RE: Impact of modified turbulent diffusion of PM2.5 aerosol in WRF-Chem simulations in Eastern China
Author(s): Wenxing Jia and Xiaoye Zhang
MS No.: acp-2021-435: MS type: Research article; Iteration: Revised submission

General Comments - Peter Taylor

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?

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.

Minor points

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 z_0. 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 z_0 values based on land use. At this stage just avoid discussing z_0 unless you plan to dig into the WRF code and find out.

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"

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?

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

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

p8 line 210 ++ It is fine to use fine grid finite differences to approximate gradient Ri. Formally a bulk Ri_B 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.

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".

p15 line 393   Are model values of dry deposition available? Do they play a significant role in the $PM_{2.5}$ budget?