

RESPONSE TO REVIEWER #1

"[...] This paper presents the LDAS-Monde data assimilation system and it evaluate results over Europe. Results are well presented, showing interesting consistent impact of the data assimilation on soil moisture, LAI and hydrological variables such as discharge. It is a innovative study of high interest for the community because it presents multi-variate data assimilation in a coupled land surface and river routing model. The paper is very clearly written and very well presented. I recommend the paper to be published after the minor comments below are accounted for."

The authors thank anonymous Reviewer 1 for his/her review of the manuscript and for the fruitful comments. Responses to the Reviewer 1 are structured as follow: (1) 1.X: comments from Reviewer 1, (2) Response to 1.X: author's response and author's changes in manuscript when any. For sake of clarity, line and page numbering from the first submission is used.

1.1 [Page 1, lines 12-13: The first sentence of the abstract should be reformulated to clarify the two objectives of the paper which are testing LDAS-Monde and improving monitoring. It does not make sense to says that LDAS-Monde "is tested ... to increase monitoring accuracy...". Testing itself only allows to evaluate and to asses monitoring accuracy.]

Response to 1.1

Agreed, the first sentence on the abstract has now been reformulated.

"In this study, a global Land Data Assimilation system (LDAS-Monde) is tested over Europe and the Mediterranean basin to increase monitoring accuracy for land surface variables." Is now:
"In this study, a global Land Data Assimilation system (LDAS-Monde) is applied over Europe and the Mediterranean basin to increase monitoring accuracy for land surface variables."

1.2 [Page 2 line 31: Replace "Assimilation impact shows that" by "results show that"]

Response to 1.2

Agreed, it has now been changed in the revised version of the manuscript.

Assimilation impact shows that the LDAS works well constraining the model to the observations and that stronger corrections are applied to LAI than to SM. Is now: *"Results shows that the LDAS works well constraining the model to the observations and that stronger corrections are applied to LAI than to SM."*

1.3 [Page 2 line 33: "The assimilation impact's evaluation is succesfully carried out using...". It is not clear what succesfully means here. Is it that it worked technically or that it is a comprehensive evaluation using different data sets, or the results show good performance?. I would just replace by "A comprehensive evaluation of the assimilation impact is conducted using ..."]

Response to 1.3

Agreed, it has now been changed in the revised version of the manuscript.

"The assimilation impact's evaluation is succesfully carried out using [...]" is now: *"A comprehensive evaluation of the assimilation impact is conducted using [...]"*

1.4 [Page 6, line 147: replace "depth" by "deep"]

Response to 1.4

Agreed.

1.5 [Page 8 equation 5: it is not clear what t_i and t_{i+1} are. t_i must be the analysis time, but "+1" needs to be clarified: time step? analysis window length?]

Response to 1.5

It is now clarified in the revised version of the manuscript, it represents the end of the 24-hour assimilation window.

“The control vector evolution from time t_i to time t_{i+1} is then [...]” is now “The control vector evolution from time t_i to the end of the 24-hour assimilation window (t_{i+1}) is then [...]”

1.6 [Page 8, 219: "WFDEI originates from the ECMWF ERA-Interim reanalysis (Dee et al., 2011) with a spatial resolution of 0.5": replace with by "interpolated to" otherwise it gives the impression that ERA-Interim is at 0.5 degrees resolution which is wrong.]

Response to 1.6

Agreed, *“WFDEI is based on the ECMWF ERA-Interim reanalysis (Dee et al., 2011) with a spatial resolution of 0.5°, and is corrected [...]” is now “WFDEI is based on the ECMWF ERA-Interim reanalysis (Dee et al., 2011) interpolated to a spatial resolution of 0.5°, and is corrected [...]”*

1.7 [Page 9, line 3: reformulate the sentence to avoid parenthesis (confusing because they do not correspond to mathematical formulation in this case): Where SSM_m , SSM_o , σ_m and σ_o correspond to the model and observation means and standard deviations, respectively.]

Response to 1.7

Agreed, considered sentence has been replaced by: *“Where SSM_m , SSM_o , σ_m and σ_o correspond to the model and observation means and standard deviations, respectively.”*

1.8 [Page 11 line 310: Here the time is a mean time for a given month, whereas earlier in the paper "t" was used for instantaneous time. Replace "time t" by "month mt" or something like that.]

Response to 1.8

Agreed, *“[...] where Q_s^t is the simulated river discharge (or analysed) at time t and Q_o^t is observed river discharge at time t .” is now: “[...] where Q_s^t is the simulated river discharge (or analysed) at time t and Q_o^t is observed river discharge at month mt ”.* It has also been changed in equation 8.

1.9 [Page 11 line 310, and page 19 lines 518, 521: replace " Eff. " by "Eff"]

Response to 1.9

Agreed

1.10 [Page 13 line 356: ")")]

Response to 1.10

This typo has now been corrected.

1.11 [Page 13 lines 368-370: "Soil moisture observational and background errors are also scaled by the model soil moisture range, assuming that there is linear relationship between the soil moisture errors and the dynamic range. This was already said lines 354-356. Avoid repetition.]

Response to 1.11

Agreed, “[...], assuming that there is linear relationship between the soil moisture errors and the dynamic range” has now been deleted.

1.12 [Page 14 table1: "Earth2Observe" by "EarthH2Observe" and define NIT.]

Response to 1.12

Agreed, "*Earth2Observe*" is now "*EarthH2Observe*" and NIT has been defined as “*the biomass option selected for the ISBA LSM*” in the caption as well as “Dif” indicating the diffusion scheme of ISBA LSM.

New caption is: “*Summary of the experimental setup used in this study. “Dif” indicates that the diffusion scheme of the ISBA LSM is used, ‘NIT’ represents the biomass option selected.*”

1.13 [Page 14 line 377-380: This is not clear. The Jacobian values study could be done using the background forecasts of the analysis experiment. Please clarify.]

Response to 1.13

We agree that the Jacobian values study could be done using the background forecasts of the analysis experiment. In the literature diagnostic studies of the Jacobian values have usually been performed before including (new) observations types (Chevallier and Mahfouf, 2001, Fillion and Mahfouf, 2003, Garand et al., 2001 and Rudiger et al., 2010). In this study it has been decided to follow the same approach as in Rudiger et al., 2010.

We also agree that the sentence pointed out by Reviewer #1 (line 377-380) needs clarifications, it has now been changed in the revised version of the manuscript:

“Prior to these runs, an analysis experiment without assimilating any observations has also been run over 2000-2012 to study the model sensitivity to the observations through the Jacobians. Studies of the Jacobians values have to be performed before including observations because it is essential to understand the sensitivity of the assimilation system before combining it with observations.” is now “Diagnostic studies of the Jacobian values have usually been performed before including new observations types (Chevallier and Mahfouf, 2001, Fillion and Mahfouf, 2003, Garand et al., 2001 and Rudiger et al., 2010). That is why, following Rudiger et al., 2010, an analysis experiment without assimilating any observations has also been run over 2000-2012 to study the model sensitivity to the observations through the Jacobians.”

The following references have been added to the revised version of the manuscript:

Chevallier, F., and J.-F. Mahfouf: Evaluation of Jacobians of infrared models for variational assimilation, J. Appl. Meteorol., 40, 1445–1462, doi:10.1175/1520-0450(2001)040, 2001.

Fillion, L., and J.-F. Mahfouf: Jacobians of an operational prognostic cloud scheme, Mon. Weather Rev., 131, 2838–2856, doi:10.1175/1520-0493(2003)131, 2003.

Garand, L., Turner, D. S., Larocque, M., Bates, J., Boukabara, S., Brunel, P., Chevallier, F., Deblonde, G., Engelen, R., Hollingshead, M., Jackson, D., Jedlovec, G., Joiner, J., Kleespies, T., McKague, D. S. McMillin, L., Moncet, J.-L., Pardo, J. R., Rayer, P. J., Salathe, E., Saunders, R., Scott, N. A., Van Delst, P., Woolf, H.: Radiance and Jacobian intercomparison of radiative transfer models applied to HIRS and AMSU channels, J. Geophys. Res., 106, 24,017–24,031, doi:10.1029/2000JD000184, 2001.

1.14 [Page 14 line 390: "section 222" by "section 2.2.2"]

Response to 1.14

Agreed.

1.15 [Page 14 392-395 and figure 2: It is not clear how the monthly LAI correlation are computed with one observation every ten days.]

Response to 1.15

Reviewer #1 highlights a part of the text that needs to be clarified, thanks for pointing this out. First, scores are provided for each year. For LAI (available every 10 days) they are based on a maximum of 36 values for each pixels. Then with figure 2 seasonal scores are provided: values for LAI for a considered month encompass all the 2000-2012 period, i.e. for January we have used values of all January months within 2000-2012 (i.e. a maximum of 13 x 3 LAI values). It is now clarify in the revised version of the manuscript by referring to seasonal scores instead of monthly scores (in the text and in the caption of figure 2).

“RMSD exhibits however a strong seasonal dependency as illustrated by Error! Reference source not found. (blue line) with values close to $1 \text{ m}^2\text{m}^{-2}$ from June to October” is now “Figure 2 (blue line) illustrates seasonal RMSDs (fig. 2a) and correlations (fig. 2b) between LAI from the open-loop and the GEOV1 LAI estimates over 2000-2012. From fig. 2a, a strong seasonal dependency of RMSD is noticeable with values close to $1 \text{ m}^2\text{m}^{-2}$ from June to October.”

Caption of figure 2 is now: *“Seasonal a) RMSD and b) correlation values between leaf area index (LAI) from the open-loop, analysis and GEOV1 LAI estimates from the Copernicus Global Land Service project over 2000-2012.”*

1.16 [Page 16 lines 426-429: "Sensitivity of LAI to changes in soil moisture (Table 2, bottom rows) is generally weaker than that of SSM (Table 2 top rows) suggesting that although control variables related to soil moisture will be impacted by the assimilation of LAI, they would be even more impacted by the assimilation of SSM." It is not possible to directly compare jacobians values of dSSM/dW (top row) and dLAI/dw (bottom) as they don't have the same unit at all. So, the logic of this sentence does not work. The authors should elaborate the analysis of the table results to come to the conclusion that SSM assimilation should have more impact than LAI assimilation.]

Response to 1.16

Reviewer #1 is absolutely right, the logic of this sentence doesn't work at all and it is removed from the revised version of the manuscript. Impact of each observation-types, LAI and surface soil moisture on the analysis is currently underway at Météo-France.

“Sensitivity of LAI to changes in soil moisture (Error! Reference source not found., bottom rows) is generally weaker than that of SSM (Error! Reference source not found. top rows) suggesting that although control variables related to soil moisture will be impacted by the assimilation of LAI, they would be even more impacted by the assimilation of SSM” is now: “Sensitivity of LAI to changes in soil moisture (Error! Reference source not found., bottom rows) suggests that control variables related to soil moisture will also be impacted by the assimilation of LAI.”

1.18 [Page 17: lines 451-452: just say "January", "June" and "October" to be consistent with the wording used just above (eg line 444) to present this figure. The figure caption makes it clear that it is for the multi-year period.]

Response to 1.18

Agreed.

1.19 [Page 18: line 500: "Areas where positive analysis increments were found for LAI (Figure 5) are marked by a decrease in drainage and runoff (in red on Figure 9) while evapotranspiration increases (in blue Figure 9)." It is not that systematic: drainage and runoff impact is more patchy than LAI increments. I would replace "are marked by" by "tend to correspond to"]

Response to 1.19

Reviewer #1 is right, this statement has to be smoothed: "*are marked by*" is replaced by "*tend to correspond to*".

1.20 [Page 19, line 516 and Figure 10 caption: "46.0858 N-3.21641E" is it necessary to give 5 digits? It is finer than the model resolution and if the purpose is the traceability of the observed GY location is there an site id number that could be used instead? It would be clearer in the paper to stick to two digits latitude-longitude information (as in line 523 for the discharge).]

Response to 1.20

Agreed, to our knowledge Agreste does not provide id number. New caption is: "*a) Correlation values for the above ground biomass from the open-loop with grain yields estimates from Agreste French agricultural statistics portal (<http://agreste.agriculture.gouv.fr>) over 45 sites in France plotted against correlations between the same quantities but above ground biomass from the analysis; b) same as a) for RMSD values; c) scaled anomalies time-series of above ground biomass from the open-loop (black dashed line) the analysis (black solid line) and grain yields observations (red solid) for one site in Allier, France (46.09N-3.21E).*"

1.21 [Figure 14: not discussed in the paper]

Response to 1.21

Corrected, it was a typo referring to the wrong figure when discussing GPP evaluation. Also, figure 14 is now Figure 15 as Reviewer #2 suggested to split figure 12 in two figures.

1.22 [Page 20 line 552-555 and Figure 12: I would expect figure 12 d and i to be identical as they are both indicated to show 'analysis-Model' evapotranspiration. Please clarify.]

Response to 1.22

Figure 12 has now been split into two figures (as suggested by Reviewer #2), one representing evapotranspiration (using estimates from both GLEAM, top row, and FLUXNET-MTE, bottom row projects) and one for Gross Primary Production (using estimates from the FLUXNET-MTE project). When referring to the labelling of the current version of the manuscript, if figure 12 d) and i) represent the same information, not the same period is considered: 2000-2012 for 12 d) and 2000-2011 for 12 i). It explains that even if geographical patterns are similar color intensity is slightly different.

1.23 [Page 21: line 569: 'the observation operator is the very thin top layer' this is not correct. Replace by 'the model equivalent was the very thin top layer'. Also replace "that is a thick layer" by "that was a thick layer" to be consistent with the first part of the sentence line 568.]

Response to 1.23

Agreed, it is now corrected in the revised version of the manuscript.