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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assimilation of ground versus lidar observations for PM_{10} forecasting" *by* Y. Wang et al.

Anonymous Referee #2

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Referee report on paper by Wang et al Assimilation of ground versus lidar observations for PM10 forecasting

The article compares the efficiency of ground-based and lidar observational networks in Europe. The criterion of the network quality is the possibility to constrain the atmospheric dispersion models via data assimilation of the observations. This criterion is quite well known as a way to quantify the added value when adding a site to existing network or while designing a new one. In the current study, the authors based their analysis on existing AirBase network for in-situ data and extended LEONET lidar network.

In general, I found the paper interesting but difficult to read. It also lacks the strong message, which was evidently expected by the authors when the work was started.

The efficiency of extended lidar network appeared to be comparable or worse than that of already existing AirBase. The difference of 2% in the RMS reduction at the second day of the forecast is evidently insignificant. Intuitively, the opposite should be true: the possibility to assimilate 3D fields should increase the model scores quite a lot. It did not realize in the current experiment, probably because of very few lidar sites. However, extension of this network is costly. Therefore, demonstration of sufficiency of AirBase network for data assimilation tasks is probably the most interesting conclusion of the paper, which is bound to raise further discussion. Therefore, I favour the publication providing the below comments are taken into account.

Specific comments

Introduction. L.36-40. This sounds as a biased statement: data assimilation is quite efficiently used in the AQ world by many groups, both in research and, since recent, in operational forecasting. PM10 is indeed not heavily addressed yet – for many reasons not discussed in the paper. However, limitation to PM10-only narrows the review and excludes the main groups routinely working with data assimilation in Europe and US. I suggest to give a broader outlook here.

L.60-65 and elsewhere. Why lidars for PM10? One would argue that lidars are more sensitive to PM2.5 since the surface-to-mass ratio for fine particles is higher. Also, actual aerosol size spectrum seems to favour finer particles, which provide the main surface-wise contribution. No such discussion is given but a coupe of sentences would be useful.

L.167-175. These equations are trivial and unnecessary in the paper body. They should either be moved to annex or eliminated.

L176-178. I did not understand. Two sets of criteria are given with the same statement that if they are satisfied, the model performance goal is met. So, which of the sets is the right one?

L.178-182. And again something strange: the statement construction suggests that the PM2.5 and PM10 scores are different but the actual wording says that in both cases the criterion is met. That should be rewritten. And why PM2.5? It is not the goal of the paper, neither it is mentioned elsewhere.

L.184. The opposite is true: accuracy of the twin run is of high importance because its synthetic measurements must reproduce the real pattern and real situation. Otherwise conclusions on the network design and quality will not be applicable for real life.

L.203. “as now explained” should be removed.

L.211. Please explain how the error covariance matrix “sigma-capital” is obtained from the function f(d_v,d_t). Only after that one can proceed into Cholesky decomposition of this matrix.

Eq.7. Please state the features of gamma clearly: normal distribution requires mean and variance to be fully defined.

L.236. It is a hand-waving justifying an easy-to-do but not rigorous approach. I agree that over-estimation of the DA efficiency will be for both in-situ and lidar networks – but will it be overestimated to the same extent? The features of the datasets are much too different to claim that. For instance, initial conditions are “forgotten” faster near the ground than aloft – and (in)correct emission/deposition at/near the surface will overwhelm the impact of transport or transformation representation, which are the main players higher up. In that sense, “ideal” model choice will probably favour lidars more than ground stations. Some discussion of this kind is needed here.

L.258-259. Did not understand the sentence. Shouldn’t it be just removed?

L.261-262. Why the first model level?? It is wrong in general and directly contradicts to the section 5, which suggests much more rigorous approach. The whole paragraph seems to be forgotten from some historical text edition.

L.270-277. Standard deviation instead of variance would be better here: more intu-

itively understandable numbers

L.285-286. An empty sentence, which seems to have no connection to the section. Please remove.

L.306. This is strange. Why would the horizontal correlation length decrease in the free troposphere in comparison with ABL? A common knowledge is that the fields are much smoother in FT, so should be the errors, shouldn’t they? A short discussion is needed here.

Section 6. Here the problem shows up, owing to the above-criticised choice of the ideal-model twin run setup. The selection of exactly the same model for twin, control, and DA runs, combined with perturbation of only the initial conditions, lead to reduction of the model error with time in all runs. Indeed, with influence of initial conditions fading out with the forecast length, all runs fall to the same main path, i.e. become non-distinguishable from each other. This is why RMSE reduces and the correlation coefficient grows in all runs all the time. The role of DA is only to make it faster. Such discussion is entirely missing. A reader without deep experience with OSSE is left guessing why RMSE continues to decrease also outside assimilation window, whereas in all real applications it will immediately start growing. Explanation should be added and also related to Figure 8.

L.364-365. I see no “powerful impact” whatsoever – the impact of those networks is practically identical. Those few % of difference to either direction are well inside the method uncertainty and simplifications of the setup. The strongest statement possible here is that those networks lead to a similar effect, despite 40-fold smaller number of lidar sites. However, the costs can be comparable – or higher in case of the lidar network.

Section 7. I am strongly lacking the sensitivity run with 9 existing lidar stations. Comparison of this, already existing lidar network, with already existing AirBase would be very useful for practical applications. I would advise to do this exercise.

L.381 and elsewhere. The word “global” should not be used in this context: improving European network does not lead to better global forecasts. I guess, the authors meant “in-general” or “overall” in all such occasions.

L.396. I would remove the word “strongly”. With all the simplifications in the setup and very similar scores against AirBase there is no reason to make so bold statement. Actually, the boldness of the message is lowered already by the next sentence, so let’s be consistent.

L.408-415. This paragraph is neither understandable nor adds anything to the conclusions. Please remove.

General remark. Please do not provide too many digits. For instance, out of three digits in 56.1% improvement, maximum 2 are really known, i.e. 56% should be shown.

Figures.

Figure 4 is unreadable. Figure 5 is unreadable. Figure 8. Instead of empty names “text X” in the legend, a meaningful abbreviation would be very helpful. For instance, instead of “test 1”, one can use “AB 50km 1500m”. That would dramatically simplify the analysis of the very busy pictures. Also, the range for correlation coefficient should start from 0.5 or 0.6 to make the lines distinguishable. Figure 12 and 13. Empty names “Network X” should be replaced with meaningful abbreviations, e.g. Net.YY, where YY is the number of sites.

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