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Interactive comment on "Impact of climate, vegetation, soil and crop management variables on multi-year ISBA-A-gs simulations of evapotranspiration over a Mediterranean crop site" *by* S. Garrigues et al.

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We would like to thank referee-2 for the review of our paper. Your remarks were very useful to improve the overall quality and clarity of the paper. We carefully considered your comments and we modified the manuscript. Below, we provide answers to each of your comment

General comments

- "Overall this paper is interesting and is useful to the scientific community, however

the paper is difficult to read due to the presentation of the results, its organization and confusing sentences. Specifically the organization of the different simulations and the associated comparisons are confusing." Furthermore, the authors rely too much on presenting tables of metrics instead of performing critical analysis of the results. As such the analysis section is weak and confusing. This paper would greatly benefit from better organization and consistency in the methodology, as well as a more thorough analysis with more figures and less tables. There is also the need to revise the text for grammar and clarity. "

We made the following major changes to improve the clarity of the paper and enhance the analysis of the results: We re-organized the simulations and the associated comparisons: we used the control run (CTL) as the reference/truth simulation to assess the impact of each driver on ET. In the previous version of the paper, we started by analysing the simulation performed with the large-scale datasets for all the forcing variables (climate, soil properties, irrigation, vegetation). The other simulations were derived from it by replacing the large-scale dataset used for each forcing variable by the corresponding local observations taken at the Avignon site. This was done consecutively for climate, irrigation, soil texture and vegetation dynamic. The impact of the forcing variable was assessed by comparing the simulation using the large-scale dataset and the simulation using the local observation. The shortcoming of such organization is that the simulations were not compared to the same control simulation which does not facilitate the critical analysis of the results. In the revised version, we used the CTL simulation as the baseline to derive the rest of the simulations. The CTL simulation is achieved using the local observations for all the forcing variables and the in situ values for the soil properties. It is the closest simulation to the measurement (Garrigues et al., 2015) and is considered as "truth" for this work. To test the sensitivity of ET to each forcing variable, we replaced the local observations used in CTL by the large-scale dataset. The forcing variables were tested one by one. This new organization of the experiments allows to better identify the impact of each driver on ET. We modified Section 5 to explain the new organization of the simulations and the associated comparisons. We modified the characteristics of the experiments in Table 3. We ran theses new simulation cases and we updated the results. Results have slightly changed compared to the original version of the paper. But, their analysis leads to the same conclusions on the hierarchy of the impact of the tested driver on ET. Irrigation has the largest influence followed by vegetation dynamic and then by climate. We modified our discussion to better emphasize the key role of irrigation for land surface modelling over cropland. We illustrated the metrics presented in Table 4 by a Taylor diagram to show the hierarchy of the impact of each driver on ET at various time scales (half-hourly, daily, monthly, yearly). Since all simulations are now compared to the CTL simulation, we removed Table 5 which gave the performance scores against eddy-covariance measurements. We revised the text for grammar and clarity. We removed redundancies between Introduction, Section 3 and Section 5. We re-organized the result and discussion sections (cf. Answer to referee-1)

Specific comments

- Pg-2057, line 26-28: I think it would be clearer to define the original model first then build off of that to describe the latest version.

We removed the description of the model in Introduction to avoid redundancies with Section 3 dedicated to the description of the model. In Introduction, we stated that we use the ISBA-A-gs land surface model. In Section 3, we started by describing ISBA then we described its A-gs (coupled photosynthesis-stomatal conductance model) version.

- Pg-2059, line 15: Table 1 is very helpful, but I get very little out of Figure 1. What are you trying to show in figure 1 that is not given in Table 1?Âă

Figure 1 illustrates the crop succession which is a key characteristic of this experiment. It shows how the succession of winter and summer crops results in long periods of inter-crop period for which the soil is bare. This aspect is not explicitly shown byTable 1. We think that Fig. 1 can help the readers to better understand how the experiment

was designed. We clarified the references to Fig.1 in Section 2.1.

- Pg-2061, lines 6-8: Define all depths, not just the first one.Âă

We gave the depth of the superficial layer in the general description of the model since it keeps a fixed value in the model. Conversely, the depths of the root-zone and the width of the deep reservoir are location-dependent. We provided the root-zone depth and the deep reservoir size used in this experiment in Section 5.1 dedicated to the model implementation at the Avignon site.

- Pg-2061, lines 9-10: This is very vague and provides little technical info on how the model calculates the carbon and water fluxes. Isn't stomatal conductance needed for net assimilation? So does the model solve the equations through iterations? What are the equations? Is this coupled to the latent heat flux? Since this paper focuses on ET I think there needs to be more details about how ET is calculated in the model.

The total latent heat flux is simulated by computing separate soil evaporation and plant transpiration fluxes. We provided detailed explanations on how soil evaporation is computed in Garrigues et al., 2015 (in press for hess). The stomatal conductance used to compute the plant transpiration is simulated using the A-gs version of ISBA. A-gs explicitly represents the functional coupling between the stomatal conductance (gs) and the net assimilation of CO2 (A) (Calvet et al., 1998). The net assimilation of CO2 is first computed at the leaf scale accounting for the limiting effects of the air CO2 concentration and the radiation. The first-guess of the stomatal conductance for CO2 and water vapor are derived from the net assimilation of CO2 using a flux-gradient relationship which accounts for the effect of air humidity deficit on stomatal aperture. The stomatal conductance is corrected to take into account the interactions between the diffusion of CO2 and the diffusion of water vapor through an iterative process. The impact of the soil water stress on the stomatal conductance is represented through two types of plant response to drought, depending on the evolution of the plant water use efficiency. For drought-avoiding type of plant (e.g. C3 crops), the stomatal conductance and the plant transpiration are reduced by increasing the sensitivity of stomatal aperture to air humidity deficit while the net assimilation of CO2 is kept up by increasing the mesophyll conductance. This tends to increases the plant water use efficiency. For drought-tolerant type of plant (e.g. C4 crops), the stomatal conductance is increased while the net assimilation of CO2 is depleted. For this strategy, the water stress has a low impact on the plant water use efficiency which is unchanged or slightly reduced. Under a critical fraction of the root-zone water reservoir, severe stress is triggered and both the net assimilation of CO2 and plant transpiration are depleted. The stomatal conductance and photosynthesis are finally upscaled to the canopy using a simple radiative transfer scheme described in Jacobs et al., 1996. We re-wrote Section 3 dedicated to the model description. First, we described the original ISBA model and then the A-gs version of ISBA as suggested above. We provided more detailed explanations on how photosynthesis and stomatal conductance are computed and parametrized. We chose to provide no equations for sake of concision. We gave the key paper and technical report references where model's equations can be found.

- Pg-2064, lines 20-23: Does this mean the land cover in the model was changed according to the crop schedule shown in Table 1? Is the change in land cover consistent with the LAI forcing? I think this section should be revised to be more explicit as to how this is done.

The 12-year period was split into sub-simulation periods which correspond to crop and inter-crop periods. We explicitly represent the succession of crop and inter-crop periods in the simulations by changing the model land cover and the associated LAI accordingly to the crop schedule presented in Table 1. The C3 crop patch was used to represent wheat, pea, and sunflower. The C4 crop patch was used to represent maize and sorghum. Inter-crop periods are represented by the bare soil patch. When the LAI climatology is used, we used the LAI cycle provided by ECOCLIMAP-II for each model land cover. When the local LAI is used, the LAI time trajectory consistently depicts the dynamic of the crop succession. LAI is null for the inter-crop periods. We re-wrote

the sub-Section 5.1 dedicated to the model implementation at the Avignon site. We clarified how the crop succession is represented in the simulations through consistent changes in both land cover and LAI.

- Pg-2065, line 1: This section should start by describing the control run and then progressively discuss how the individual runs vary from the control run and should be consistent with Table 3.Âă

As mentioned above, we re-organized the simulation cases. All tested simulations are compared to the control run (CTL). We modified Section 5.2. We started by describing CTL which is achieved using the local observations for each forcing forcing variable. Then we explained how the individual simulations are derived from CTL by replacing the local observations by the large-scale data sets for each forcing variable. We modified Table 3 which describes the characteristics of the new simulations.

- Pg-2065, line 7: The caption in Table 3 is too long and needs to be revised. I also find it difficult to read the second column, as I cannot tell where the rows end and start. Also I think it would be best to have the control run first followed by the other simulations starting with the ones most similar to the control. It might also be helpful to bold the aspects that are different from the control.

We modified Table 3 to reflect the new organization of the experiments given above. The control run (CTL) is presented first. Then, the simulations derived from CTL are given and are organized per type of driver (climate, irrigation, vegetation, soil parameters). For each simulation we used bold style to highlight the variable that is different from CTL. We improved the presentation and clarity of Table 3

Pg-2065, line 22: I find clim reference confusing as it makes me think of a “Climatology”. I would suggest using in-situ or local instead."

We removed clim. We changed the naming of the simulations to reflect the new organization of the experiments. The new name indicates which variable is changed from the control run. For exampleÂă: SAFRAN means that the SAFRAN climate replaces the local climate used in CTL. The new names of the simulations are defined in Section 5.2.

- Pg-2067, line 7: So the control run is considered as truth? I would be more direct about saying that.

The control run is the simulation the closest to the eddy-covariance measurements of ET. Its performances were evaluated in a previous paper (Garrigues et al., 2015, in press for HESS). In the revised version, we removed the evaluation against eddy-covariance measurements and we considered CTL as truth to evaluate the other simulations. We clarified this point in the text.

- Pg-2067, lines 13-16: I think it would be more consistent and clearer to use the same metrics for both validations and essentially the only difference would be what is considered truth. What is the difference between the Mean Difference and the BIAS, it is unclearÂă.

We removed the comparison with eddy-covariance measurements. All simulations are evaluated against CTL considered as truth. Mean difference and bias are identical metrics. We chose to use the term 'mean difference' in the revised version and we removed the term bias.

-Pg-2068, line 2: I find Figure 2 too complicated as each line is referenced to something else. It makes it difficult to easily see differences and associated them with differences in the simulations. I think it would be a lot simpler to show all simulations relative to the control run. This would also make it easier to relate back to Table 3 to understand the differences.

We modified Fig. 2 as suggested. We displayed the differences in cumulative ET between each simulation and CTL. This new figure clearly shows the hierarchy of the impact of the tested drivers on ET. Irrigation and vegetation have the greatest influence

while changing the climate forcing dataset has little influence on ET simulation.

-Pg-2068, line 9: I don't think these tables are effective at communicating the results and this information would be much better as a Figure illustrating the key results and Pg-2068, line 23: Again tables are not an effective way of communicating results. I know you can't show all the results in figure, but showcase the most important results.

We removed Table 5 which was providing the simulation performances evaluated against measurements. We modified Table 4. New Table 4 provides the metrics quantifying the scattering between each simulation and the control run. We provide the mean difference (MD) and the SD of the differences (SDD). We removed the correlation coefficient which brings little to the analysis. We removed the RMSD which is linked to MD and SDD by $RMSD^2=MD^2+SDD^2$. New Table 4 is now simplified and its reading is facilitated. We kept Table 4 in the revised version since it brings quantitative information which are complementary to Fig. 2. To illustrate the key results of Table 4, we used a Taylor diagram which shows the impact of the forcing on ET at various timescales.

- Pg-2069, line 5: I think it would be better to discuss the differences in the forcing before looking at the evaporation since the differences in the forcing are likely directly related to the difference in evaporation. Also, what is "water flux", flux from what to what, if you mean precipitation as is mentioned in the caption then use that and be consistent throughout.

We modified the structure of the Result section. In the revised version, we start by analysing the differences between the large-scale and the local values for each forcing and the differences between large-scale datasets for the climate. Then, we analyse the impact on ET. We corrected the y-label in Figure 5 and we used rainfall instead of water flux.

- Pg-2069, line19: Same with this table 7, too much information, just show what is important with a figure.

Table 7 is already illustrated by Fig. 4 which shows the differences in hourly and seasonal downwelling shortwave radiations, Fig. 5 which depicts the differences in hourly and seasonal downwelling longwave radiations. We simplified Table 7 by removing the results on net radiation (NR). The latter can be derived from the analysis of the differences in the radiation forcing variables. We mentioned the results on NR in the text.

- Pg-2069, line 26: Figure 6 caption is too long and needs to be condensed. Also add a zero line for reference.

We reduced the caption of Figure 6 and we added a zero line.

- Pg-2073, line 10: Figure 8, how do you have negative irrigation? That doesn't make much sense am I missing something? Please clarify.

There is no negative irrigation. We corrected the y-axis limits in Fig. 8. This was an artefactÂă: when the standard deviation was larger than the mean, the lower part of the error bar was below zero.

- Pg-2077, line 28: What is the under study? Please clarify.

We replaced 'the crops under study' by 'the cropsÂă considered in this work (mainly wheat, maize, sorghum, sunflower)'

- Pg-2078, line 17: I think a little discussion as to the limitations and the big pictureÂăimplications of these results is needed.

We added the following sentences at the end of the conclusion to summarize the implications of our results.

'Our results showed that the key challenges for the spatial integration of a land surface model over Mediterranean cropland concern the representation of: the variability of irrigation practices in land surface model. Irrigation was proved to have the largest influence on long time-series of ET although it concerns only short period of time of

the crop succession. the spatial distribution of the soil hydrodynamic properties at the regional scale, the vegetation dynamic at seasonal (phenology) and interannual time scales, the spatiotemporal variability of rainfall which can be particularly important for Mediterranean climate characterized by local convective elements.. Strategy combining models and new remote sensing observations with high spatial resolution (∼10-20 m) and high temporal frequency (5-10 days) offer great promises to resolve vegetation dynamic and retrieve the spatial distribution of soil properties for cropland.'

Technical corrections

- Pg-2055, lines 18-20: This sentence needs to be revised for clarityÂă

We modified this sentence as followsÂă: 'It is an essential information to represent air temperature and air humidity of the surface boundary layer (Noilhan et al., 2011) and to monitor river discharge (Habels et al., 2008)'

- Pg-2055, line 21: Should be "a" before Land surface modelÂă

We added it.

- Pg-2055, line 22: Should be "scales"

We replaced time scale by 'timescales'

- Pg-2055, line 23: What do you mean couple an LSM to a hydrology model? Do you mean a routing model that produces streamflow from gridded runoff ?

This is what we meant by LSM-hydrological coupled model.

- Pg-2055, lines 26-27: Needs to be revised for clarity

We modified the text as followsÂă: 'The forcing variables concern the climate and the land surface characteristics. They are generally provided by large-scale datasets which are characterized by coarse spatial resolution (10-50 km). These datasets may be not accurate enough to resolve the spatial and temporal variability of ET at a regional scale.'

- Pg-2056, line 6: Most reanalysis use a coupled atmosphere-ocean-land model, not just and atmospheric model.Âă

We replaced 'atmospheric model' by ' coupled atmosphere-ocean-land models'

- Pg-2056, lines 13-16: Sentence needs to revised for clarityÂă

We replaced 'Zhao et al. (2011) found median Mean Absolute Error (MAE) values ranging from 0.5 °C to 2 °C for 4 reanalysis datasets evaluated over 6 French sites.' by 'Zhao et al. (2011) have evaluated 4 reanalysis datasets over 6 French sites. For air temperature, they found Mean Absolute Errors (MAE) which range from 0.5 °C to 2 °C'

- Pg-2056, line 17: Not sure "Besides" is the right transition word here.Âă

We removed besides and modified the structure of the sentence. We replaced 'Rainfall and radiation are frequently reported as the most uncertain variables (Szczypta et al., 2011; Bosilovich et al., 2013b). Besides, they are two main external drivers of ET (Teulling et al., 2009; Miralles et al., 2011).' by 'Rainfall and radiation, which are two key external drivers of ET (Teulling et al., 2009; Miralles et al., 2011), are frequently reported as the most uncertain variables (Szczypta et al., 2011; Bosilovich et al., 2013b).'

-ÂăPg-2058, line 1: Define A-gs

In the revised version of the paper, ÂńÂăA-gsÂăÂż is defined p6, line 4-7 byÂă: 'We use the Interactions between Soil, Biosphere, and Atmosphere (ISBA) land surface model (Noilhan and Planton, 1989; Noilhan and Mahfouf, 1996) in its A-gs version (coupled photosynthesis-stomatal conductance model) (Calvet et al., 1998).'

- Pg-2058, line 7: It is usually called "Hydrologic monitoring" not hydrology monitoring.Âă

We modified the text. We used hydrological monitoring (p6, l12 of the new version)

- Pg-2058, line 11: What is meant by a long period of time? This is very subjective.Âă

We agree and we replaced long period of time by 12-year.

Interactive comment on Geosci. Model Dev. Discuss., 8, 2053, 2015.