (eg: Baldwin+Dunkerton, 2001, Science), so maybe 500hPa is not the best level to look at.

L404: The word "persist" can sound like you are talking about specific events that last this long; make clear you are talking about the average.

L406: can propagate

L404-406: Based on your results I am not really convinced you can make this statement with such certainty. Fig. 1 shows no signal for either type of SSW below 500hPa except for U (with essentially no statistical significance). Further, it seems like the surface temperature anomalies are stronger in the displacement case (if there are any significant differences at all) following the SSW.

L422-423: This is a hypothesis, right? You don't actually look at any "pure" monsoon diagnostic.

L439: Did you remove a potential inter-annual trend due to these policies or other climate signals?