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Biogeosciences Discussions

Interactive comment on "Observed and modelled ecosystem respiration and gross primary production of a grassland in southwestern France" *by* C. Albergel et al.

Anonymous Referee #1

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In the paper a modification of the ISBA-A-gs model to take into consideration soil water content in the Ecosystem Respiration simulation is presented. To validate the approach, data measured at one eddy covariance site over grassland in South of France is used. The topic is important and interesting since water limitation effects on soil respiration can play major roles in some ecosystems, although this has been already discussed in the past and solutions similar to the one proposed here have been presented (e.g. Reichstein et al 2003). The paper is clear but there are a number of issues that should be clarified and better explained before publication and for this reason I recommend the submission of an improved version of the work.

One of the main concerns is that the authors used only one site to calibrate and validate the model. This is introducing some circularity since the model is validated on the same data used in the parameterization (e.g. Fig3). Given the availability of multiple eddy covariance sites I would strongly recommend to test the model using data measured at other sites to validate the generalization capacity of the new formulation.

The f(Wg) factor proposed assumes a linear effect of water availability on respiration and doesn't consider the fact that water excess also affect respiration due to anaerobic conditions (e.g. Skopp et al 1990).

References should be provided for the eddy covariance processing, in particular 1) move the sentence at P436L23-25 at page 435 to explain why a quite small wind-sector has been used 2) explain how the u* threshold has been estimated since this filter is affecting strongly the night time data availability for the model calibration 3) since the model has 5 minutes time resolution (P433L6) explain how the eddy data have been processed (5 minutes also for eddy data??).

P435L15: since only nigh-time data are used to parameterize the model, Fig.1 should present also the distribution of nigh-time data to verify how much they are representative (for T and Wg) of the pooled data (i.e. is it ok to apply a night-time-parameterization to daytime conditions?).

P436L18 and L20: should be Hz and not GHz. It is also not clear from the code reported which IRGA model is used. I would suggest to use "LI7000" or "LI6262" if you used a close-path and "LI7500" if you used an open-path.

P439L23-28: give an explanation why excluding water deposition periods improve the model-measurements fitting. Is it due to problems in the eddy covariance methodology (e.g. open-path IRGA) or due to a not correct simulation of respiration after an increase of water deposition (e.g. due to difficulties to model respiration pulses in dry conditions, anaerobic conditions if above the FC, ...)?

P440L9-14 and Fig.3: comparing these two days (July and October) there are other factors changing in addition to Wg (plant physiology, T, probably LAI etc.). Two days in the same period and with similar T should be used to remove other factors that could potentially contribute to explain the differences.

Fig.5: the figure is not very useful since it is difficult to really compare data and model outputs.

Fig 6: not clear is in this plot Eq1 or Eq4 have been used. Also this figure is not simple to analyze, I would suggest create one single plot with the cumulative curves of the three variables.

Skopp, J., M. D. Jawson, and J. W. Doran, Steady-state aerobic microbial activity as a function of soil water content, Soil Sci. Soc. Am. J., 54, 1619– 1625, 1990.

Reichstein, M., et al., Modeling temporal and large-scale spatial variability of soil respiration from soil water availability, temperature and vegetation productivity indices, Global Biogeochem. Cycles, 17(4), 1104, 2003.

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