Response to Editor

The authors sincerely thank the reviewers' professional evaluation and valuable suggestions. According to suggestions from Editor and reviewers, we have made corresponding corrections to our previous manuscript, and the detailed point-by-point responses are listed below

Response to Referee #1

RE: Impact of modified turbulent diffusion of PM2.5 aerosol in WRF-Chem simulations in Eastern China

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General Comments - Peter Taylor

Question1:

The authors have made a number of changes and have provided detailed responses to questions raised about earlier versions. I still have concerns about the modified stability function used, Eq 4, in connection with the Turbulent Diffusion Coefficient (TDC) and feel that the degree of improved performance is exaggerated. Despite Response 14, I am not really sure whether the % improvements are C/A or C/OBS values. Providing the bias values of both the original and the new schemes might be clearer. From Fig 4, Relative Bias is given as (NEW-OBS)/OBS and it is clear that individual stations have a wide range of bias % and that modest improvements of order 10% (line 453) in the mean over all stations may not warrant the claim, line 286 that "the new scheme can significantly reduce the degree of overestimation", and similar on line291. Also line 452 could make it clear whether these are % reductions of % overestimates rather reduction in the % overestimates themselves? Reduced by, or reduced to?

Response1:

Thank you again for your professional comments and valuable suggestions to improve the quality of our manuscript. Based on these comments and suggestions, we have made careful modifications to our pervious manuscript, and the detailed point-by-point responses are listed below.

Indeed, we agree with you that the coefficients of the function can continue to be corrected in the future with the support of more observation data. However, the role of turbulent diffusion is very important. Other processes may affect the simulation of pollutants, such as dry deposition process and emissions. Much more researches need to be done in this field in the future. Percentage improvement here refers to the improvement of the new scheme compared with the original scheme. Actually, the absolute bias represents the difference between the new scheme and original scheme. According to your suggestion, we have modified this part. The percentage (%) in Line 452 refers to the reduction in the overestimates themselves, and it is reduced by.

Question2:

While I appreciate the need to reference relevant prior work I am surprise that a paper on a relatively narrow topic needs about 60 references. The paper is well written but will need some language editing.

Response2:

Thank you very much for your affirmation. In the description part of the model, there are many parameterization schemes, and each scheme needs corresponding literature, which may lead to too many literatures. According to your suggestions, we have modified the language and deleted some unimportant references.

Minor points

Question1:

p5 Line 109 It may be misleading to say that the roughness is considered as zero. As the authors note in Response 8 to questions on the previous version, WRF does treat the PBL and surface layer and, unfortunately, the PBL code ignores z0. Although no changes have been made to the surface layer code it probably does involve a roughness length based on land use maps. WRF boundary layer modules, MYNN and YSU make use of z0 values based on land use. At this stage just avoid discussing z0 unless you plan to dig into the WRF code and find out.

Response1:

Thanks so much for all your helpful advice and info! We have revised the statements in this part.

Question2:

p5 line 120 Does this suggest that Ri values in the original field data were based on (60m - 10m) differences? Do these really give representative "gradients"

Response2:

The disturbance of the early time of the GPS sounding balloons taking off would cause uncertainty of the mass concentration of $PM_{2.5}$ near the ground, so we selected 10 m as the lower height to avoid this. According to the constant flux layer hypothesis, the upper level should be within the surface layer. For convenience of calculation, we rounded the height difference to 50 m. Therefore, 60 m was selected to be the higher level for calculating the vertical gradient of $PM_{2.5}$ concentration.

Question3:

p6 line 141 I did not have time to look into the "L-band radiosonde system" but assume it transmits 1Hz data as it rises. The issue is what vertical z resolution does this represent - what is the rise rate?

Response3:

The resolution of L-band radiosonde data is 1 Hz, and the rise rate of data in this study is about 6m s^{-1}. If necessary, we can provide some raw data. Thanks.

Question4:

p7 line 185 Not clear what the 8 months are, 4 with the original model and 4 with the new?

Response4:

Yes, your understanding is correct.

Question5:

p7 line 185 Re Fig 2, I am just curious whether kstart = 1 always in these runs?

Response5:

Yes, k_{start} is always the same in all runs, and $k_{start} = 1$.

Question6:

p8 line 210 ++ It is fine to use fine grid finite differences to approximate gradient Ri. Formally a bulk RiB would be based on two widely separated levels, one of which is normally the surface, and should be specific to those levels. As Garratt (1992, p37) notes, politely, some authors use the bulk term incorrectly.

Response6:

You're quite right. In practice, the gradient Richardson number is often approximated in finite difference form and the resulting parameter is sometimes referred to as the bulk Richardson number. In our model setting, we have encrypted the number of vertical layers below 2 km, which can better analyze the structural characteristics of the boundary layer. Please see Lines 163-165 in the revised manuscript.

Question7:

p11 line 269 See general comment. Need to make clear what "mean absolute bias" means. Is it the mean of an absolute value <|OBS-Model|> or just <OBS-Model>, where <...> means "mean".

Response7:

We are very sorry for the confusion caused by the unclear expression. The "mean absolute bias" is the mean value of absolute bias. We have modified the corresponding statements. The absolute bias here refers to the deviation between the new scheme and old scheme relative to the observation. And the calculation formula of absolute bias is $\mathrm{AB} = |\mathrm{RB}_{\mathrm{new}}| - |\mathrm{RB}_{\mathrm{original}}|$, where $|\mathrm{RB}_{\mathrm{new}}|$ and $|\mathrm{RB}_{\mathrm{original}}|$ represent the relative bias of new and original schemes, respectively. The calculation formula of relative bias is $\mathrm{RB} = \left(\overline{\mathrm{X}_{\mathrm{sim}}} - \overline{\mathrm{X}_{\mathrm{obs}}}\right) / \overline{\mathrm{X}_{\mathrm{obs}}} \times 100\%$.

Question8:

p15 line 393 Are model values of dry deposition available? Do they play a significant role in the PM2.5 budget?

Response8:

In the current model, we really do not separate the contribution of each process. However, based on the previous research results of individual cases using process analysis (Chen et al., 2019), it can be seen that the process of dry deposition has a certain impact, but the contribution of dry deposition is less than those of emission and turbulent diffusion. Next, we hope to analyze the long-term heavy pollution episodes in detail through the method of process analysis.

References

Chen, L., Zhu, J., Liao, H., Gao, Y., Qiu, Y., Zhang, M., Liu, Z., Li, N., and Wang, Y.: Assessing the formation and evolution mechanisms of severe haze pollution in the Beijing–Tianjin–Hebei region using process analysis, Atmos. Chem. Phys., 19, 10845-10864, https://doi.org/10.5194/acp-19-10845-2019, 2019.