

REVIEW egusphere-2025-4098 (2025-11-10)

The manuscript titled “ PM2.5 Assimilation within JEDI for NOAA’s Regional Air Quality Model (AQMv7): Application to the September 2020 Western U.S. Wildfires” by Wang et al. with reference egusphere-2025-4098 is a valuable scientific contribution in operational aerosol assimilation, particularly for wildfire smoke events. Overall, it is very well written and provides a detailed investigation of the PM2.5 assimilation of AirNow and PurpleAir surface observations in AQMv7 for the US in September 2020. The spatio-temporal investigation of the influence on different forest lead times (1-24hours) provides interesting insights.

Overall, I suggest accepting the manuscript with the following mayor corrections:

General comments:

- A) My first general comment is on the specific purpose of this paper: For me, it is not clear whether the novelty of the contribution is about (1) the general implementation of an PM2.5 assimilation system for surface stations, or (2) just about the impact of additionally assimilating PurpleAir observations. While the text (title, abstract, introduction) suggest (1), some parts remain confusing in this regard:
 1. In l. 81-84 you mention other literature assimilating AirNow PM2.5 in the US. Please specify the distinctive novelty of this contribution compared to previous work.
 2. It is not clear in Sec.2 what the new developments are and what was already available before. Please state more clearly in the beginning of the section or for each subsection.
 - incl l.129: is this part of the new developments?, otherwise please add reference
 - incl l.159ff: (same)
 3. Along these lines, it is also not clear in the summary in Sec.5 if l. 565-572 is about new developments. Please specifically summarize the purpose and novel developments of this study in Sec.5 (see also general comment D).
- B) Sec. 4.1 contains plenty of plots which are only discussed shortly. This is fine, but I would suggest moving some less important plots in the Apx. (e.g. Fig.5, Fig.8) to increase readability. Or divide into subsections and consider expanding the description of results from each plot (eg temporal, spatial comparison, especially if Sec.4.2 is deleted/shortened: see general comment C)
- C) Sec. 4.2: Concerning the whole content of Sec.4.2: The only new result I can see in this section is l.538-541. All the rest seems to reproduce the results of Sec.4.1. I suggest removing this whole section, and maybe putting some of the plots in the Apx. (eg Fig.10 if the results in l.538-541 are considered important). Otherwise:
 - Please explain more clearly why you are specifically looking at the forecast initialized at 12UTC. You mention the operational 72-hour forecast, but only show results for forecast hour 1-24. What additional information does this investigation provide compared to the overall results above?
 - e.g. Given the fact that I don't know where Areas B and C are exactly (see smaller comment 15): What is the additional result of l.531f compared to Fig.7? Also l.533-537, l.542-544
 - Please restrict this section to specifying and discussing the novel results that cannot be obtained from Sec.4.1.
- D) Sec.5: I'm missing an actual summary and discussion of the results of this study. What conclusions do you draw from the results above? You show plenty of plots with different

aspects in Sec.4, I would wonder if the only important aspects you want to summarize and discuss are in l.573-584.

1. e.g. Do you have any explanation or conclusion from the temporal and spatial differences between the runs that you describe in Sec. 4?
2. The remaining parts of Sec.5 are about previous work (as far as I understand, l.565-572, compare general comment A:3) and outlook (l.585-608).

Smaller comments:

1. Sec.3 includes lots of different subsections. I would suggest giving a short overview of the content at the beginning of the Sec.3.
2. Sec.3.1: Please provide some more general information on the region that you're discussing, eg west/east? which state?... For a non-US reader it's not obvious where the Willamette Valley is. And please specify what you are referring to as "broader region"?
3. l. 234f: Which reference PM2.5 data did you use in your modified regression equation? Please specify.
4. l. 244-256: Please restructure this paragraph. If I understand correctly, you're switching between AirNow (l.245, l.247f, l.250ff) and PurpleAir (l.246, l.248f) multiple times. Please formulate more clearly.
 - o l. 247f: Does this refer to PurpleAir? Please specify.
 - o l. 248f: Redundant information with l.245.
 - o l.249 "5%": For comparison to your setup, please specify the type of observations they assigned with 5% errors, eg lowcost or "AirNow-like" stations?
 - o l. 253: Unit of "1.5" missing (should be the same as PM2.5 concentrations, no?)
 - o l. 254ff: Unclear, please specify where these numbers coming from. Is this a result from the spatial averaging to a 0.1deg grid? As far I understand, this can only reduce the effect of large PM2.5 observations if there are always multiple observations within one gridcell. Is this always the case? Please explain.
5. l. 259: Please specify the matched stations in Fig.1d. Does this refer to locations where both networks have closely located stations? If yes, which criteria did you use to define "closely located"?
 - o At the end of this subsection, it is also not clear why you are looking at the matched values in Fig.1d. What does it tell you? Is there any consequence you are taking from it? Otherwise, remove Fig.1d.
6. l. 289: Is the linear relationship the same for all 70 variables? Please describe.
7. l.308: inconsistent notation of innovation vector: here(l.308) bold d, Eq.2: non-bold d, Eq.9: non-bold d with subscript b and superscript o. Please unify or define differences.
8. l. 338: I would associate a "cutoff scale" this to be the distance at which the correlation function is "cut off", assuming zero correlation beyond that. But this is different from a correlation length that determines the shape of the correlation function itself. Please specify.
9. l. 374: Please specify what happens to the PM2.5 field at the initialization times. Do you initialize from the last analysis? Since you assimilation PM2.5 every 3 hours, what is the difference in PM2.5 between the assimilation times and the initialization times? Or does the initialization only apply to meteorological fields?
10. l. 383: Are these independent observations left out for the experiments that assimilate AirNow? And if not: How can the bias of DA_AN be worse than DA_ANPA (and DA_PA) during the first forecast hours, if compared agianst the same assimilated AirNow data (Fig.3)? Please discuss the implications of validating the different experiments with AirNow data concerning potentially temporally-correlated errors.
11. Fig.3: Please discuss why the bias of the joint assimilation DA_ANPA is closer to CTR than each of the single-obs. assimilations DA_AN and DA_PA. Please also discuss the change in

biases over time, i.e. increasing underestimation with forecast time which makes the single-obs. assimilations DA_AN,DA_PA being least biased for longer forecast hours.

12. l. 458f: This can hardly be quantified from Fig7. For the assimilation runs, it would be helpful to show their differences to AirNow validation observations. I would suggest e.g. removing their absolute PM2.5 fields (Fig.7c,e,g) and showing their differences to AirNow validation along with their differences to the control (keeping Fig.7,d,f,h).
13. l. 483ff: I assume you refer to mean reductions (domain-averaged)? Local reductions seem to be much larger and varying. Moreover, when you only discuss mean reductions, this does not fit to in the discussion on spatial distributions here (introduced in l.442f, summarized in l.487ff). Please either discuss spatial distributions OR
 - show only mean reductions in the figure (replacing Fig.8)
 - and move this paragraph where it fits
14. l. 491f: This was not mentioned above. From which plot do you see this? Please specify.
15. l. 517-522: Please write a bit more where these Areas A,B,C are located (eg states). For external readers, it's hard to guess just from the given information.
16. l. 525f: Is this a guess? Or how do you conclude this?
17. l. 579-582: The sentence suggests that these results from the given reference. But the reference is from 2023 while the case study is from 2025, that's not possible. Please clarify.
18. l. 582f: Now it's November 2025, I assume the data coverage did not change significantly within 2 months. Furthermore, you only use data from 2020 in this study. Is there maybe a mistake in the year (see also comment above)? Or I completely misunderstand this paragraph.
19. l. 585-595: Confusing. You talked about the results before (l. 573-584), and now afterwards about the implementation (l.585-595). I suggest restructuring.
20. l. 596-608: I'm not sure if you need to discuss these challenges in that much detail here. This work is about assimilation of surface observations, and in my point of view it's enough here to say that you are planning to include improved AOD assimilation into the system.

Technical and formulation-related comments:

- l. 27-29: That's a very technical sentence for the abstract. At this point, it is not clear what control variables are in this context. And how background error standard deviations scale to background state values. It looks like quite a lot of information was squeezed into one sentence. Is it necessary to be included in the abstract? If yes, please expand, otherwise I suggest removing here.
- l. 34: please explain the abbreviation “CONUS” once.
- l. 45f: Do you refer to the specific AQI of EPA, or in general to any AQI? Please specify.
- l. 179-183: Is there a square missing somewhere? The diagonal of a covariance is the variance, squared standard deviation. So Sigma should be the variance matrix, or it has to be Σ^2 in Eq.(5). (also in l.184ff).
- l. 198f: please specify. In that region (which region, see smaller comment 2)? Or in whole US?
- l. 240ff: Can this be seen from Fig.1b? Please add reference to plot.
- l. 257f: The two sentences introducing different plots in Fig.1a-b and Fig1.c-d are confusing, because they are disconnected from their descriptions/interpretations below. I suggest moving the sentences referring to Fig.1a-b (l.259-263) directly after mentioning Fig.1a-b here. And moving the sentence introducing Fig.1c-d down right before its description in l.263ff.
- l. 297-301: You are mentioning multiple times that background PM2.5 error variance is denoted as Σ^2 . That might be confusing. I'd suggest defining once, and using either words or symbol afterwards.

- l. 327: Technically, Fig.2 is not not PM2.5 space. This would be eg showing the background error standard deviation as function of PM2.5. Do you mean the PM2.5 background error standard deviation as (weighted) sum over all 70 variables?
- l. 389f: This sentence is doubled with l.383.
- Fig.4: Description of mean missing in figure caption.
- l. 422: two verbs (“presents shows”)
- Fig.5, 7, 8,: Icons and labels are very small and hard to see/read.
- l. 523: capital “F” in “figure”
- l. 593: remove “.” before reference
- l. 593ff: is an ensemble prediction system planned for AQM? Please explain or remove sentence.