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# Comment on acp-2022-48

Anonymous Referee #2

Referee comment on "Regional $PM_{2.5}$ pollution confined by atmospheric internal boundaries in the North China Plain: boundary layer structures and numerical simulation" by Xipeng Jin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-48-RC2, 2022

## GENERAL COMMENTS

Note: the reviewer had only access to part 2 of the study. Publication of part 1 is needed before publication of part 2.

In the manuscript by Jin et al., three representative cases of pollution in the North China Plain are selected and discussed. The corresponding meteorological conditions simulated by the Weather Research and Forecasting (WRF) model are presented. In a first part, these simulations are validated by comparison with the observed spatial distribution of near-surface potential temperature and wind velocity, and with the vertical profiles of wind speed and potential temperature from soundings. In the second part, the three-dimensional structure of the meteorological systems is analysed for each of the three cases with reference to the dynamics of the pollution dispersion.

The manuscript presents the extensive work done by the authors, which may be of interest to the scientific community, but it is not always effective in clearly explaining the relationship between the meteorological conditions and the pollution dispersion (e.g., cases 1 and 2). Additionally, the reading is further complicated by to the very poor English. The structure of the manuscript could be also improved. Hence, the manuscript can be accepted only after major changes.

## SPECIFIC COMMENTS

1. The dynamics of the pollution diffusion are only addressed from a meteorological perspective, in terms of transport and horizontal/vertical dispersion processes. Emissions (e.g., their spatial distribution and their temporal variations) are not even mentioned. The

authors should state why emissions are of secondary importance compared to the meteorological configuration they deepen in their manuscript, and why chemical transport models (e.g., WRF-Chem) are not needed/advised for the interpretation of these results.

2. While for case 3 the results of the meteorological simulations and the pollution dispersion dynamics are clearly explained, this is not the case, in my opinion, for cases 1 and 2. I think that the authors should better relate their numerical results with the pollution dispersion dynamics, i.e. by discussing the relation between Figs. 8-9 with Fig. 3 in the corresponding cases.

3. The structure of the paper could be improved. Figure 12 is very explanatory and, in my opinion, should be shown at the beginning of the manuscript, in order to introduce the cases. However, it should be clearly stated from the beginning that the "frontal category" is not addressed in the paper, as it has been already discussed by Jin et al. 2021 (otherwise the reader will be convinced that the three cases discussed in the manuscript are the three ones shown in the figure). Hence, the paragraph "Frontal category" at page 13 and the corresponding Fig. 7, referring to a case that is not properly introduced (2 December 2017) should be removed.

**TECHNICAL REMARKS**

- Is "Atmospheric Internal Boundaries" common expression? I have found very few papers referring to AIB, with part of them using it with reference to the tropopause. Moreover, case 3 is essentially due to orographic obstruction, therefore I wonder if this expression should be used at all;

- large part of the abstract (e.g., classification, percentage of occurrence, etc.) describes the work done in part 1 paper (as clarified in the Introduction), therefore the abstract should be rewritten in a more specific way for the present manuscript. It should rather focus on the results of the validation and use of numerical weather simulations to explain the pollution dispersion. The first category should not even mentioned, as this was already studied in a previous publication. Mention of the sub-categories in the abstract is premature (l. 10-14 are obscure to the reader). Also, no classification (l. 2) is made in the present manuscript, but rather some representative cases are chosen and discussed;

- please, revise use of "the/a" articles, which are missing in many sentences, e.g. at l. 1, 7, 19, 117, 277, 351;

- as well "as": l. 7, 496;

- l. 29: "is" --> "plays";

- l. 31-32: what "property"? What "variation"?

- l. 51: Do you mean "At the intermediate scale"?

- l. 67, "is still insufficient": any bibliographic reference to support this sentence?

- l. 79: missing conjunction?

- l. 86: "associate" --> "associated";

- l. 88: "the" --> "an";

- l. 106: "was" --> "were";

- l. 114: "the three-point moving average method" --> "a three-point moving average", unless a more specific technique is meant here (reference needed in that case);

- l. 126, 341: "pentagram" --> "star"; "pentables" --> "stars";

- l. 137, "three categories/six types": it is difficult to understand the relationship between the "categories" and the "types";

- l. 141-142: is the frontal case was already studies in a previous paper, there is no need to recapitulate it here;

- l. 142-145: too much detail relative to part 1. The reader should be able to understand this manuscript independently from the first part;

- l. 156: the two peaks are not clear in all sites. Also, Fig. 2a does not show the formation stage (increasing concentrations) for most sites;

- l. 170-173: for case 1, the build-up seems to start also at the south-west side, not only along the mountains. For the same reason, it is difficult to state that the "pollution center has been transferred eastward";

- l. 205: "... southern edges"?

- l. 206 and 256: is "high-pressure invasion" a Chinese idiom?

- l. 215-218: clearly state that these are observations and that they are spatially interpolated based on Jin et al. 2021;

- l. 189: rephrase this sentence, it is unclear;

- Figs. 4, 5, 8, 9, and 10 are too small. Consider rotating them by 90° and displaying them at full page;

- l. 240: the large correlation coefficient of the potential temperature may be simply due to the day/night cycle, which is common in both the model and the observations, thus it is not representative of the model performances;

- l. 250-251: rephrase;

- l. 260-262: what "area"? Also, the main clause is missing;

- l. 264-265: "can be" --> "is";

- l. 280: "being" --> "playing";

- l. 308: "critical to" --> "critical for";

- l. 309-310: instead of listing all cases, wouldn't it be simpler to just say "For all cases"?

- l. 324: I cannot see any Figs. 7c-d;

- l. 332-336: if subtypes are not introduced, then rephrase without mentioning them;

- l. 365: "left" --> "west";

- l. 374 and 478: please, explain what you mean by "sub-synoptic scale characteristics/features";

- l. 394: "extracted" or "shown";

- l. 430, "more susceptible to the local property": unclear;

- l. 442-444: grammatically inconsistent, please rephrase;

- l. 460-470: this case is not discussed here, please remove this part;

- l. 502: "roughly" --> "rough".