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Paper: gmd-2014-260
Title: Impact of climate, vegetation, soil and crop management variables on multi-year ISBA-A-gs simulations of evapotranspiration over a Mediterranean crop site.

Status: major revision

Avignon, 15 Jully 2015,

Dear Editor,

We would like to thank you and the two anonymous referees for the review of our paper. We have carefully considered the remarks given by the referees. They helped us to improve the clarity and the quality of the paper.
We improved the structure of the paper as suggested by both reviewers. As suggested by referee-2, we re-organized the experiments to compare each simulation to the control run considered as the reference. We simplified the numeric results in tables which are now better illustrated by additional figures (Fig 3, 11, 12). Below, we provide detailed answers to each of the referee's comments. The modifications are highlighted in yellow in the text, except from Section 5 to 8 which have been totally modified.

1/Response to the comments from referee-1

1. ***"Please remove the unnecessary duplication of descriptions in the introduction, the model section, results and discussion sections."***

   We improved the structure of the paper and we removed redundancies. The changes are highlighted in the revised manuscript.

2. ***"A small map of the field site helps the reader to get an impression of the location."***

   We added a map of the field site which includes the location of the measurements.

3. ***"I suggest to summarize the ISBA model properties in a table."***

   We chose to not include this Table for sake of brevity. We improved the description of the model and we give key references where exhaustive description of the model properties can be easily found.

4. ***"The evaluation metrics with table 4 alone is too abstract for the reader. Here some scatter plots could help to see the distribution of ET values in the different simulations. It would be nice to see also other time scales, e.g. monthly and seasonal values."***

   We added four scatter plots to illustrate the scattering of ET values generated by the main investigated drivers (new Figure 11):

   - climate: SAFRAN versus local climate.
   - irrigation: No irrigation versus irrigation
   - vegetation: ECOCLIMAP-II versus local vegetation
   - soil parameters: Pedotransfer estimate of the soil hydrodynamic parameters versus estimates of the soil parameters derived from in situ measurements.

   We computed the metrics at the monthly and seasonal timescales that are reported in new Table 6 (new version of Table 4 in the submitted version).

We improved the presentation and the clarity of Table 4 (see response to referee-2). We removed the correlation which adds little to the analysis. We also removed RMSD which is not necessary since we provide the systematic (MD) and random (SDD) component of the total scattering quantified by the RMSD. Results of Table 4 are organized by type of forcing: climate, soil and irrigation using bold character. Table 4 is Table 6 in the revised version.

5. ***"The results section "Impact of soil texture" is rather short. It could be enlarged by a comparison of the results with results of other long-term simulations (e.g., Smiatek et al., 2015, DOI: 10.1127/metz/2015/0594)."***

   We discuss the structure of the pedotransfer function used in ISBA. Our results show the limitations of this pedotransfer function to properly resolve the spatial variability in available water capacity and thus to describe the ET dynamics. We included the reference provided by the referee in the Discussion section (see the first sub-section dedicated to the soil properties).

6. ***"Furthermore I suggest to combine the sections "Results" and "Discussion" to avoid duplication and to extend the discussion of the results in comparison with long-term simulations, e.g. Guillod et al., 2013 (***http://dx.doi.org/10.1007/s00382-012-1395-z***)"***

   We re-organized the result and discussion sections to avoid redundancies and improve the clarity of these sections. The new result section is divided into two parts. First, we analyse the uncertainties in the large-scale drivers. Then, we analyse the impact of the drivers on ET. We moved the interpretation from the discussion section to the result section. The discussion is now focused on the implications of our results regarding the spatial variability of the forcing variables and the application of the model at larger scale. We included a comparison with other long-term simulation studies in the discussion as suggested.

7. ***"Minor comments:Please check spelling in Table 1. "radiation" 2. The fonts in figures 4 and 6 are rather small compared to the other figures"***

   We checked and corrected spelling in Tables. We increased the fonts of Fig 4 and 6.

2/Response to the comments from referee-2

2.1 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" in this work. To test the sensitivity of ET to each forcing variable, we replaced the local observations used in CTL by the values derived from the large-scale datasets used in the standard implementation of the model. 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. The soil parameters and irrigation have the largest influence followed by vegetation dynamic and the climate variables. We modified our discussion to better emphasize the key role of irrigation for land surface modelling over cropland.

- Since all simulations are now compared to the CTL simulation, we removed Table 5 which gave the performance scores against eddy-covariance measurements.
- We chose to keep the Tables which provides meaningful numerical results for the critical analysis of the results. But we simplified these Tables and we added new Figures (Fig. 3, 11 and 12) to showcase the key results.
- 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)

2.2/ 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 Section 3, we start by describing ISBA then we describ 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? »***

  Figure 1 (Fig. 2 in the revised paper) 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 inter-crop periods for which the soil is bare. This aspect is not explicitly shown by Table 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. »***
  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). 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 CO_2 (A) (Calvet et al., 1998). The net assimilation of

CO_2 is first computed at the leaf scale accounting for the limiting effects of the air CO_2 concentration and the radiation. The first-guess of the stomatal conductance for CO_2 and water vapor are derived from the net assimilation of CO_2 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 CO_2 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 CO_2 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 CO_2 is depleted. For this strategy, the plant water use efficiency is reduced. Under a critical fraction of the root-zone water reservoir, severe stress is triggered and both the net assimilation of CO_2 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 ac-cording 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 pro-gressively discuss how the individual runs vary from the control run and should be consistent with Table 3. »***

  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 values taken from the large-scale datasets 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 changed variables of the control run. 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 example : S_{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 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 unclear »**
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 (Fig 10 in the revised manuscript) 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.

- **« 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 that becomes Table 6 in the revised version. New Table 6 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 $RMSD^2=MD^2+SDD^2$. The new Table 6 is now simplified and its reading is facilitated. We kept this Table in the revised version since it brings quantitative information which are complementary to Fig. 2 (new Fig 10). 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 (new Fig. 4 in the revsied manscript)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 (new Table 5) is already illustrated by
  - new Fig. 6 which shows the differences in hourly and seasonal downwelling shortwave radiations,
  - new Fig. 7 which depicts the differences in hourly and seasonal downwelling longwave radiations.

We simplified the new Table 5 by removing the results on net radiation. We mentioned the impacts of biases in radiation on net radiation 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 (new Fig. 9). This was an artefact : when the standard deviation was larger than the mean, the lower part of the error bar was falling below zero.

- **« Pg-2077, line 28: What is the under study? Please clarify. »**
  We replaced « the crops under study » by « the crops 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 picture implications of these results is needed. »**
  We added the following sentences at the end of the conclusion to summarize the implications of our results.
  « This work shows that the key challenges for the spatial integration of a land surface model over Mediterranean cropland concern the representation of:
  - the spatial distribution of the soil hydrodynamic parameters which control the available water capacity and the soil hydraulic diffusivity.
  - The variability of irrigation practices in land surface model. Irrigation was proved to have large influence on long time-series of ET although it concerns short period of time of the crop succession.
  - The spatiotemporal variability of rainfall which can be particularly important for Mediterranean climate characterized by local convective elements.
  - The vegetation dynamic at seasonal (phenology) and interannual (crop rotation) timescales.
- 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 and need to be fostered in the future.»

2.3/ Technical corrections

- **« Pg-2055, lines 18-20: This sentence needs to be revised for clarity »**
  We modified this sentence as follows :
  «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 model »**
  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 follows :
  "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. »**
  We replaced 'atmospheric model' by ' coupled atmosphere-ocean-land models'

- « **Pg-2056, lines 13-16: Sentence needs to revised for clarity** »
  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.** »
  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).”

- **« Pg-2058, line 1: Define “A-gs” »**
  In the revised version of the paper, « A-gs » is defined p6, line 4-7 by :

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