

Interactive comment on “Improved meteorology and surface energy fluxes in mesoscale modelling using adjusted initial vertical soil moisture profiles” by Igor Gómez et al.

Anonymous Referee #2

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In this manuscript, the authors analyse the impact of different soil moisture initialization on meteorological forecasts for two sites in Spain. The forecasting model is the well-established RAMS scheme. The forecasting period is from 6th to 11th of July 2011. The evaluation focuses on turbulent fluxes (i.e., sensible and latent heat), radiation components (i.e., downwards shortwave and upward longwave and shortwave radiation), land surface temperature, and soil moisture. The authors thus provide a comprehensive evaluation of atmosphere-land surface exchange fluxes and land surface states. If local observations of these fluxes are not available, then simulated quantities from GLDAS (based on the Noah LSM) are used. I would like to congratulate the authors to this comprehensive analysis which reveals model deficiencies. Unfor-

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Unfortunately, the authors provide little discussion of their results and there are substantial aspects left out of the analysis. My major criticism is that all of the results are a direct consequence of the thermodynamic physics underlying land surface schemes. If soil moisture is reduced, then sensible heat will increase, latent heat will decrease, land surface temperature and upward longwave radiation will increase. An important aspect here is that biases in turbulent fluxes are reduced, but biases in radiation are increased (i.e. upward longwave radiation). Thus, the authors simply shift biases from one component of the model to another, which is not discussed at all in the manuscript. The model bias could originate from different sources (e.g., model structural errors, errors in model parameters, etc.). The authors only focus on one source, that is the initialization of soil moisture without much motivation or discussion why other sources are left out. It is well known that simulated soil moisture is a quantity that heavily depends on the model structure (Koster et al., 2009). Therefore, I am not surprised that unsatisfying model behaviour is emerging if RAMS is initialized with soil moisture from another modelling system (NCEP FNL). This is especially true given the large differences in the spatial resolution (3 km vs 1 deg). Moreover, I found seminal work on forecast with RAMS that initialized soil moisture as 50% of field capacity (Castro et al., 2005) that has not been considered in this work. Based on this analysis, I find the findings of the authors rather trivial and a consequence of transferring soil moisture from one model to another without considering differences in model structures. Moreover, the manuscript has frequent references to wrong figures and tables which makes it often difficult to correctly understand what the authors want to express (see further comments below). As a result of my analysis, I recommend to reject this manuscript. The authors need to reassess their strategy for initialization of soil moisture, but this would be a new paper.

Further general comments:

The title is misleading because there is no general improvement but errors are shifted from turbulent fluxes (i.e., sensible and latent heat) to outgoing longwave radiation.

There are frequently general statements that can be made without this study because

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these are based on the thermodynamic physics underlying any land-surface scheme. For example, p. 9 l. 275: "Drying the soil,... " and p. 10 l. 287ff: "Considering the meteorological variables..."

I do not think that evaluating relative humidity is a good choice because it depends on both the atmospheric vapour pressure and air temperature. Biases in the later are thus transferred to biases in relative humidity. Using specific humidity would be better because these are more independent of temperature biases.

Throughout the manuscript, the authors are referring to different circulation states (e.g., mesoscale circulation). Results are separated for these in the table. It would have been helpful if the corresponding periods of the different circulations are also highlighted in all figures displaying time series, for example Fig. 8.

Why is Exp. 2 not displayed in Figs. 3 and 5?

Further specific comments:

- p. 7 l. 204: It should read Fig. 3c.
- p. 8 l. 249: It should read "sign"
- p. 9 l. 265: It should read Fig. 5d
- p. 9 l. 276ff.: The differences in thermodynamic variables between Exp. 2 and 3 are not displayed in table 7. In table 4, the differences between Exp. 2 and 3 are as large as those between the other experiments. This statement is thus misleading. I also would avoid the use of the word "really" in general.
- p. 10 l. 288: Fig. 6 has not been discussed yet in the text. Why is it referenced here?
- p. 10 l. 294f: Soil moisture is controlling both the partitioning of energy into sensible heat and latent heat and is a model dependent quantity. However, changing soil moisture to reduce a cold bias in temperature is just one option. The cold bias could also originate from deficiencies in process parametrization including model parame-

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ters. These are often conceptual and associated with substantial uncertainties (for example the exponents in equation 2). The authors should answer why the initial soil moisture field they use is too wet and justify the use of a drier one.

- p. 10 l. 304f: I do not understand the use of the word "adjust" here.

- p. 10 l. 306f: According to table 7, I would argue that all of these datasets provide a comparable performance.

- p. 10 l. 323f: The large bias by RAMS in downward longwave radiation could also lead to the cold bias the authors try to remove by adjusting soil moisture. This gives the impression that the authors want to get the right result for the wrong reason. The authors should perform further evaluation of cloudiness and the atmospheric radiation scheme to identify the origin of this bias.

- p. 11 l. 326f: It is surprising to see that upward longwave radiation is overestimated. This clearly indicates that there is a problem in the model structure because removing biases in the turbulent fluxes (i.e., sensible and latent heat) introduce biases in this radiation component. This indicates that surface and skin temperature cannot be calculated in a way to satisfy both turbulent fluxes and radiation.

- p. 11 l. 336ff: I cannot see a reversed trend between the derived LST from RAMS and SEVIRI at the BON station during daytime. The authors need to clarify what they want to express here.

- p. 11 l. 338ff: I do not see an added value of the evaluation of soil moisture because of two reasons. 1) It is not a result that EXP3 and EXP4 have drier soils than EXP1 because this is how the experiments have been designed! 2) The comparison against GLDAS is troublesome because GLDAS is run at a much coarser resolution and also has a different soil depth in the first layer compared to RAMS (10cm vs. 2cm).

- p. 11 l. 345ff: I am confused about the presented results because I would have expected that the black solid lines in Figure 9 should be equal to the corresponding red

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and green lines in Figure 8b and 8d. Both of these are claimed to show soil moisture in the top layer, but they are different. The striking feature of Figure 9a and Figure 9b is that the soil in Layer 3 and 4 becomes wetter with lead time. I assume that this water is transferred from lower soil layers by capillary rise but the rate seems to be very high.

- p. 12 l. 362: The abbreviation BRX should be reintroduced here.

- p. 12 l. 365: The authors did not show that any observations of soil moisture. This conclusion is not supported by the manuscript. GLDAS is used as a reference, but values between GLDAS and RAMS cannot be compared because of substantial differences in spatial and vertical resolution.

- Conclusions: The authors do not mention at all that the bias in upward longwave radiation is increasing with drier soils and thus, biases are transferred from the turbulent fluxes to the radiation components.

- Figure 5: The x-axis label with Time(hours) is confusing. It should be days. Do not use red and green lines in the same plot because this is not color-blind friendly.

References:

Christopher L Castro et al. "Dynamical downscaling: Assessment of value retained and added using the Regional Atmospheric Modeling System (RAMS)." J Geophys Res, 2005 vol. 110 (D5) pp. 681-21. <http://doi.wiley.com/10.1029/2004JD004721>

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