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Comment

Interactive comment on “Assimilating remotely sensed cloud optical thickness into a mesoscale model” by D. Lauwaet et al.

D. Lauwaet et al.

dirk.lauwaet@ees.kuleuven.be

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We are pleased that both reviewers indicate that our study is interesting and worth publishing in Atmospheric Chemistry and Physics. We have tried to address all of the reviewers' comments with particular attention to all of the major issues. The most important issues that were asked for were the addition of validation data, an in-depth look at both the non-diagonality of the background error covariance matrix and the error on the background saturated specific humidity, and an assessment of the model sensitivity to the parameters that are related to the standard deviation on the total water content and the observed cloud optical thickness. Regarding the validation data, we have added two more observation stations (Bordeaux and Oensingén) that are both in the regions where the assimilation scheme has the biggest impact, and a comparison

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is made to total column water vapour from radio-soundings at Trappes. Both the non-diagonality of B and the error on the background q_s are addressed in extra Appendices where these issues are looked at in depth and their effect on the assimilation scheme is tested on an observed radio-sounding profile. Furthermore, a sensitivity study on the model parameters a and b is added to the manuscript. We believe the applied changes significantly improve the scientific value of the manuscript and hope they make it acceptable for publication in Atmospheric Chemistry and Physics.

Reviewer 1 Specific comments and technical corrections

* Page 13356 line 9: Actually, what is implicitly assumed is that the background error variance in the horizontal is infinite, or that the horizontal structure is unknown. This is equivalent to an independent pixel approximation where at each pixel the cloud information is retrieved independently from the other points in the satellite field of view. This assumption is made by most satellite retrievals. Please reword this sentence to make the point clear.

The sentence is adapted as suggested by the reviewer (P 2 L 10) and also in the conclusions on P 18 L 16.

* Page 13357 lines 14-15: This is actually incorrect. The systems that the authors mention are used in operational applications and are therefore rather computationally efficient. Maintenance of the adjoint codes is largely independent of resolution, apart from occasional retuning of the linearized cloud schemes used in the analysis systems. It would be more appropriate to say that only centres with access to large computing facilities can afford to run and maintain these type of systems while research groups in smaller centres and universities need to resort to other techniques for computational feasibility.

The text is adapted as suggested by the reviewer (P 3 L 21) and also in the conclusions on P 18 L 17.

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* Page 13358 line 8: could the authors elaborate more on this - why can ADAS only handle cloud profiling data?

ADAS needs information on the vertical extent of the clouds to estimate cloud types and related in-cloud vertical velocities which can not be derived from our two-dimensional cloud optical thickness data. This is also mentioned in the text now (P 4 L 19).

* Page 13358 line 27: Replace 'To' with 'The'

The text is adapted as suggested by the reviewer (P 5 L 19).

* Page 13360 line 16: Please add 'The subscript b denotes the background fields'. In general, given that there are many symbols introduced in the paper, please make sure that they are all defined.

This sentence is added to the text (P 8 L 1). Furthermore, in the revised manuscript, a list of all the introduced symbols is provided in Appendix A (P 21).

* Page 13361 line 13: The assumption of a diagonal B matrix is very common. How much more work would be involved if you tried with a simple non-diagonal B-matrix, for example an exponential decay for correlations with a prescribed length scale to be determined with sensitivity studies?

The assumption of a diagonal B matrix is indeed a simplification of the method. It is not straightforward to implement a non-diagonal B matrix in the operational assimilation scheme, but we have performed an off-line test of the scheme with a non-diagonal B matrix that uses an exponential decay of the correlations with a decorrelation length of 2 km, corresponding to the value used by Zupanski et al. (2005) for the vertical decorrelation length of total water mixing ratio in their study on vertical correlations in error covariance modelling. The result of this test for a radio-sounding profile at Trappes is added to the paper (Appendix B P 22), so the impact of this assumption can be assessed by the readers.

* Page 13362 line 11: When a layer contains no simulated condensed water qc_{bi} then

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there is no information on how much that layer contributed to the cloud optical thickness. This is a common 'problem' when retrieving profiling information from integrated measurements, in the sense that the vertical distribution of the retrieved quantity is entirely dictated by the background. By assigning a cloud variance to non-cloudy background layers as a function of saturation and total water content, the authors are implicitly allowing for the analysis to adjust the water vapor profile in absence of background clouds.

This remark is true and it is now explicitly mentioned in the text (P 10 L 19).

* Page 13363 lines 15-19: These are strong assumptions - have the authors thought of investigating the sensitivity of the analysis to the choice of these parameters? As a general comment, it is common practice in the retrieval community to provide retrieval products, for example cloud optical depths, and not to provide pixel-level uncertainties. While for other applications this might be perfectly fine, from an assimilation perspective this is rather poor, because the data users are left with the task of arbitrarily assigning errors at the pixel level, and that can lead to severe errors in the analysis.

It is true that the choice of these parameters has some effect on the analysis. In this case, these values are based on evaluation data (specific humidity profiles at Trappes and Cloudsat COT data, respectively) as is indicated in the text now (P 12 L 15). Furthermore, the sensitivity of the analysis to these parameters is investigated by varying them between 0.1 and 0.5. The results for the station of Melun are presented in Section 4.2 (P 18 L 2) and Table 4 (P 32). It is clear that they have a significant effect on the outcome of the analysis and should be chosen carefully.

* Page 13365 line 7: For those readers not familiar with cloud retrievals it might appear unclear why the assimilation is only active during the day. Please explain.

A short explanation is added to the text (P 14 L 14).

* Page 13365 lines 25-30: Would having correlations in B help with this?

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No, having correlations in the B matrix will only affect the distribution of the assimilated cloud water, as can be seen in Figure 9 (P 42), but the layers still would need to be brought to saturation to keep the clouds in the model.

* Page 13366 line 18: It would be good to have some verification results for stations in that area where the assimilation had the biggest impact. Are there any stations available that the authors can use?

The authors have gathered data from two more stations (Bordeaux and Oensingen) that are in the areas with the biggest impact (SW France and the Alps). These results are added to the revised manuscript and help to better assess the impact of the assimilation scheme.

* Page 13366 line 25: Comparison with assimilated data can only be used to check the first guess, and to see how the assimilation fared. The authors need to use independent data to assess the performance of their system. Please add one or two figures in which the cloud data are quantitatively assessed with, for example, MODIS and/or MISR optical depths. That can also give a measure of the quality of the COTs derived from the SEVIRI instrument (also a way to independently check that the error assumptions reported on page 13363, line 18 are reasonable).

To have an independent validation of the COT's from SEVIRI and the assimilation, a MODIS COT image is added to Figure 8 (P 41), as asked for by the reviewer. Clearly, the position of the clouds compare very well between both instruments, although the mean value of the MODIS image is 18 % higher than the SEVIRI image.

* Tables: Please report temperatures in Celsius. It is more intuitive when speaking of surface temperatures.

The Table is adapted as suggested by the reviewer (Table 1 P 29).

* Figures 3 and 4: The x-axis unit is odd - please use days. For clarity, it would be useful to have another plot with the zoom over June 15–17, for example, where the

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impact of the assimilation was the largest.

The values of the x-axis are changed to Julian days, as suggested by the reviewer. We believe these Figures are clear without a zoom, but if the reviewer insists, a zoom window can be added.

* Figure 6: Some verification in the areas of biggest impact (South-West France and Alps would be welcome.

The authors have gathered data from two more stations (Bordeaux and Oensingen) that are in the areas with the biggest impact. These results are added to the revised manuscript and help to better assess the impact of the assimilation scheme.

* Figure 7: Cloud fields need to be assessed more quantitatively using independent observations.

A comparison to the MODIS COT image is added to Figure 8 (P 41) as suggested by the reviewer.

Reviewer 2 General corrections

* In Section 3.3 of the paper the model background cloud water is derived from q_t and q_s ($q_{sat}(T)$). The error in this derived value of cloud water will thus depend on the errors in the model background values of q_t and q_s (the errors in q_s will, in turn, depend on the model background error in temperature, T). The contribution to the error from q_s is ignored in the paper, effectively assigning zero to these error terms in the Bmatrix (and hence not providing any feedback from the observations onto the model background temperature field). This approximation greatly simplifies the assimilation procedure, but it needs to be explained clearly to the reader, and the likely implications of ignoring the error in q_s need to be discussed.

It is true that in the original assimilation scheme, the contribution to the error from q_s is implicitly set to 0. This is also mentioned in the text now (P 12 L 4). To assess the implications of this approximation, the authors have derived an alternative formulation

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for the error variance of simulated cloud water, taking into account the error on q_s . These formulas are presented in Appendix C (P 23-24) and they are used in an offline test with an observed radio-sounding profile at Trappes (Figure 10 P 43). For this test, the error on q_s is set to 20 %, which corresponds to an error on the background temperature of 3 K (equal to the RMSE of background surface temperature in Table 1). The results indicate that the difference between both formulations is small and it is not likely to have a significant effect on the results of the assimilation scheme.

* In Section 3.4, the errors associated with various meteorological fields are assigned. An explanation must be given as to why a value of $a = 0.3$ is reasonable [explaining why it is at least approximately correct] based either on simple arguments or some example data.

The value of a is obtained from a comparison between modelled and observed specific humidity profiles at Trappes, as more than 80 % of the observed data points are within this error margin of the simulated profiles. This is also mentioned in the text now (P 12 L 15). Furthermore, the sensitivity of the analysis to the parameters a and b is investigated by varying them between 0.1 and 0.5. The results for the station of Melun are presented in Section 4.2 (P 18 L 2) and Table 4 (P 32). It is clear that they have a significant effect on the outcome of the analysis and should be chosen carefully.

* In Section 3.4 the cloud liquid water and cloud ice amounts in the model are modified. An explanation is required as to how the additional cloud ice and water is fitted into the Lin et al hydrometeor scheme.

As mentioned in the text (P 13 L 5), the old cloud liquid (q_c) and cloud ice (q_i) values in the model are replaced by the resulting analyzed q_{ca} value, which is entirely assigned to q_c when the temperature is above -10°C and to q_i when the temperature is below -30°C . For the temperature range in between, q_{ca} is divided between q_c and q_i applying a linear ramp between both temperature thresholds.

* At the end of Section 4.1 it is stated that “This can not be avoided if we want to retain

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the assimilated clouds". This statement is only true if the only parameter of the model being adjusted is the water content, as is the case for the assimilation presented here. In other assimilation methods (such as 4D-Var) errors in cloud position are frequently corrected by adjusting the pressure and wind fields to move the positions of the clouds, meaning that in some cases the cloud errors can be corrected without incurring a humidity bias (although Benedetti and Janiskova, 2008 do also notice a negative impact on humidity in their study, as you note). The paper being reviewed only discusses surface humidity – it would be interesting (but, in my opinion, not essential for acceptance of the paper) to include some observations of total column water vapour from Ground-Based GPS stations for comparison with the total column water vapour in the reference and experiment models.

In the revised manuscript, a comparison with total column water vapour from radiosounding observations at Trappes is added, as suggested by the reviewer (Figure 5 P 37). There is an overall good agreement between the observations and the simulations. The assimilation scheme has only a slight impact on the total column water vapour at this location and even improves the small negative model bias. So the problems with the humidity at the surface do not translate in drastic changes in the vertical moisture profile.

* Simple 1D assimilation schemes such as this do not modify all of the model fields, and can thus result in model fields being inconsistent with each other. In some cases this can result in meteorological instability in the model (resulting in e.g. increased convective rainfall, or increases in the RMS of the vertical component of the wind). Yucel et al. 2003 also find that these inconsistencies can cause the benefits of the assimilation to rapidly disappear in their model forecasts. On line 2 of page 13368 it is stated that the assimilation does not "disrupt the model stability". The results presented in the Figures and Tables of the paper give little indication as to whether or not the stability is affected by the assimilation, as comparisons of the convective rainfall, vertical component of the wind, etc produced in the reference model and experiment model are not given. In

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order to support this conclusion, the Editor should make sure that they are happy that at least one Figure or Table is included which gives a comparison of the stability in the reference model with the stability in the experiment. It would be interesting to see what changes in precipitation amount are found in the regions of Southern France where the two models show the largest differences in water paths.

It is true that the stability of the model is an issue when only some model fields, in this case the moisture and temperature, are modified. In order to assess the impact of the scheme on the model stability, a comparison of the vertical wind speeds and the precipitation amounts between the reference and the assimilation experiment are added to the manuscript (Figures 6 and 7 P 38 and 40). It is clear that the assimilation has some effect on these variables, especially in the regions where most changes occur (SW France and the Alps). The changes in vertical wind speeds are relatively small (less than 10 % of the monthly mean values) but the change in precipitation is substantive (26 % increase of the monthly mean rainfall) although it should be noted that this is a dry month where eventual changes have a strong effect on the overall statistics. By modifying the model moisture and temperature fields, these kind of changes can be expected and do not seem excessive. The validation stations in these regions that are added to the manuscript (Oensingen and Bordeaux) both show improvements of the target variables surface temperature and incoming shortwave radiation, which gives us confidence that the benefits of the assimilation scheme are certainly larger than the consequences of eventual inconsistencies. The authors admit that the statement that the assimilation does not at all disrupt the model stability is probably too strong and this sentence is changed in the text (P 19 L 10).

Minor corrections to the text

* Page 13358 line 27 – “To two primary parameters” should be corrected

The text is corrected now (P 5 L 19).

* Page 13359 lines 16-17 – According to ESA, SEVIRI should be capitalised as follows:

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“Spinning Enhanced Visible and InfraRed Imager”.

The text is adapted as suggested by the reviewer (P 6 L 13).

* Page 13364 line 21 – “decrease halfway the month” should be corrected

The text is corrected now (P 14 L 5).

* Page 13366 line 19 – “Most positive changes” is ambiguous and should be changed to either “Most increases” or “Most beneficial changes” depending on which meaning is required.

The text is adapted as suggested by the reviewer (P 17 L 3).

Minor corrections to figures

* The colour scale bar on Figure 6 (page 13379) should have the “zero” point labelled so that the reader can easily see which colours are negative and which are positive.

The colour bar is adapted as suggested by the reviewer (P 39).

References Zupanski, M., Zupanski, D., Vukicevic, T., Eis, K. and Vonder Haar, T. 2005. CIRA/CSU Four-Dimensional Variational Data Assimilation System. Monthly Weather Review 133, 829-843.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C9078/2011/acpd-11-C9078-2011-supplement.pdf>

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