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Interactive comment on “Development of a source oriented version of the WRF/Chem model and its application to the California Regional PM₁₀/PM_{2.5} Air Quality Study” by H. Zhang et al.

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This is a very interesting paper that compares differences in PM mass, size distribution, and radiative effects of aerosols over regional scales associated with external and internal mixing approaches. It is also clear that a lot of work was needed to implement the source-oriented external mixing approach into the WRF-Chem model. As one of the contributors to WRF-Chem community model, I have several comments that would be useful for the authors to address.

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Page 16458, line 11: I do not think the authors have shown that the approach “more accurately predicts radiative feedbacks”. While their approach is more realistic than assuming only internal mixing, they have not compared simulated aerosol optical properties with any observations. What they have shown are differences resulting from external and internal mixing assumptions. The approach and results are better stated in lines 26 and 27.

Page 16459, line 5. Figure 7 shows the absolute differences, so it is difficult to infer percent differences as stated in this sentence. It would seem that a maximum difference of 20% may not occur in many grid cells.

Page 16459, lines 7-8: The authors mention increases in atmospheric mixing; however, the changes in radiation are small and may not significantly affect vertical mixing and the paper never shows how differences in the mixing state of aerosols affect boundary layer structure and mixing.

Page 16460, line 25. Matsui et al. JGR (2013) is a recent paper that simulates the BC mixing state in the WRF-Chem model. While this recent development is not yet available in the public release of the model, it is an example of a more detailed representation of the aerosol mixing state in a regional model.

Section 3: Are particles from different sources permitted to mix together? This is not clear in the description. If not, what is the impact of neglecting coagulation between different particulate types? Lines 1-9 on page 16466 suggest that coagulation happens only within a single source type.

Page 16465, lines 4-7: Regarding the initial and boundary conditions, were there observations available that provide the mixing state of particles for use in WRF-Chem? This information seems to be defined in the papers cited, but it seems significant enough that some description is needed in this paper.

Page 16466, lines 18-20: In the public version of WRF-Chem, the ACM2 scheme is not

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compatible with chemistry. The developer of ACM2, has been modifying the boundary layer scheme to include mixing of trace gas and aerosol species that will likely be available in a future release of WRF-Chem. Therefore, some discussion is warranted to describe how the non-local and eddy diffusion is included in the present version so that confusion is avoided for future users of the code.

Page 16467 lines 1-2: I believe the standard WRF code (without aerosols from WRF-Chem) assumes no aerosols for those radiation calculations. The latest version (3.5) does include climatological aerosols for some of the radiation schemes.

Page 16467 lines 3 – 13: The authors describe how aerosol optical properties are calculated, which is very similar to the existing aerosol optical property module in WRF-Chem as described by Fast et al. (2006) and Barnard et al. (2010). Was this module modified to account for the new aerosol model, or was a new optical property module developed? Based on the description, the basic difference between the authors treatment of aerosol optical properties and the default treatment of aerosol optical properties is simply due to differences in the aerosol model mixing state.

Page 16471, line 12. The use of the word “feedbacks” here does not seem technically correct. The aerosols directly affect the shortwave radiation, but feedbacks are usually assumed to be processes that close the loop, i.e. changes in radiation that subsequently affect meteorology and then back to aerosols.

Page 16471, line 25. It is true the maximum difference is 2 K, but this is not occurring over a large region (it is hard to differentiate the pink and red regions in the figure).

Page 16472, lines 20-29: There is not much explanation for the differences in total BC between the external and internal mixing states. I assume that most of these differences are due to differences in dry deposition?

Page 16474, figure 12: There are large differences in PM concentrations, but it is not clear what are the factors contributing to the differences. The large differences imply

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large differences in mass in general but that might not be the case. Are some of the differences between the two simulations simply due to small meteorological differences that result in small spatial shifts in the PM plume? It would be useful to compute the total mass as a function of time over the San Joaquin Valley between the two simulations to get a better idea of the real differences in mass.

Page 16475, line 7: The phrase “more accurate optical properties” does not seem right. I agree that the source-oriented external mixing approach is more realistic than internal mixing; however the authors have not shown that the present optical properties are more accurate in terms of observations.

Page 16476, lines 6-9: I agree that the model is more realistic, but in the future it would be interesting to compare the BC mixing approach of Matsui et al. (2013) with the present source-oriented external mixing model.

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