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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluating model performance of an ensemble-based chemical data assimilation system during INTEX-B field mission" *by* A. F. Arellano Jr. et al.

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We sincerely thank the referee for the comments and recommendations, which we find to be very useful in clarifying and improving the manuscript. We describe below our response to the comments made by the referee. We also revised the manuscript to incorporate these changes.

Referee Comment 1

I don't particularly see the value of Fig 15. All it seems to say is that mean forecast RMS errors are always larger than analysis errors. For conciseness it seems the Figure could be omitted without significant impact to the paper.

Authors Response

We omitted Figure 15 as suggested by the referee. In addition, we edited the text in section 4.5.2 to reflect this change. We now refer Figure 15 to Figure 6 instead. Also, we changed Figure 16 to Figure 15.

" To illustrate, we took the 6-hourly ensemble-mean forecast CO and the analysis CO in the observation space and averaged over the INTEX-B domain (from equator to 65N latitude, 90E to 90W longitude). The resulting time series of the spatially-averaged observation increment describes the evolution of the forecast and analysis errors relative to assimilated MOPITT retrievals. Similar to the global time series shown on the left panel of Fig. 6, the mean forecast RMSEs are systematically larger than the analysis errors. This difference represents the adjustment made in the analysis step to bring the modeled CO closer to observations. We can also take the temporal mean of these increments and look at its spatial distribution to explore characteristic regions where the modeled CO is frequently adjusted. Figure 15 shows the spatial distribution of the mean adjustments about the April 6-May 1 2006 period. "

Referee Comment 2

The introduction and discussion of Taylor diagrams (section 4.2.2 and Fig 9) needs to be improved. I have made guesses as to certain details of Fig. 9, elaborated below. If my guesses are correct I suggest minor clarification and if incorrect, more significant clarification is needed. (1) Definition of " sigma " (horizontal axis). I assume it simply refers to concentration, but also seems to apply to RMSE. (2) Definition of the iso-lines on Fig 9a (labeled 0.2 ..1.2). After studying your text I assume this is the skill. If so, what is the significance of values larger than 1? (3) If I understand what sigma is, I think I understand how the model predictions are plotted in (sigma, R, skill) space. I don't understand how the total RMSE points (large open circles) are plotted or more precisely, what (if any) the relationship is to R and skill values.

Authors Response

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We edited the first part of section 4.2.2 to introduce and discuss the construction and interpretation of the Taylor diagram. We find that the Taylor diagram is well suited for our analyses, since our comparisons involve the whole 20-member ensemble. The sigma in the horizontal axis is defined as standard deviation of the observed CO concentration. The other sigma is the standard deviation of the modeled CO concentration (in our case, for each ensemble member and its mean). From this plot, the RMSE is inferred, as the distance of the obs quantity to the overall model quantity (including the bias). We described this in the text and include 2 relevant equations from Taylor (2001).

"Results are shown for both the experiment with MOPITT assimilation (COASSIM) and without MOPITT assimilation (REFSIM). Based on Taylor (2001), a typical RMSE metric (E) can be decomposed into two orthogonal components, a bias term ($\bar{E}$) and a pattern RMS difference (E'). That is,

$$E^2 = \bar{E}^2 + E'^2$$

where

$$E'^2 = \sigma_f^2 + \sigma_r^2 - \sigma_f \sigma_r R$$

such that σ_f and σ_r correspond to the standard deviation of the modeled CO concentration and the observed CO concentration, respectively; and R corresponds to the correlation between the modeled and observed CO concentration. We can then construct a diagram which shows these relationships, by plotting σ_f and σ_r as distances from the origin in a polar graph (see Fig. 9b), with the angle represented as the $\arccos$ R. The shortest distance between the modeled and observed quantity represents the pattern RMS difference (or unbiased RMSE). We can also extend this to indicate overall means, by attaching to the plotted model quantity, a line segment that is perpendicular to the line defined by E' and whose distance is equal to $\bar{E}$. The distance from the obs quantity to the extended model quantity is then equal to E. A best fit to observations is typically interpreted to exhibit the smallest pattern difference. However, we can also define a skill score which summarizes the relationship between the modeled and

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observed CO concentrations. Here, we defined a skill S as:

$$S = \frac{4(1+R)^4}{(\hat{\sigma_f} + \frac{1}{\hat{\sigma_f}})^2(1+R_0)^4}$$

where $\hat{\sigma}_f$ is the ratio of σ_f and σ_r, and R_0 is the maximum potentially realizable correlation, assumed here to be 0.90 based on assimilated CO and MOPITT comparisons. This skill score, which is based on Eq. 5 of Taylor (2001), is superimposed as contours in Fig.9a. Unlike typical skill scores, this skill score places more emphasis on modeled CO that is highly correlated with observations and that exhibits better simulation of the pattern difference. The skill approaches unity as σ_f approaches to σ_r and as R approaches to R_0. It is also interpreted to decrease with increasing RMSE but with additional penalty for low correlation and low model variability. Note that the skill also depends on maximum realizable correlation R_0, which in this case, is assumed to be lower than a perfect correlation of 1. This means that a perfect skill score (or most skillful model) will have a range from 1 to about 1.2 since it is relax towards achieving a correlation of R_0 as well. ”

Referee Comment 3

P.9718 line 22: further demonstrate (not demonstrates)

Authors Response

This has been changed to “further demonstrate”

Referee Comment 4

P.9720 line 11: do you mean pollution (control) strategies?

Authors Response

Yes. This has been changed to “ pollution control strategies ”.

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Referee Comment 5

P.9725 line 11: not clear what you are referring to as “(initial assimilation)”.

Authors Response

We deleted our comment “(initial assimilation)” to avoid confusion. The comment however pertains to the first assimilation cycle.

Referee Comment 6

P.9728 line 11: sentence fragment “observations <from the> MOPITT..? ”

Authors Response

This has been changed to “observations from the MOPITT ” .

Referee Comment 7

P.9730 line 16: need additional info regarding this paper (i.e., citation, submitted, in preparation). If “ in preparation ” , change text to “ will be discussed ”.

Authors Response

We changed the text as suggested by the referee.

“ Details on the scale mismatch between satellite retrieved profiles and global chemical transport models will be discussed in a separate paper (Arellano, A.F., and P.G. Hess, Ensemble-based estimates of CO sensitivity during INTEX-B field mission, manuscript in preparation). ”

Referee Comment 8

P.9732 line 23: “ subset of NCEP data ” is not clear. Do you mean subset of the observations used in the assimilation.

Authors Response

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This has been changed to: “ given also that only a subset of NCEP observations was used in DART/CAM assimilation. ”

Referee Comment 9

P.9735 line 14: “at 700 hPa <is> translated ”

Authors Response

This has been changed to “ at 700 hPa is translated ” .

Referee Comment 10

P.9735 discussion of Fig 10: In this qualitative evaluation it is important to acknowledge that assimilation sometimes results in worse prediction. Specific clear examples include the DC8 comparison, observation points approximately 1550 and 2100.

Authors Response

We added this important comment in the text:

“ The plots further show that MOPITT CO assimilation better captures the observed variability and improves the modeled vertical structure relative to INTEX-B observations, particularly for C-130 CO. We note however that the assimilation sometimes results in poor model prediction, for example, in DC-8 CO comparisons (Fig. 10) during a flight at the end of April (i.e. points from 1500 to 2100). ”

Referee Comment 11

P.9735 discussion of Fig 11: You note that bias correction applied to the assimilation results, which begs the question of biases inherent in REFSIM results. I think it would help pint out that, given the differences between observed and REFSIM vertical profile shapes, a simple bias correction for REFSIM would not significantly influence your findings.

Authors Response

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We agree with the referee. We modified the text to make this point.

“ This can also be seen in the averaged vertical structure (Fig. 11), expressed here as median vertical profile, where COASSIM CO is better correlated with the observed vertical structure than REFSIM CO. Given the difference in the shape of the observed and REFSIM vertical profiles, applying a simple bias correction for REFSIM would not significantly influence the improvement we find in COASSIM CO relative to observations. ”

Referee Comment 12

P.9736 discussion of Fig 12: The differences would be more easily seen (and perhaps described) if you added a difference plot as a third panel.

Authors Response

We agree with the referee. We modified the figure to include the difference plot.

Referee Comment 13

P.9736 line 7: “ not accurately represented in the REFSIM. ” Rather than REFSIM, this should say “model ” or “ both simulations ” as any CO source and transport deficiencies are essentially the same in your simulations. The CO assimilation is presumably helping to compensate for these inherent deficiencies.

Authors Response

We fully agree with the referee and changed from REFSIM to model.

Referee Comment 14

P.9736 section 4.4: Suggest changing the same to “ Illustration of CO forecast sensitivity”

Authors Response

We changed “ CO forecast ” to “ Illustration of CO forecast sensitivity ” as suggested

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by the referee.

Referee Comment 15

P.9738, lines 1-2: " ensemble spread still appears to be under-estimated " . On what do you base this statement? A most interesting aspect of Fig 14 concerns where larger differences are found in panel (b) than in panel (a). I speculate that what you are trying to say is that this situation indicates where the CO error estimates are too small. If so, you should clarify the text, -and if not, I have missed the meaning, and you should clarify the text.

Authors Response

We clarified the text. The statement " ensemble spread still appears to be under-estimated " is based on a comparison of the RMSE and ensemble spread. The spread, which reflects the uncertainty of the posterior mean is lower than the RMSE in our assimilation. We agree with the referee on the point of Fig 14. We modified the text to make this point clearer.

" To an extent, the COASSIM ensemble spread is an improvement in representing the structure of the uncertainties in the CO distribution since it appears to represent additional features other than emission uncertainties. However, when compared to the model RMSE relative to MOPITT (Fig. 6), the posterior ensemble spread still appears to be under-estimated, especially in the downwind region where the variability is mostly transport-induced. Such differences in the structure of the ensemble spread between COASSIM and REFSIM demonstrate the spatial variability of the error estimates and point to locations were the errors are apparently too small. For this reason, a more appropriate characterization of the uncertainties as estimated from the ensemble approach is to take advantage of the structure by conducting an analysis of the correlation in conjunction with an analysis of the variance. "

Referee Comment 16

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P.9738 line 15: “ forecast diverge(s) from ”

Authors Response

This has been changed to “ forecast diverges from ” .

Referee Comment 17

P.9740, line 24: The statement made in the first sentence is only true given an independent set of measurements ie, the aircraft insitu data. I would suggest rewriting as “ Constraining CO using the assimilation system , and evaluating against independent measurements, provides important ”

Authors Response

We full agree with the referee. We rewrote the text as suggested by the referee.

Referee Comment 18

P.9735-9738, section headings: You use symbol notation in these headings that does not appear anywhere else in the manuscript. You should either omit them, or if important define them in the text and/or a table and make use of them in the discussion.

Authors Response

We omitted the symbol notations.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9717, 2007.