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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulating aerosol–radiation–cloud feedbacks on meteorology and air quality over eastern China under severe haze conditions in winter" *by* B. Zhang et al.

Anonymous Referee #2

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My overall rating of this paper is somewhere between minor and major revisions; I've marked it as major since I would like a second look at the paper once the revisions are complete, not an option if they are marked as minor. The authors need to describe the potential impact of grid resolution vis-à-vis the cloud formation setup in their simulations, and (the most important point), they need to provide a solid justification for the absence of secondary organic aerosols in their model simulations and a quantitative discussion of the potential impact of its absence (or, better, repeat the simulations with a SOA parameterization in place). Aside from those two concerns, there are a

few minor issues where the text needs to be clarified, and a large number of cases where spelling or grammar needs to be corrected. The paper is basically sound and will be a useful addition to Atmospheric Chemistry and Physics once these issues are addressed.

Main issues:

(1) I have concerns with regards to the authors' conclusions regarding the relative importance of the aerosol direct versus indirect effect. In their work, for the time period and region studied, they found that the indirect effect was relatively minor compared to the direct effect. This is contrary to other studies (the authors quoted Forkel's work, and there are others coming out in the Atmospheric Environment special issue on Phase 2 of the Air Quality Model Evaluation International Initiative (AQMEII-2) which the authors may wish to examine). One of the things that has come out of that multi-model comparison (where the indirect effect dominated across multiple models and domains in North America and Europe) is that the magnitude of the indirect effect response is very sensitive to the manner in which it is implemented and on the cloud microphysics parameterizations used. I agree with the authors' suggestion that this may be a special case in that the winter haze events simulated are for relatively low cloud conditions, and their check of the model response with the more sophisticated Morrison et al microphysics scheme instead of the Lin scheme suggests a general low sensitivity to the microphysics helps in reducing the possibility that the microphysics itself is an issue. However, they should also discuss how the resolution of the model grid relates to the cloud formation in the model, and how this may affect the indirect effect response. That is, the authors are carrying out their simulations with a relatively coarse resolution of 27km in the horizontal. This in turn requires the use of a cumulus parameterization (the authors make use of the Grell-Devenyi, 2002 scheme), since the cloud microphysics parameterizations are not capable of creating cumulus clouds at that resolution. It is not clear in their section 2.1 exactly how the model generated aerosols are incorporated into either the radiative code (direct effect) or the cloud formation parameterizations

(indirect effect). For the direct effect, one needs a means of working out the particle radiative properties (optical depth, single scattering albedo, asymmetry factor) for incorporation into the radiative transfer. What was the means of doing that employed in this work (e.g. a Mie code incorporated into the model, a lookup table? What mixing assumption was used – heterogeneous mixture or core-shell?)? A few words of how this is set up is needed, beyond the references given on page 26089. For the indirect effect – how were the aerosols incorporated as cloud condensation nuclei into the parameterizations used? E.g. what was the means by which speciated aerosols were converted to cloud condensation nuclei numbers in the aerosol microphysics scheme (e.g. Abdul-Razzak and Ghan (2002), or some other scheme?)? Was the model modified to incorporate the effects of aerosols into the cumulus parameterization and if so, how? If not, then the authors should caveat their conclusion regarding the relative importance of the direct and indirect effect by noting that due to the resolution employed, only part of the indirect effect is incorporated, due to the need for a cumulus parameterization (which lacks a feedback connection to the aerosols) and the low resolution of the model, which requires the use of a cumulus parameterization.

(2) The authors mention in a single line in the text (page 26089, line 16-17) and once in the conclusions that secondary organic aerosol formation was not included in their model. This choice was not explained or justified in the text. This is a potentially major problem, in that organics often make up the bulk of the PM2.5 mass, especially in urban areas. The authors' PM2.5 comparisons to observations may be therefore be flawed – had secondary organic aerosols been included, they would potentially have much higher PM2.5 values, and a larger impact of aerosols on direct and indirect effect radiative transfer. The authors need to explain why this choice was made: WRF-CHEM comes with secondary organic aerosol formation parameterizations – why was one of these not used in their work? Ideally, they should repeat these runs and analysis with a secondary organic aerosol formation algorithm in place. Failing that, the authors need to provide a justification for the lack of this source of aerosol in their simulations, as well as quantifying the potential impact of its absence on their results, using estimates

of speciated PM2.5 for their study area.

Minor Issues:

Page 26086, line 20: "improves model's performances" should be "improves model performance".

Page 26087 Line 6-7: "burn out the cloud" is not a particularly clear description of the meteorological process concerned. Please explain in more detail. Line 7: "increasing interests" → "increasing interest". Line 16: "version of Weather" –> "version of the Weather" Line 21: "convections" –>→ "convection" ; "the PBL, and" might be better as "the PBL, hence"

Page 26089: Line 12: "as gas-phase" –>→"as the gas-phase" Line 16: "matters" –>→ "matter" Lines 28-29: There needs to be an explanation as to why this particular forecast cycle was used (i.e. why not 2 days, or 1 day, or 7 days)? Section 2.1 also needs mention of the aerosol assumptions used in the model for the EMP scenario (e.g. any default aerosol direct effect radiative properties, any default assumptions regarding cloud droplet numbers or cloud liquid water content).

Page 26090 Line 4: "the radiative" →–> "the direct radiative" Line 8: "model setups" –>→ "model setup" Line 20: What was the basis for this particular split in PM mass between nucleation (not nuclei mode) and accumulation mode? Give a reference or an explanation.

Page 26091: Line 12: I have some concerns that all of the observation stations are in cities – the observations may thus be controlled by very local sources, which may not be captured that well in the model emissions at that resolution. Do the authors have rural stations that could be used for comparison as well? Line 14: "aerosols distributions" –>→ "aerosol size distribution" Line 16: "performances are" –>→ "performance is"

Page 26092: Line 4: "meteorological variables." –>→ "2 m relative humidity and tem-

perature." Others have not been shown, so the statement should be limited to the analysis presented. Line 22-23: "may have influences on the accuracies" –>→ "may influence the accuracy" Mention, in the discussion of Figure 2, that a later analysis is made for all three scenarios (and include the RAD run results in Figure 12).

Page 26093: Line 1: "distributions" –>→ "distribution" Line 4: "well captures": this sort of qualitative phrasing should be avoided in favor of a qualitative statement of the model biases, etc. For example, from Fig 4, the model apparently has a negative bias of 80 ug/m3 in the cities to the north-west. The authors also mention later (line 12) that the model has an overall low bias for cities with high PM2.5 levels, which seems to contradict the "well captures" statement made on line 4. Line 18: "performances" –>→"performance" Line 24: "emissions is" –>→ "emissions are" Line 26: "productions of" –>→ "production of"

Page 26094: Line 12: "matters" –>→ "matter"

Page 26095 Figure 7: Note that the "blue to red" colour scales make it difficult to distinguish contour levels that are on the low or high end of the scales. Suggest using a rainbow scale for the difference plots as well. Line 16-17: "which our finding is consistent with." –>→"which is consistent with our findings." Line 21-22: The authors should add a few lines discussing the potential impact of ice nuclei on clouds here. Line 25: "a weaker" →–> "weaker" Line 28-line 1 next page: "Due to a weaker convection resulted from" –>→ "Due to a weaker convection resulting from".

Page 26096: Line 6: "radiations. So that changes" –>→ "radiation. Changes" Line 16: "formations mainly occur" –>→ "formation mainly occurs" Note that the caption for Figure 8 needs to mention that a-f are model values and g-l are differences (and for the latter, which differences correspond to which panels needs to be identified).

Page 26097 Lines 15-16: There should be some mention in the text whether the feedbacks improved the forecast for the pollutants. I may have missed this. Lines 22-23: Might be worth comparing to the AQMEII-2 Atmospheric Environment Special Issue

papers, if you want a more recent and multi-model comparison (see their on-line page – there are a few comparing the equivalent of the authors' RAD, EMP and BASE simulations, for North American and European domains). Line 28: Max enhancement is 69.3 ug/m3, but the colour scale in the figure only goes to 28: suggest that the entire max to min range is included in the scale. Page 26098 Line 1: "Bohai Sea" –>→ "the Bohai Sea". I suggest that one of the starting figures show the locations of place names. Readers unfamiliar with the study region will not know where any of the places mentioned in the description are located. Line 3: "respond to" –>→ "responds to" Line 8-9: There needs to be a justification for why the authors feel that the reduced PBL height and stabilized lower atmosphere is "the most important". Why? Line 13-14: "which is the same situation for" –>→"and also for" Line 25: reference to Easter et al should also appear in section 2.1.

Page 26099: Line 2: "from WRF-CHEM model configurations" –>→ "from the WRF-CHEM model configuratons used here" Line 10: "Chengdu." –>→ "Chendu (left column of panels in Figure 11)." Line 11: "January The" –>→ "January. The" Line 12: "has a" –>→ "have a" Line 13: "cities" is misspelled. Line 14: "Suppressions" –>→ "Suppression" Line 15: "Changchun," –>→ "Changchun (right column of panels in Figure 11)," Line 23: "are suppressed" –>→ "is suppressed"

Page 26100: The first paragraph merely restates what has already appeared in the paper and is unnecessary: delete. Figure 12 and the text associated with it should include the RAD run. Line 14: "model's performances" –>→ "model performance". Line 20: I'm not sure if NCP has been defined earlier in the text. Please define it, if not.

Page 26101: Line 5-6: "partially due to the missing of smaller scale temporal and spatial information averaging". This portion of the sentence is not clear, please rewrite. Line 11: "Fig 12" should be "Fig 13" here, I think. Line 25: I suggest you quantify the last statement by including some bias values for the entire grid for each of the runs, to show how the overall performance changed.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 26085, 2014.