EGUSPHERE-2022-1125 | Development and technical paper

Submitted on 20 Oct 2022

Passive Tracer Modelling at Super-Resolution with WRF-ARW to Assess Mass-Balance Schemes

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Author Responses to Referee Comments

Submitted on 01 Jun 2023: Anonymous referee #2

Color Code

Referee Comments in Grey

AC - Author Comments in Blue

(Sepehr Fathi, 13 June 2023)

Referee #2:

"Passive Tracer Modelling at Super-Resolution with WRF-ARW to Assess Mass-Balance Schemes" by Sepehr Fathi, Mark Gordon, and Yongsheng Chen

Recommendation: Minor revisions

General comments:
I appreciate authors addressing my two major concerns. The manuscript has been improved from its initial submission, and I think it can be accepted for publication after minor revisions.

Minor comments:
1. Abstract and introduction

- P1, L1: Different model resolution criteria were used to define "super-resolution" throughout the manuscript, and please unify it. For example, 250 m was used at L1, 50 m at L14, and 100 m at L29.

AC_1: Revised
L14: ($\Delta x = 50m, \Delta z \simeq 10m, \Delta t \simeq 0.2s$)
L29: (e.g., $\Delta x < 100m, \Delta t < 1$)

- P1, L3: "top-down retrieval" => "top-down emission-rate retrieval"
AC_2: Done

- P1, L5: "top-down retrievals" => "top-down retrievals of emission rate"
AC_3: Done

- P1, L12: "sub-grid TKE" => "sub-grid turbulence" AC_4: Done

- P2, L36: "for inverse method analysis" of what?
AC_5: revised to "for inverse method analysis of emission rates."

2. Section 2.2. Model and Technical Setup
- P6, L131: Specify the version of the WRF model used in this study (e.g., V3.8, V4.0, etc.), and cite the WRF technical note for the version used.
AC_6: Done

- P6, L139: Specify the order of spatial discretization methods used.
AC_7: Mentioned in the following sentence.

- P8, L194: "dispersion" => "diffusion" AC_8: Done

- P9, L199: "K Option was set to" => "For vertical and horizontal diffusion by sub-grid turbulence, K Option was set to" AC_9: Done

- Figure 2: "decreasing size" => "decreasing domain size" AC_10: Done

3. Section 3.1. Model Sensitivity
The authors used terminologies "evaluation" and "error" for comparison between d04 and d05 (and also between d03 and d04) in Section 3.1 Model Sensitivity, but they should be replaced with "comparison" and "difference". This would better distinguish comparison between different model resolution results (Section 3.1) vs. evaluation of simulation results against observational data (Section 3.2). Detailed suggestions are listed below.

- P12, Table 4: “Evaluation of” => “Comparison of” AC_11: Done

- P12, L278: “evaluated the performance of” => “compared” AC_12: Done

- P12, L278: “The evaluations were made” => “The comparison was made” AC_13: Done

- P12, L280: “Root mean square (rms) error scores” => “Root mean square (rms) differences” AC_14: Done

- P12, L281 and L286: “rms errors” => “rms differences” AC_15: Done

4. Section 3.2. Meteorological Evaluation
- P14, L301, “by interpolating”: Please, specify what interpolation method you used (e.g., linear interpolation).
AC_16: Revised, “linearly…”

- P16, L348, “Model wind fields ~ within on standard deviation for the three cases”: This is not true for Case 1. It should be mentioned that “except for Case 1”.
AC_17: Revised to “...within 1-2 standard deviation.”

- P16, L349, “WBEA wind speeds are lower than both model and aircraft wind speeds”: This is not true for Case 2. It should be mentioned that “except for Case 2”.
AC_18: Revised to “WBEA wind speeds are on average lower than aircraft measured values for all cases, and less than model wind speeds for cases 1 and 3.”

5. Section 3.3. Plume Characteristics
- P16, L363: “after start-up” => “after 1-hr spin-up” AC_19: Done
- P18, L367–L368, “covered less downwind range”: I disagree to this. If you compare the east boundary of the model domain, surface emissions covered more downwind range compared to stack emissions.
AC_20: Note that tracer release points for different sources are at different distances to the domain eastern boundary. Text was added to further clarify this “… covered less downwind range **(downwind distance from the point of release)**”.

- P20, L397–L413: There are a number of misinterpretations about the stability of the atmosphere in these paragraphs. I think discussions and analyses referring to the gradient Richardson number should be removed or rewritten to revise wrong statements in the current manuscript.

First of all, all three cases seem to be under thermally unstable conditions, from the characteristics of the plume dispersion and the mean temperature profiles that are well mixed below Zi. I assume case 2 is more purely thermally unstable with no and/or very weak wind shear, while other two cases are both thermally (e.g., by surface daytime heating) and mechanically (e.g., by vertical wind shear) unstable. The two paragraphs should be revised to correct that all three cases are under unstable conditions, not only Case 2 as stated wrongly in the current manuscript.
Second, the gradient Richardson number computed using adjacent vertical grid points is not appropriate to diagnose the instability for Cases 1–3, i.e., in the atmospheric boundary layer (ABL) driven by daytime surface heating. One thing to consider regarding the gradient Richardson number in thermally-driven unstable ABL is that when there are surface heating and dry convection is generated by it, turbulent mixing can be very active throughout the ABL even though local temperature gradient and the gradient Ri are larger than zero; i.e., a strong turbulence mixing by large-scale convection happens even though dtheta/dz > 0 and gradient Ri > 0. In this case, using a gradient Ri can mislead the instability of the ABL, as wrongly interpreted in this manuscript. What is frequently used to diagnose instability of the ABL is the Richardson number computed using surface-layer variables (e.g., surface fluxes of momentum and heat), which can take account of the effects of surface heating and shear. Similar to the first point, the two paragraphs should be revised to remove the analysis using the gradient Richardson number and correctly mention that all three cases are under unstable conditions, not only Case 2.
Third, the critical Ri of 0.25 used in this study is suitable for shear-driven turbulence, and the critical Ri frequently used for thermally unstable condition is 0.0. Even with the critical Ri of 0.0, using the gradient Richardson number would lead to misinterpretation of the instability of the ABL, as I mentioned in the second point.

AC_21: Thank you for pointing out this correction in the manuscript. In fact all three cases were under unstable conditions, at different rates. The gradient Richardson number for all three cases was below the critical value for the bottom 1/3 to 1/2 of the ABL. The following revisions were made, and text added to clarify this:

- Old line 400, new 404 line: **"As a result of (thermal and dynamical) unstable conditions**, tracer plumes from emission sources mixed in the vertical up to 2000 m during the simulation time."

- Old line 407, new line 412: "Atmospheric conditions during this case were fairly **constant**…"

- Text was added to old line 409, new line 413: **“Note that for this case, similar to the other two cases, atmospheric conditions were unstable (both thermally and dynamically) within the bottom 1/3 to 1/2 of the ABL.”**

- Conclusion line 568: “During case studies on 20 August and 2 September, atmospheric conditions **were less variable and the vertical wind shear was weak**, with higher wind speeds of about 5-15 m/s.”