10 May 2026

Response to RC 1 – GMD 2026-429

We thank the reviewer very much for his positive, detailed and valuable contribution to improve the manuscript. We have addressed the feedback in the revised version of the manuscript and respond to all comments below.

General comments

The authors present a new interesting tool, AgPaDS, developed to rapidly simulate Lagrangian transport in the atmosphere, coupled with advanced visualization and interactive simulation configurations, for agricultural applications. They compare its performance with HYSPLIT, one of the mostly used atmospheric transport models, demonstrating a significant increase in computational efficiency across different tests, thanks to the use of GPU. This tool goes into the direction of solving persistent challenges faced by agricultural researchers, such as (a) the simulation time (which is dealt by directly in the model) and (b) the coupling of windborne pathogen transport with plant epidemiological dynamic models (which the authors propose as a possible advancement for this tool).

Specific comments

Undoubtedly, the improvement in computation time compared to the benchmark is outstanding, and the visualization tools are also remarkable.

- *Thank you for the positive feedback.*

The article's focus is more on the algorithmic aspects than on the Lagrangian implementations, for which the authors drew inspiration from NAME. I recognize that this is a very technical paper, and as someone not deeply familiar with Lagrangian modeling at this level of implementation, I admit it is not always easy to follow.

For instance, (i) I would have preferred the model description (section 3) to appear first in the Materials and Methods;

- *Before submission, we had also discussed the ordering of sections 2 and 3, as both, the current ordering and a swap, have their advantages/disadvantages. The feedback is useful for us, as it indicates that, indeed, a swap in order would make it easier for readers to follow the manuscript. We have addressed the comment by inter-changing sections 2 and 3 in the revised manuscript.*

- (ii) acronyms (GPU, ECMWF, CUDA) are used without explanation.
  - *We have inserted the definition of all acronyms on first mention.*

On the other hand, despite the claims of a better compatibility of this tool with crop or epidemiological models, the coupling does not seem straightforward.

- *Our main argument is not a better compatibility in the sense of an easier or more user-friendly loose coupling of AgPaDS with epidemiological models (i.e. use file output of AgPaDS as input for epidemiological model), but instead a new possibility for tight coupling of the two modelling approaches, i.e. execution of atmospheric transport simulations as part of the main time-loop of a spatiotemporal epidemiological model, enabled by performance gains and model architecture (e.g., gridded release). We have already implemented this in a meteorology-driven SIR meta-population model (unpublished) and have conducted initial tests showing promising results. Originally, we had planned to include this in the current manuscript, but as it represents another layer of model development and testing and the manuscript is already comprehensive, it is beyond the scope of this paper. However, to address the comment, we have included an additional paragraph at the end of the discussion to summarize the approach in conceptual terms. In a follow-up paper, we will introduce details.*

I commend the authors for their accurate model evaluation setups, both in terms of experimental design and in terms of indicators used to measure the comparisons.

- *Thank you very much.*

However, I believe that a couple of points require revision or at least further discussion.

In the second experiment (Table 3), the authors state that differences between HYSPLIT simulations and AgPaDS are less than one order of magnitude, but the data actually show a one-order-of-magnitude difference; this is not inadequate per se, but needs to be better contextualized.

- *Corrected. Considering all metrics it is less than or equal to approximately one order of magnitude. We agree with the second part of the comment, indeed it is important to consider typical values and uncertainties regarding this quantity. This is included in the discussion.*

Moreover, in the third set of experiments (~Line 735), when comparing the simulation of atmospheric transport of *Phakopsora spores* by hurricane Ivan if it would be feasible to compare the simulated and observed deposition/presence of the soybean rust infections in USA (and not on the severity), instead of leaving it qualitatively.

- *We are not sure if we understand the comment correctly. It sounds as if the reviewer is suggesting that we are comparing the severity of soybean rust infections in qualitative terms with simulated deposition patterns, which we are not. As illustrated in Figure 7, we are comparing simulated deposition patterns with locations of sites of first detection of the pathogen in the USA. Unfortunately, a more exact quantitative evaluation is not possible given the available data, because the source strength is unknown (no direct measurements of pathogen emission rates nor sufficient information about infected area and infection intensity to estimate source strength in Colombia). Please note that, here we directly follow the authors approach of the original study – Isard et al – who have stated the same, the empirical data is too sparse to quantify emission and deposition rates in more detail, so they consider unit release in their simulations.*

My last question is, given the strong inspiration drawn from NAME, is there a specific reason why the authors chose to benchmark against HYSPLIT rather than on NAME?

- *As part of the development and testing phase, we have drawn inspiration not only from NAME, but also from IAMS, HYSPLIT, MPTRAC, and others (as referenced in the text). The decision to benchmark against HYSPLIT was motivated mainly by: (i) HYSPLIT is available online for everyone without license requirements; (ii) HYSPLIT is very widely used, including in directly related studies in crop epidemiology (e.g., Radici et al, collaboration of NOAA with FAO on desert locusts).*

Minor corrections

- L 15: GPU, which is the Graphical Processing Unit.
    - *Included explanations of acronyms throughout.*
- 38-39 “et al.” is sometimes italicized, but not always. Please homogenize throughout the text.
    - *Corrected, thanks.*
- 40. So far, the introduction quotes very general papers, without going into details. For instance, the authors do not tell the name of any of these “devastating crop diseases” (L. 38) and seem to suggest that long-distance-dispersed plant pathogens cause 17-30% of yield loss (L.40), while these percentages aggregate all infectious plant diseases.

   - *The introduction is kept fairly general on purpose, as the manuscript is not about one or two specific diseases, but about a new model that can be applied to a range of airborne diseases, and bioflows more generally. However, we agree that it could profit from some more specifics, and have included concrete examples. It was not our intention to make it seem as if airborne diseases alone cause these losses. We have rephrased to avoid misunderstandings.*

- 52: atmospheric transport models or Atmospheric Transport Models (as in L. 10)?
   - *Have ensured consistency throughout.*
- 80: What does I/O mean? Please, define acronyms throughout the text.
   - *Input/Output; have included explanation of all acronyms throughout.*
- 85: "These challenges evolve around, unknowns around pathogen viability decay during atmospheric transport: uncertainty estimates for processes involved in atmospheric transmission of crop pathogens". What does it mean?
   - *Rephrased to clarify. One key example of unknowns is a lack of exact experimental data on the proportion of spores, out of large populations of fungal spores (millions/billions), that survive certain regimes of meteorological factors like UV, temperature and humidity. One key example of previous uncertainty estimates in the literature is the work of Aylor, which is referenced in the sentence.*
- L81-132: these bullet points are interesting and cover exhaustively the issues with ATMs, but it think they could gain in readability – for example, by splitting or shortening some sentences and capitalizing the initial letter of each point.
   - *Noted and revised in attempt to improve legibility.*
- 97: I wonder if the authors meant "links" and not "vertices" (which I assume is asynonym of "nodes").
   - *Yes, corrected, it is links / edges.*
- L 120: Missing full stop.
   - *Corrected, thanks.*
- 140: Is the figure correctly placed here?
   - *The figure is placed on top of the page on which it is first referenced.*
- 210: P in Python should be capitalized, here and in the rest of the text
   - *Done; thanks.*
- 219: Specify what ECMWF is.
   - *Acronyms defined on first mention.*
- 361-369: What do subscripts v, p, lambda and phi stand for? Also, is m_v the same as m_i (I do not think so)?
   - *The subscripts lambda, phi, p denote spatial dimensions. In the initial version of the manuscript these are introduced in a later section of the manuscript, but they should have been introduced upon first mention, so this is corrected now. The two different notations m_v and m_i are chosen to represent different quantities. As explained in the text, m_v is used to denote a function to model viability decay, in the equation introducing the conceptual model, whereas m_i is used to denote the material carried by each simulation particle, i.*

- 394: SSD?
  - *Acronyms are now defined at first mention.*
- 397: CUDA? Quoted 33 times, also in Fig. 2
  - *Acronyms are now defined at first mention.*
- 460-461: I am not sure of the rationale. Is N the number of simulation particles as in L. 371?
  - *Yes, once introduced, the same notation is used throughout.*
- 490: Consider telling more explicitly the reader the dimensions of wind data in ERA 5 (x, y, pression), not just as unit of measures.
  - *This has been explained in the text already (see section 3.4.1 on model grid and section 3.4.3 on input data.*
- 503: Consider telling the reader what a timescale is.
  - *Included short summary of Lagrangian timescale.*
- 510: no space before comma.
  - *Checked for spaces before comma.*
- 537-554: A very interesting overview of possible techniques of implementing viability decay!
  - *Thanks.*
- 570: This is not the first time I see this value of this parameter used for wet deposition. This is quite evidently computed on an approximation, since 25.4 mm = 1 inch and 63.2% is just 100*(1 – exp(-1)), or the expected loss after 1 unit of time. It is a pity that there are better estimations for this parameter.
  - *As noted in the text, we choose to follow the parsimonious and simple, yet useful, parameterization for wet deposition as implemented in IAMS. There are various other parameterizations of wet deposition in the literature on atmospheric dispersion modelling, but few for the specific case of crop pathogens. We choose here a proven generic approximation, noting that it is straightforward to adapt this part of the model to specific crop pathogens in future use-cases, e.g. when specific empirical data are available for specific pathogen types.*
- 675: Despite atmospheric trajectories and material deposition look very similar, the same can not be said about Lagrangian particles in the air. Why?
  - *Whilst on first sight it appears like there may be much larger differences between the Lagrangian particles in air compared with the deposition patterns and trajectories, this is mostly a consequence of the type of visualization in the figure - the 2D point cloud shows 10 K simulation particles, which, in the central parts of the plume, overlap strongly, whilst in the outer parts individual particles / smaller subsets of particles follow slightly different paths. We choose to visualize individual Lagrangian particles instead of particle densities, as we want to illustrate a core numerical characteristic of the model, discrete simulation particles. The fact that small subset of Lagrangian particles in air deviate is to be expected, as these are stochastic particle ensembles. The gridded deposition plots show that the overall distribution of particles is indeed in good agreement. Trajectories are deterministic, so differences here are not due to random variations, but other model differences, e.g. interpolation scheme of meteorological data to trajectory position.*
- 692: Also relative differences HYSPLIT – AgPaDS look quite important int Table 2. Could you please discuss?

- *There are differences between the two models, but this is to be expected, and is discussed in detail in the manuscript (see second paragraph, section Discussion).*

- 710: The same can be said of Table 3, for example of the median deposition value. Despite what you state in L. 712, thz difference between 5.8* 10^{-10} and 2.8* 10^{-11} is ~1 order of magnitude.
  - *Corrected to smaller/equal to approximately one order of magnitude. Importantly, as stated above by the reviewer and already noted in the discussion, the extent is acceptable considering given uncertainty ranges, data sparsity, and the main modelling goal here of obtaining a highly performant yet sufficiently good approximation.*

- 735: "Whilst the available data does not allow for exact quantitative evaluation". This sentence makes me ask if (1) there were no better episodes of LLDto test the model performances, such as stem rust of wheat in East Africa or Asia or (2) a test to compare the presence (and not on the severity) of the soybean rust infections in the USA and your simulations would be feasible.
  - *The phrasing "exact quantitative evaluation" is intended to refer to an experimental design as used in large-scale tracer experiments, including the exact measurement of emission rates at the source along with measurements of spore concentration in air as well as deposition to the surface at various locations and times around the source. To the knowledge of the authors, on landscape to global scales this type of data does not exist for crop pathogens. We have added a clarification on the phrasing. The core objective is the development of a broadly applicable simulation tool, rather than a specific model for a specific disease outbreak like stem rust in East Africa. Therefore, we chose to evaluate against 2 existing well-tested simulation frameworks, complemented by a case-analyses that contains what the reviewer is suggesting, a comparison to presence/absence by looking at first detection sites in the USA. In future studies, we plan to conduct further evaluations as part of specific use-cases.*

- Line 873: "in comprehensive".
  - *Corrected, thanks.*

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