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Interactive Comment

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

Anonymous Referee #2

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1 Summary

This paper decribes a 1-month trial assimilating Cloud Optical Thickness (COT) measurements into an ARPS model using 1D Optimal Interpolation. The assimilation procedure uses two fields from the model background (q_t – the total water, and q_s – the humidity saturation point) and one observational field (τ_0 – the COT). In order to simplify the assimilation, a very simple description of the errors in these fields is used (with the B matrix for the background errors being diagonal, with the values corresponding to the error in the total water q_t simply being set to $0.3q_t$, and the error in the saturation point q_s calculated from the model background assumed to be zero at all locations and times). The method takes advantage of the linear nature of COT to obtain an analysis

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with minimal computation. The 1D nature of the assimilation and the nature of the B-matrix chosen means that only the water content is directly affected by the assimilation, and the model dynamics and other fields are only affected indirectly.

2 General corrections

In Section 3.3 of the paper the model background cloud water is derived from q_t and $q_s\,(\equiv q_{sat}\,(T))$. 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 B-matrix (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.

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.

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.

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

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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.

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 dissappear 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 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.

3 Minor corrections to the text

Page 13358 line 27 – "To two primary parameters" should be corrected

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

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“Spinning Enhanced Visible and InfraRed Imager”.

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

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.

4 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 13355, 2011.

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