

REVIEW egusphere-2026-4624 (2026-10-15)

The manuscript titled “Terminal-Guided Back-and-Forth Nudging: Time-Reversed Smoke-Plume Reconstruction with Fourier Neural-Operator Acceleration” by Glišović et al. with reference egusphere-2026-4624 approaches the important need for accurate estimations of wildfire emission properties as well as their resulting plume history. This contribution introduces a novel modeling and optimization approach based on a spectral formulation of the problem and applies it to several idealized cases.

Generally, the modeling concept and results presented in this manuscript appear to be of scientific interest and value. However, the overall appearance of the manuscript, specifically concerning its length and clarity largely hinder its understanding and detailed evaluation:

Most prominently, the manuscript is too long and it appears that too much content squeezed into one paper.

- The abstract needs to be condensed with focus on the main objective.
- The presented model itself consists of several components the overview of which remain unclear and thus the reader quickly loses track of what has actually been done.
- Likewise for the results which include multiple experiments nicely separated into subsections, but make the reader easily lose the main objective of the manuscript.
- Additionally, there are several supplements added to the study.

Therefore, I would suggest considering a subset of the following changes:

- Condensing the manuscript content to a single specific objective (and making this very clear from the beginning and throughout the document).
- Splitting the manuscript into two (eg model description and results or different parts/stages of the modeling system including their description, results and discussion, ...).
- Revising Fig.2 or creating a new overview figure which visualizes the individual model parts, workflow and the different experiments more intuitively.
- More clearly connecting the different experiments with the respective parts/aspects of the model to be investigated.

Note: The comments and suggestions in this review reflect view from a different scientific discipline (i.e. data assimilation and optimization in air quality / chemical transport models), thus different problem understanding and naming conventions might be a major cause for confusion. However I believe that such cross-discipline work should be understandable for related disciplines up to a certain degree.

Therefore, I suggest major revisions of the manuscript w.r.t. the above and below mentioned comments prior to revision of smaller aspects and reconsideration for acceptance. Once a revised version is submitted, I am happy to provide more detailed feedback.

General comments:

A) Despite good efforts in manuscript structuring the content is not clearly outlined. This includes:

1. the formulation of the problem to be addressed (e.g. plume only observed at single time but spatially resolved, no observation error, ...) and of the specific study objective (if it is the one given in 1.472, it was never explicitly formulated before)
 - both to be clarified in the beginning of introduction and most important aspects also in abstract
2. the motivation, selection (incl proper comparison to existing methods see B) below) and overview (compare above) of the presented modeling framework, (in fact, the discussion

in Sec.4 provides a much better context which should be formulated more clearly right from the beginning)

3. The connection between individual model components in Sec.2, their respective evaluation experiments in Sec.3 and the discussion of these results in Sec.4 can hardly be made with the current structure of the manuscript (compare above).
- B) Some parts of the introduction are based on specifics of the model and are therefore not understandable at this point (e.g. l.49ff) or contain a lot of technical details (l. 71ff) rather than providing a short overview of the specific objective and content of this manuscript.
- C) The distinction and advantages of the presented method compared to existing methods need improvement:
1. Data assimilation and related inversion approaches generally not aim to perfectly reproduce observations, but rather provide a weighted combination of prior model forecast and observations. Also, they usually deal with problems where the plume is at no time fully observed in physical space. And so on...
 2. Is the time-reversed formulation in Eq. (8) not conceptually similar to a adjoint model in 4Dvar (see e.g. l.651-660)? And why is it advantageous? There are several studies applying 4Dvar or other adjoint methods to emission and plume optimization (e.g. Elbern et al., 2007 and many later studies). These methods do not suffer from small-scale amplification (compare l.223) and computational efforts scale linear with the number of iterations (compare l.351f).
 3. A discussion of ensemble inversion methods is missing completely.

Although it would be interesting to see how the presented approach compares to the existing ones, it would be out of the scope of this study. But a discussion of conceptual similarities and differences is critical for the novelty of this manuscript.

- D) Some smaller aspects of the model are not explained, particularly concerning randomness:
1. (l.403): how do the random seeds influence the model? And what is their purpose?
 2. (Sec.3.1):
 - How is the L^2 reconstruction error exactly defined? W.r.t. which quantity or run?
 - How is the L^2 error normalized to provide %-values?

The exact definition of relative errors becomes particularly critical for quantities that can become close to zero like in this case where it can significantly influence the interpretation of results.

Related context question: Does errors>100% mean that the error is larger than the true estimate, i.e. that no plume at all would provide better results? What does that tell up about the simulation?

3. (Sec.3.5): how are the “150 held-out test event” events generated? And what is an “event” in this context? A single scalar concentration value? A full spatio-temporal field? I cannot find any description in the manuscript.
- E) Some parts of the results are limited to listing performance numbers without further explanation or interpretation:
- Sec. 2.5: interpretation of differences between runs to be clarified, and results of Tab.2 to be explained and interpreted
 - Sec.3.1: results are missing interpretation
 - Sec 3.2: interpretation of differences between runs to be clarified
 - Sec. 3.5: results are missing interpretation
 - S7: l.631-639 is missing purpose and interpretation

If the respective experiments provide important conclusions, they have to be explained accordingly. Otherwise, please remove the experiments in shake of condensation of the manuscript.

Smaller comments:

1. the terms “terminal field”, “teacher” and “student”, “input channels”, “amortized” need explanation when being mentioned for the first time
2. (abstract):
 - It is not clear what exactly is estimated, eg spatio-temp concentrations? location of plume? emissions causing plume? (compare other comments in abstract). And what is observed? The estimation task is fairly different depending on the spatial and temporal extend of observations (eg single local observations, or time series, or complete fields)
3. (introduction):
 - (related to C) above) Despite the authors good efforts, the introduction of the two difficulties also needs clarification: I would understand: 1. structural: dependency on model dynamics? (eg if winds are wrong, the emissions are wrongly estimated although reproducing terminal observations). 2. information loss by diffusion? If so, it's not clear why emission-inversion systems address the structural difficulty, but not reconstructing an earlier plume when they optimize the emissions that created this plume (l.56f). Or did they invert the emissions at the observation time? Similarly for l.60.

There are many studies on time-inverted emission optimization in the field of air quality data assimilation. The estimation method is different from the one presented here, but - if I'm not mistaken - both tackle the same questions. This needs more explanation and discussion, pointing out similarities and distinction of the new approach need to be formulated more clearly here (e.g. around l.70).
4. some symbols are not explained: G_{β} in Eq.(9), $G_{i,\theta}$ in Eq.(18)
5. Fig. 7b: Axes are confusing because not consistent with other plots $x \sim \text{time}$, $y \sim \text{error}$.
6. Fig. 10: Here is hardly any difference visible in the physical-spcae maps. Can you show the difference maps to the reference instead?