Manuscript review amt-2020-166 « Improvement of numerical weather prediction model analysis during fog conditions through the assimilation of ground-based microwave radiometer observations: a 1D-Var study ».

First of all, the authors thank the two anonymous reviewers for their positive feedback and helpful comments to improve the manuscript. All modifications have been taken into account ; most relevant are highlighted in red in the new version. We hope this new version will be suitable for publication.

Reviewer 1 :

Abstract: The authors should mention already in the abstract that the brightness temperatures are assimilated directly over a forward operator (RTTOVgb)

→ Has been clarified in the abstract :
To that end, temperature, humidity and liquid water path (LWP) retrievals have been performed by directly assimilating brighness temperatures using a one-dimensional variational technique (1D-Var).

Line 31-33: This formulation is not exact: it is true that satellite data provide limited information on the ABL, but not because of the complexity of data assimilation over lands, to be exact this issue makes the use of the data for NWP more difficult. Please rephrase.

→ Thanks for pointing this inconsistency. This has been modified :
Even if satellite data provide a global coverage all over the world, they provide limited information on the ABL due to the attenuation by clouds and degraded vertical resolution in the ABL. Additionally, uncertainties in surface properties (such as skin temperature and emissivity , Guedj et al. (2011)) limit the assimilation of surface-sensitive channels over lands.

Line 44: it could be emphasized here that the study by Otkin and Hartung et al. (2011) with 140 MWRs was an OSSE (in contrast to your study using real data)

→ In order to highlight this aspect we used the term « simulated network of 140 MWRs » but to make it more clear we added :
The impact of a simulated network of 140 MWRs through an OSSE was also investigated by Otkin et al. (2011) and Hartung et al. (2011) on a winter storm case.

Line 123: For clarification for readers not familiar with (MW) remote sensing you could add half a sentence why transparent channels are omitted at low elevation angles

→ this explanation which was given in the initial manuscript line 174 in section 3.2 has been moved to line 125 :
Transparent channels are not used at low elevation angles due to the violation of the assumption of horizontal homogeneity

Line 134: The authors could add a sentence, why it is inadequate for fog areas

→ this has been clarified :
As demonstrated by Ménétrier and Montmerle (2011), climatological covariances are inadequate for fog areas which exhibit a much stronger positive coupling between temperature and humidity and attenuated vertical correlations above the fog layer.

Line 150-151: This is not clear. What kind of tests?

The quality of the fog B matrix (which is fixed for all fog events through the 6 month period) can depend on how many grid points in fog conditions are found within the domain (to avoid sub-sampling problem). It will also depend on how much variability in the different fog cases were taken into account. The quality of this B matrix can thus depend on the assimilation cycle which is used for its calculation. This is why we first calculated fog B matrices at different assimilation cycles. Then we run the 1D-Var algorithms and chose the fog B matrix which was giving the best RMSE with respect to radiosounding. The sentence has been rephrased to clarify.

Several fog B matrices have been computed using different assimilation cycles. The fog B matrix showing the best results in terms of root-mean-square-errors (RMSE) with respect to radiosoundings has then been selected for this study.

Section 3.2: This section is not structured well. First it is about obs errors, then bias correction, then obs errors, then both. .

We tried our best to make this section better structured. To that end we sub-divided this section into section 3.2.1 commenting only the results in line with the B matrix and section 3.2.2 commenting only the results in line with the bias correction. Table 4 and figure3 are used in both sections but only to comment the corresponding results.

Line 176: what do the authors mean by the "individual errors which were added in quadrature"? Not really clear to me.

→ it has been clarified in the manuscript :

$\sigma_{tot} = \sqrt{\sigma_{noise}^2 + \sigma_{calib}^2 + \sigma_{FM}^2}$
with σ_{tot} the total observation errors, σ_{noise} the uncertainty due to noise, σ_{calib} calibration uncertainties and σ_{FM} the uncertainty due to spectroscopic errors in the radiative transfer model.

Line 214-215: not fully clear. . . So the authors want to say the dataset consists of stratus clouds, profiles with fog, and some clear-sky profiles?

→ Correct, we just want to highlight that the RS profiles were launched during IOPs when we were expecting fog or stratus lowering. Thus, some of them are clear-sky, a few are under fog conditions and others in stratus-cloud. We clarified the sentence :
Radiosondes were launched during IOPs in different atmospheric conditions: the majority are under stratus-cloud and fog conditions and a few of them in clear-sky

Line 251-256: Please give more explanations in this paragraph, why the underestimation of specific humidity at nighttime is due to an overestimation of saturation, ...

→ As explained at the beginning of section 4, the AROME model predicts a thick fog layer whereas the observations (in-situ tower measurements) show fog no more than 10 m thick. Thus, we know the AROME model overestimates the saturation. The true temperature should be higher and should not reach saturation. , water would stay in its gas phase instead of being converted into liquid . What we observe is that the model converts too much water vapour into liquid (erroneously). The specific humidity is thus underestimated. One sentence has been added in the manuscript :

Indeed, as the fog layer was thicker in AROME than in the observations, we believe the model converts too much water vapour into liquid erroneously, which makes it underestimate specific humidity

...and why most of the model increments are produced by the B matrix cross-covariances ?

→ As concluded in section 3.2, the configuration 3 is used in the next sections. It means a block-diagonal B matrix is used under no fog conditions and a fog B matrix with cross-covariances is used when there is fog in the observation. From the visibility measurement of figure 4, we see that fog is only observed at 0 UTC and then between 5 and 9 UTC.
It means that a fog B matrix with cross-covariances is used at 0 UTC and then between 5 and 9 UTC. Outside of this time period, a diagonal B matrix is used. In figure 6, we can see that the specific humidity after 1D-Var is almost identical to the background every time a diagonal B matrix is used.
The larger increments observed during the fog events are thus attributed to the cross-covariances between temperature and humidity. We clarified this point :
This is likely due to the use of the cross correlated fog B matrix under these conditions, as opposite to the use of a block diagonal B matrix when fog is not observed.

L264: For clarification his could be rephrased to ". . . During the period where the model fails to simulate the stratus cloud, the LWP is significantly increased in the 1D analysis with values between. . ."

→ Agree, it has been modified

L279: The authors could add one sentence on what the visibility diagnosis is based.

A new sentence has been added to the manuscript to be a bit more explicative. The full explanation about this visibility diagnosis will be discussed in the manuscript of Dombrowski-Etchevers et al. (2020) :

In this new diagnosis, the visibility is directly deduced from the liquid water content at ground. It was computed through a statistical regression between hourly maximum of liquid water content forecast by AROME and observed minimum of visibility on 100 ground stations during five months.

L317-318: This is not clear. Does it mean the profiles used are not forecasts but taken from an analysis with conventional data already assimilated?

→ Currently in the Météo-France 3D-Var scheme control variables are : temperature and surface pressure, specific humidity, wind. Thus at each assimilation cycle only these variables are updated to match all the observations assimilated. However, the hydrometeors are kept unchanged.
It means that the analysis state of the AROME model for the hydrometeors correspond to the previous background, which is a 1 hour forecast. The hydrometeors are then balanced with the other fields in the first time steps of the forecast through the model physics.
This has been clarified in the manuscript :
In fact, as hydrometeors are currently not included in the control variables of most operational variational data assimilation schemes, these fields are kept unchanged during the analysis. Thus, the analyzed hydrometeor fields correspond to the previous background. Consequently, in the following statistics, the background values of LWP correspond in fact to the LWP in the operational AROME analysis. These fields are then modified according to the updated temperature and humidity analyses in the first time steps of the forecast through the model physics.

Technical corrections

L31: Better: “. . . wich is undersampled by observations. "Even though satellite data provide a global coverage. . .“

→ Agree and modified

L48: Better: impact of this network was found to be neutral..”
→ Agree and modified

L51: Better: “AROME model with a one-dimensional..”
→ Agree and modified

L55: correct to: “. . . and evaluates the impact. . ..”
→ Agree and modified

L122: correct to: “. . . consists of..”
→ Agree and modified

L129: replace “spatially” by “horizontally” (because spatially comprises vertical and horizontal directions)
→ Agree and modified

L144: Better: “...with a horizontal resolution set to 3.2km and . . .” (“finally” should be omitted)
→ Agree and modified

L167: no comma here
→ Agree and modified

L167: better: “but also on an adequate specification of. . .”
→ Agree and modified

Line 201: do you mean Config1 here?
→ Correct, thanks for pointing this error

General: References to figures in the text should be with capital “F”. “Figure X” instead of “figure X”.
→ modified

L222: Typo: “almost”
->This sentence has been removed in the new version but thanks for pointing the error

L229: Typo: cloud base height
→ agree modified

L232: better: “. . . fog is observed at 10m altitude during 40 minutes at midnight and. . .”
→ agree modified

L257: better: “. . . by night leads to the effect that the fog layer is not saturated any more in agreement. . .”

→ agree modified

L262-64: Better: “. . . with a maximum reaching 90gm-2 at 7UTC. This value, however, decreased down to. . .”
→ agree modified

L269: Better: “While the previous focuses on an extreme. . .”
→ has been modified into : While the previous **section** focuses on an extreme

L382: Better: “. . . has been investigated with. . .”
→ agree and modified

L429: Better: “. . . on temperature and LWP and small but. . .”
→ agree and modified

Figure 8 Caption: should be re-phrased to: “. . . differences compared to tower measurements. . .”
→ agree and modified

Figure 13: The axes are difficult to read. Maybe the figure could be enlarged to improve this.
→ The figure has been made again to make it more readable