Assimilation of ground based lidar and ceilometer observations of aerosols from the European E-Profile network into ECMWF's Integrated Forecasting System (IFS-COMPO, CY49R1)

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Response to reviewer comments 2

(Reviewer comment are given in blue, our response is rendered in black.)

The paper of Kahnert et al., describes the efforts being made to assimilate continuous operating ground-based lidars (in the framework of E-Profile) into the aerosol forecast model at ECMWF. Generally, the paper is very well written. The procedure and new developments for assimilation of this novel data are properly described. Furthermore, the effect of the assimilation efforts are discussed and show the clear benefit for the aerosol forecast (at ground wrt PM2.5 and PM10) when using ground-based profiling instruments. For this reason, I consider the manuscript as highly relevant and in general suitable to be published in GMD. While the main part of the manuscript deals mostly with a short overview of the assimilation efforts and the discussion of the results, the technical and also very interesting part is given in the several appendices. Due to the high relevance of the Appendix, I feel that its content gets a bit underrepresented if discussed after the main manuscript but if the authors like it can stay as is.

A similar issue has been raised by the first reviewer, Jeff Reid. We refer to our reply to his comment #2, in which we explain our rationale of collecting much of the details in an appendix. However, we will make an effort to address the reviewer's concerns without digressing too far from the main topic or obfuscating the paper's core message.

However, I have some comments which need to be tackled before the paper can be accepted. Even though they are more of technical nature, I consider major revisions as appropriate for the manuscript as the impact on the paper is severe. While I have not much to criticize in the major text part, most comments for the reasons stated above are for the appendix but also for almost all figures.

Attention: English language editing of the manuscript (spelling, grammar, sentence structure) is not provided anymore by Copernicus. But I did not check for this in detail during the review.

Two of our co-authors are native speakers.

Major issues

1. My major concern is about the quality of the figures. Partly they need to be completely overworked. In some figures, basically nothing is visible due to unfavourable scaling (x- axis, y-axis, z – axis). Partly, the labels are missing or way too small so that the figure is not self-explaining. All in all, it appears sloppy.

This aligns with comments from the first reviewer and will be adequately addressed.

2. Furthermore, one needs to homogenise the writing and style of the whole manuscript: Labels/captions in the plots start partly with capital letters partly not – please unify.

   Partly () and partly [] is used for units in the plots – please unify.

   Please also unify times in the manuscript (text and plots)

Agreed.

3. For an outside model world reader, it would be great to make clear what the difference between CAMS and IFS-Compo is and then also stick to it in the text. Currently, it is quite often mixed up.

The IFS-COMPO is a modelling system that can be set up in very many different ways, such as in terms of the data that are being assimilated. For CAMS we use one specific setup of the model. We will clarify this in the revised text.

4. In general, it would be great to provide some of the model specific explanations in a more understandable way for the general reader or at least give proper reference.

We appreciate this comment, because it complements some requests by the first reviewer. While the first reviewer asked for more technical details about the model (e.g. the spectral truncations in different loops of the minimisation tasks), the second reviewer asked for more basic explanations of data-assimilation jargon that we who work in this field take for granted. This is a reminder that potential readers of our manuscript will have very different backgrounds. We are inclined to mostly follow the second reviewer here and (a) take out details that are unfamiliar to non-experts, lengthy to explain, and not essential for understanding the paper (example: "spectral truncation T511"); and (b) explain or re-iterate explanations of technical terms that are essential (example: "analysis increment").

5. Could you explain why you assimilate CL(3)51 and CHMK differently? Could you do both simultaneously – if yes – why not done, if not – why?

We assimilate all three instrument types, as well as Mini-MPL, simultaneously. It is unclear to which part in the text this comment refers, but our best guess is that the confusion stems from the fact that we *validate* CL31 and CL51 instruments separately from CHM15k instruments (e.g. Figs. 4 and 5). This is simply because these instruments measure at different wavelengths (cf. Table 1). Thus Figs. 4 and 5 show different *spectral* optical properties, namely, the attenuated backscatter coefficient at 1064 nm and at 910 nm, respectively. It would be meaningless to compute a station average over all CL31/CL51 *and* all CHM15k stations combined. In the code we do account for the spectral dependence of the aerosol optical properties and of the gas extinction, see Appendix A.

Figures to be adjusted

The following figures need to be overworked:

6. Fig. 1: Quality of pixel image is poor, please provide higher resolution or vector graphics.

Agreed, we can provide this in eps format, if the journal accepts it.

7. Fig. 2 + Fig. 3: Is way too small. Almost no labels are readable. Please enlarge all labels/captions and consider splitting the Figure into 2 different ones.

Agreed. We will also redo these figures, following comments by reviewer 1, and add a selection of AERONET stations as well as vertical cross sections.

This will result in splitting up the figures and rearranging the panels. In the process, we will also enlarge the labels.

8. Fig. 4: Is hardly readable. Almost nothing is seen with the current color scheme – please change so that one can "see" something – also consider adjusting the z-scale (maximum at 5.353 – why?):

We will try our best to do some damage control here by changing the z-scale. However, we can neither change nor take responsibility for the colour scheme. We are all too aware that this colour scale will be testing the patience of the majority of readers. Our original colour scheme was substantially more readable, but it did not comply with journal policies, which forced us to change it. Of all options complying with the journal's requirements, this was the least of all evils. For details, please see our response to comment #13 of reviewer 1.

9. Fig 5: same as Fig 4, but also label not correct as it is not only CL51 but also CL 31.

Agreed, good catch.

10. Fig 6: same as Fig 4

Yes.

11. Fig 7: same as Fig 4 but also label not correct as it is not only CL51 but also CL 31.

Yes.

12. Fig: 8: No y-axis labels -> need to be added

Agreed.

13. Fig. 8; Bottom row plots: Labels to small, image quality poor: Please enlarge labels, enlarge image quality, and separate figures.

Yes, separating the figure will probably help significantly.

14. Fig 9: same as Fig 8.

Yes.

15. Fig. A1: Please change y-axis scale for two-way transmittance, e.g. from 0 to 1.2. Currently hardly anything is visible in e and f

The first reviewer proposed a logarithmic scale instead (see comment #15 of reviewer 1). We will go with the suggestion by the second reviewer.

16. Fig. A3: Please use SI units (micron is not appropriate). Please also plot the absolute values in the Figures.

SI units: agreed.

Plotting absolute values: In principle, we do not object to this suggestion. However, the reviewer also proposed in comment 24 below that we provide this information in a table, which would be redundant. We will only pick one of these two suggestions.

17. Figure B3: Is the x-axis in the centre plot correct?

Well spotted by the reviewer – We will make the change in the revised manuscript.

18. Figure C1: Please unify x-scale with other plots. Epoch time is not appropriate here…

OK, we can change that. But please note that the exact date is really insignificant in this context. The point is merely to provide a simple illustration of the different components of the mask, not to investigate any specific event on any specific date.

19. Figure C2: x-scales partly different (for the same day) and no label provided - thus hardly interpretable. Please also increase quality of image.

OK.

20. Figs. D1 + D2: see comment on Figure 4

Agreed.

21. Fig. D4: I am sorry, but I do not see nothing with the current color scale. It is simply not distinguishable. Please change color coding. Furthermore, you need to explain all abbreviations in the caption! It's partly just guessing what is meant, E.g. what is "def mask"? The conservative mask?!? Remember that you need to explain all abbreviations in the caption. Furthermore, please also check the labels, aod should be AOD, Rmse should be RMSE etc… please be consistent in the whole manuscript (and of course between figures and their caption).

Agreed. We assume that the controversial colour scale is the one for the PM10 Mean Bias, as the other plots are, in our opinion, quite discernable.

Specific comments:

22. Page 16: Lines 245 to 249: For curiosity: Are their plans to do so, e.g. assimilate profiling data at multiple wavelengths or with additional capacities (Raman, depolarization)?

There is work underway to assimilated ATLID/EarthCARE. Concerning ground-based lidar stations, see our reply to the next comment.

23. Page 16: Lines 257-260: There are also many other networks which either provide already or work on providing near-real time data. I think it would be worth reaching out to them to advertise your work. Furthermore, a clear conclusion on what is needed for assimilation side except near-real time would be great. For example, att. backscatter profiles are already provided by ACTRIS, EARLINET, AD-Net just to name some – all are also part of the WMO GAW Aerosol Lidar Observation Network (GALION) and I wonder why one could not simply use this data as well.

This is a good suggestion, and we *are* communicating with other networks through various channels. One challenge is that everyone seems to have a different notion of what "near-real time" (NRT) means. Therefore, it is essential that data providers who want their data to be used in operational forecasts be aware of what the operational centre means by NRT. At ECMWF the global CAMS model has two analysis cycles per day, ranging from 3:00 a.m. to 3:00 p.m., and from 3:00 p.m. to 3:00 a.m., respecitively. Our definition of NRT is that the data be available no later than 3 hours after the end of an analysis cycle. Example: A measurement performed at 2:59 p.m. can be included in the analysis cycle from 3:00 a.m. to 3:00 p.m. if it is available no later than 6:00 p.m. on the same day. And just to be absolutely clear: by a measurement "being available" we mean that it has to be received by us by that time. At the moment, networks such as EARLINET do not yet meet this criterium. But our contacts at these networks *are* aware of this.

24. Page 17, 282: Could you please list the values you use for the Lidar ratio and the mass mixing ratio for all aerosol species in IFS-COMPO – probably a table would be good for that.

    See our reply to comment #16.

25. A1: instead of using a median atmosphere for the correction of water vapor absorption wouldn't it be better to use the IFS output for this? I

guess everything is better than median profiles...and H20 is rather variable in time and space. Can you comment on this?

As explained in A1, we do not perform any radiative transfer modelling as part of the work presented here, we merely make use of the results of the Correlated K-Distribution Model Intercomparison Project, CKDMIP (Hogan and Matricardi, 2020). We hope the reviewer understands that we will not embark into a significant sidetrack that lies far outside the scope of this paper to discuss the rationale of the CKDMIP work. Readers who want to know more about that are referred to the CKDMIP reference (Hogan and Matricardi, 2020).

26. Page 19, 309 and 310: Why to only consider Ozone absorption at 532 nm and 1064 nm? Can you explain? It is not obvious at all from Figure A1.

Ozone optical depth is 3-4 orders of magnitude lower than that of water vapour; this is obvious in the 3rd top panel. At 355 nm it is also very low. The fact that Rayleigh scattering dominates at this wavelength has been explained in L 306-307.

27. Page 20, 331-33: What are adjoint tests + operator and tangent linear model --> for me it is not clear what is meant with this sentence. Please add some words.

    The observation operator has been introduced in L 56-57, and the tangent linear approximation of the observation operator is introduced in L 62-63 and the text accompanying Eq. (1). We are aware that this is a very concise summary of our assimilation system. But it is far beyond the scope of this paper to provide a comprehensive introduction to variational data assimilation. However, we can add a definition of the adjoint operator and the equation stating the equality of two scalar products, on which the adjoint test is based.

28. Page 20, 333: What is CY50R1? Never explained. I assume it is the successor – but this is just an assumption....

    The versions of the IFS are called cycles, and they are consecutively numbered, such as CY48R1, CY49R1, CY50R1, etc. We can add a brief explanation.

Page 20: A2:

29. I really appreciate the efforts to account for the non-sphericity of dust particles. And obviously, everything is better than using Mie theory to calculate optical properties for dust. However, I think it is worth noticing

in the manuscript that the spheroidal model cannot accurately reproduce the backscatter coefficient and thus also the lidar ratio. I therefore have my doubts that the calculated lidar ratios (which I do not know as just the ratio to the spherical model is provided) is in the range with observations. Could you comment on this and also provide the lidar ratio values you finally use (i.e. give absolute values for what you show in Fig A3).

Yes, we will give the lidar ratios — see our response to comment 24. Concerning the spheroids, there is not even a guarantee that the specific spheroid model we used (prolate spheroids with an aspect ratio of 1.6) is the best possible choice of a spheroid, let alone whether spheroids are a satisfactory choice of model. The issue with nonspherical particles is highly relevant and fascinating, but in the context of this paper it is yet another rabbit warren that would take us, if properly discussed, on a detour that can easily get out of hands. We will, therefore, consider adding a brief remark in the revised paper, but we will keep it short.

30. Sec C1: Why do you use these hard boundaries for the height range top in your lidar data? Given your noise mask described further below, I would assume that such strict cut offs at 3 km or 5.3 km are not necessary....can you comment on it?

    We answer points 30-36 together under point 36.

31. Sec C1: The threshold for the cloud and precipitation masks are rather low, but might be justified due to the strong attenuation in clouds and precipitation. How did you select the thresholds? Empirically? Please comment.

    We answer points 30-36 together under point 36.

32. Page 23:399 -400. Basically, it means that for the precipitation mask, you use just the height range from 500 m below cloud top to 1000 m below cloud top, correct? I.e., you screen out data even though the precipitation might not reach the ground. Correct? Can you discuss why?

    We answer points 30-36 together under point 36.

33. P. 23, Fog mask: Given Eq. C5, you might also screen out nocturnal or wintertime shallow boundary layers with a polluted PBL and clear sky above. Would this have a significant impact on your PM modelling? In fact, this threshold-based method might screen out too much?

    We answer points 30-36 together under point 36.

34. Figure C1: Second panel: In the first ~9 hours of the day, the top of the PBL is frequently misclassified as cloud. Can you comment/discuss? How do such effects affect your PM2.5 and PM1.0 simulations at ground?

    We answer points 30-36 together under point 36.

35. Figure C1: Third panel: the rain mask also affects many regions which are well below cloud boundary and most probably not affected by rain. For this reason, you lose a lot of data which might be interesting for assimilation as well. Can you comment on this? Is this by purpose?

We answer points 30-36 together under point 36.

36. Figure C1: Last panel: The noise mask plot shows some “vertical” stripes below the cloud reaching down to the ground. Why does this happen? Can you explain/discuss?

The vertical cut-off and masking method has been developed by co-author Mikaël Bacles at Météo-France, who kindly shared his findings with us prior to publishing them. We are only applying his method. Our own humble contribution here was merely to investigate the effect that the ordering of the masking and averaging operations has. Figures C1 and C2 have the *sole* purpose of briefly illustrating the mask, then discussing the effect of that operator ordering. We are reluctant to embark on a lengthy sidetrack on the details of the vertical cut-off and masking methods, as the reviewer wants us to do, for a number of reasons. First, the development of the masking method is not the subject of this paper; only our proposition of choosing a specific ordering of masking and averaging operations is original in this paper and is tested and discussed adequately. Second, the masking approach is the result of significant development work, which we could not do justice in such a sidetrack. Third, we also consider it fair to leave it to the main developers of the masking method to publish their results (which are part of a PhD thesis) and not to preemt them. A publication is presently in preparation and will be available shortly.

However, we understand the reviewer’s curiosity and the need to refer to an existing documentation of the masking method. Therefore, we will add a citation to the recent report of the European Union’s Horizon project “CAMS EvOlution” (CAMEO), in which the masking method has been presented and more background information can be found (Vincent Guidard, Mickaël Bacles, Nicolas Frebourg, Guillaume Monteil, Andreas Uppstu, and Jeronimo Escribano. "D3. 2 Report on satellite lidar, ceilometer and AOD assimilation."; https://www.cameo-project.eu/sites/default/files/2026-01/CAMEO-D3-2-V1.1.pdf).

37. Page 32, 528 to 529: “for reasons already mentioned above” This is not understandable. To what reasons do you refer to? Please discuss more intensively.

We refer to two places in the text where this has been discussed before, L 195ff and L 218ff. We will add a remark to remind the reader.

38. Section D5 could properly be included in the main body of the manuscript.

This is an appealing idea. It can be interesting even for a broader readership to understand that the system setup used in the main body of the paper has emerged from testing many different setups. However, it would require to add more explanations of data masking, optics models, and background errors to the main body of the paper. This may be worth the expansion though. We will take this suggestion under advice and weigh its pros and cons.

Some technical comments:

39. Page 2, Line 28: This sentence is a bit sloppy, please provide a correct statement what ceilometers and lidars are. E.g. it’s not the laser power backscattered but parts of the light pulses emitted by a laser into the atmosphere…

OK, we will improve this sentence.

40. Page2: Line 41: “Spectral truncation T511” never introduced. What is this?

We will remove this technical detail. For the majority of readers it will suffice to mention the (approximate) horizontal resolution in degrees that corresponds to the model’s spectral truncation.

41. Page 5, Line 117: What is “unwanted data”. This sounds a bit strange, please reformulate or explain better…

We will replace it by “clouds, precipitation, fog, and noisy data”.

42. Page 8, Caption Figure 2/3: What is “analysis increment”? Please explain.,

This has been explained in L 64 and Eq. (3). We will remind the reader by adding “analysis increment [cf. Eq. (3)]”.

43. Page 14, 204: Shows --> shows

Yes.

44. Page 26, line 458. “Earlinet” was never introduced nor referenced. Furthermore, some more information is needed here about what exactly has been tested.

We will remove this from the text, as it is not essential. Also, there are no publications from these EARLINET assimilation tests at ECMWF that we could refer to. So, it’s best to delete this text in the revisions.

45. Fig. D3: What does “time: 3” stand for? 3 UTC? Please state more correctly so that it is understandable for an outside reader.

Yes, all times are UTC. The Figure tops are automatically generated by the program that does the data processing (including masking) and stastistical analysis. Since most if not all of those headings and axis labels come out too small, we will redo them.