Review of Manuscript *The Boundary Layer Dispersion and Footprint Model: A fast numerical solver of the Eulerian steady-state advection-diffusion equation* for *GMD*

Reviewer Report

General Comment:

The manuscript presents a novel atmospheric dispersion and footprint model (BLDFM) that numerically solves the three-dimensional steady-state advection-diffusion equation using a combination of the Fourier method, the linear shooting method, and an exponential integrator. The model is validated against two test cases: one with an available analytical solution, and the other using the established Kormann and Meixner footprint model.

The numerical methodology and implementation details need to be described more thoroughly. I find the work interesting and recommend a major revision before resubmission.

Major Comments:

1. The manuscript explains the underlying differential equation and the simplifying assumptions. A more precise and detailed description of the numerical method and computational results would greatly benefit the manuscript:

   (a) Line 148: The manuscript mentions the linear shooting method to solve the boundary value problems. The authors should explain the method and justify its use in this context.

   (b) Lines 157–162, Appendix A: The presentation of the numerical method is currently too brief. The manuscript provides limited context as to why this method was chosen and what advantages it offers over alternative numerical approaches. In addition, the authors claim in the main text that the method is consistent, stable, and robust; is there evidence supporting this claim? In the Appendix, is the scheme (A1)–(A4) taken from Hochbruck and Ostermann (2010), and have the authors modified it using the approximations (A5)–(A6)? If so, could they provide readers with a rough estimate of the computational time saved by not evaluating the sine and cosine terms?

   (c) Section 3: Do the authors implement the Dirac delta distribution mentioned in section 3 in their numerical solver? If so, how is this achieved in practice?

   (d) Do the authors evaluate the convolutions in Eqs. (26) and (27) in their numerical implementation? If so, how is this done in practice? They might also consider including a short remark about potential parallelization.

   (e) Section 5: The authors write: "In order to corroborate convergence, other resolutions and different parameter settings were tested as well. The relative error decreases with higher resolution (not shown here)." However, these results should be presented–e.g., in a table or a figure–to allow the reader to assess the convergence behavior quantitatively. Additionally, the BLDFM solver consists of several components, including a Fourier transform, a linear shooting method, and an exponential integrator. The statement that "the relative error decreases with higher resolution" is too general. The authors should discuss how each component contributes to the overall numerical error.

(f) Section 5: The authors should also discuss which parts of the code are amenable to parallelization in order to achieve faster solutions, as this is an important aspect of performance for practical applications.

(g) Line 248: The authors state: "This difference may be explained by the distinct model choices." It would be helpful to briefly discuss whether numerical errors in BLDFM could contribute to these differences. Ensuring that the observed discrepancies are indeed due to model assumptions rather than numerical artifacts would strengthen the interpretation of the results. The same consideration applies to the unstable case.

Minor Comments:

1. The authors perform a detailed analytical study of the system. It would be helpful to briefly highlight this contribution in the abstract or introduction, as it provides valuable guidance for the numerical implementation.

2. Lines 43, 47: To give the reader a better sense of the scales involved, it would be helpful to provide typical ranges for the atmospheric microscale in the planetary boundary layer and for the mesoscale.

3. Lines 45-47: The authors state that advection predominantly occurs on the microscale, while the temporal evolution of wind patterns occurs on the mesoscale. Could the authors clarify whether they refer here specifically to eddy-scale fluctuations rather than mean-flow advection? This would help avoid potential confusion about the scales at which advection acts.

4. Line 66: Could the authors comment on whether steep gradients in the scalar field occur in their typical application scenarios, and if so, how the Fourier-based solver deals with them? Since spectral methods may exhibit oscillations near sharp features when resolution is limited, a brief discussion of this aspect would help readers better understand the robustness of the approach.

5. Line 96: It would be helpful if the authors could briefly comment on how idealized the assumption of periodic (or vanishing) lateral boundary conditions is. Additionally, do the authors have any thoughts on how this approach could be extended to non-periodic boundary conditions? How would that change the efficiency of the computation of the numerical solution?

6. Lines 114ff, Equations (9) - (12): For the unbounded domain $z \in [z_M, \infty]$, the authors discard the growing exponential term in the solution. This can be interpreted as a physically motivated choice to ensure that the solution remains bounded for large z. It might help to clarify this point and distinguish it from the classical maximum principle, which is formulated for bounded domains. Clarifying this point could help readers better understand the reasoning behind the boundary treatment. In addition, could the authors briefly comment on whether a nonzero coefficient B would be physically meaningful or whether it would necessarily lead to unbounded growth?

7. Equation (22): The manuscript provides the formula for α without explanation. A brief note on how it was computed and how it follows from the linear combination of the two IVP solutions would improve clarity.

8. Section 3: It might help the reader if the authors explicitly stated that the Green's function depends on the particular problem.

9. Equations (26), (27): The authors might remind the reader that Q_0 is related to the boundary condition or provide a reference to the corresponding equation.

10. Section 6: The authors mention that FKM uses simplified assumptions, but it is not clear to the reader which specific equation BLDFM is being compared to. The manuscript would benefit from explicitly stating the governing equations and assumptions of the FKM model to clarify the basis of the comparison.

11. Line 270: "BLDFM's performance has been tested against a special analytical solution." Please clarify that this assessment refers to numerical accuracy and not computational runtime or efficiency.