

Review of the *revised version* of the paper “Development of the CMA-GFS-AERO 4D-Var assimilation system v1.0 - Part I: System description and preliminary experimental results.”

by Yongzhu Liu & Colleagues, DOI: <https://doi.org/10.5194/gmd-2024-148>

MS type: Development and technical paper

Iteration: Revised submission

2025-03-10

General Comments

I in general welcome the changes in the reviewed version of the manuscript as they provide more clarification and enter a little bit more into explaining the results. I however still have concerns before being able to recommend the paper for publication.

At first, I am now confused by the modified pictures in Figures 9 and 10. Following the suggestion of Reviewer 1, the authors have renamed the labels of the experiences in a more explicit manner (Table 3), which is good. However, my understanding is that these panels refer to exactly the same experiments. I would thus expect the pictures to show exactly the same fields based on the same data. Why then are the panels displaying changed fields of increments and differences of increments differing from the original version of the paper? I have no explanation for that myself, and I see none in the answers to the reviewers nor in the paper.

Regarding Figure 7, can the authors really be explicit and state in the paper whether or not the analysis increments for temperature systematically are positive when only one single BC observation is assimilated ? I actually see no reason for it being so, but I would like the authors to provide full clarity on this, based on their experimental results. If their answer is yes (this is systematic), an explanation why would be definitely welcome.

The statements in lines 748-750 are either too strong or totally obscure, and likely a bit of both. I simply cannot understand why the experimental results in Section 5 enable one to state that “meteorological observations can help constrain the uncertainty introduced by BC observations on atmospheric variables, thereby improving the reliability of the assimilation results”. My own strong reaction could be due to the fairly unclear formulation in the text:

why or how do BC observations “introduce uncertainty” on atmospheric variables ? What is meant by this formulation ? How is “reliability” defined in this context ?

Likewise, I actually dispute that “these results demonstrate the successful implementation of the newly developed CMA-GFS-AERO 4D-Var system”.

What is meant by “successful” here ? Should the authors wish to provide proof (a demonstration) of the successful implementation in terms of exactitude of expected results, then the only way would be to use their coupled assimilation system in a fully controlled modus operandi. One possibility would be OSSE with fully simulated observations of all types, and using prescribed error statistics in the system error covariances and for perturbing the simulated observations from a ground truth. In such experimental mode, the output results should follow some statistical results that separately could be derived from the expected posterior probability density functions (i.e. independently from the experiments).

As this is not what the authors have done, and I am not claiming they should do this, my understanding is that the authors have proven that their 4D-VAR system produces “credible and realistic-looking” results: “credible” in the sense that the results could lead to further explanations with the knowledge the authors have, and “realistic-looking” in the sense that the increments display realistic structures and amplitudes case-by-case.

I therefore strongly invite the authors to consider reformulating their statements at this location in Section 5 (around lines 748-750), and elsewhere where relevant such as in the Conclusion (lines 819-820).

I would recommend some English proofreading of the paper.

Specific Comments & Typos

line 408: “Specifically, if the BC observation is assumed to take place at the initial of the assimilation window,” => “Specifically, if the BC observation is assumed to take place at the **initial beginning** of the assimilation window,”

(same suggestion holds for other locations in the text such as for instance in the caption of Figure 7)

Figure 10 and everywhere in the paper where “pressure” is referred to: what is exactly this field ? is it “surface pressure” (ie following elevation), is it “mean sea level pressure” ? is it “pressure at the first model level” ? or any other definition of “pressure” ?