

## Authors response to reviewer1

Manuscript review for HESS-2013-444: "Irrigated plantations and their effect on energy fluxes in a semi-arid region of Israel a validated 3D model simulation" by Oliver Branch et al.

General comments:

The manuscript investigated the impact of a large-scale plantation on energy fluxes in a semi arid region using the WRF/Noah LSM model.

The authors thank the reviewer for examining our submission and for the thoughtful and relevant comments given, and we would like to address each of the points in turn. Please note that any line numbers referred to here, correspond to the original submitted manuscript (as suggested by the reviewers) and not the proofread online version.

A three-month simulation was carried out, and the model was validated from different aspects based on extensive observations. However, the key conclusions and the model configurations still need further improvement. I would suggest a major revision of this manuscript before possible consideration in the publication of HESS.

The authors have considered carefully the concerns of the reviewer and seek to clarify some key points.

The total area of the actual planation is 4 km<sup>2</sup>, not a "large scale" issue as claimed by the authors.

It was not intended to claim that the observed plantation in Israel constitutes a large-scale issue as such. Rather, a large scale issue would be the implementation of similar but much larger plantations of 10,000 km<sup>2</sup> and their implications for climate change mitigation, mesoscale and regional climate impacts. This is detailed in line 43-46 of the introduction.

In addition, the authors set up only one domain of WRF with the grid spacing 2 km, which is still coarse based on the purpose of this study. I would suggest the authors set up a finer domain or several nested domains, so as to get a better representation of the land surface properties. The 100 km<sup>2</sup> plantation in the IMPACT scenario might not be comparable to the 4 km<sup>2</sup> plantation in reality; the atmospheric impact might differ substantially. I noticed that the authors also realized this disadvantage (Line 307-316, Note: the line numbers refer to the submitted manuscript instead of the discussion paper published online).

For context, we would like to reiterate that the aim is to obtain a reasonable representation of the diurnal energy balance in WRF-NOAH over simulated 'irrigated vegetation' and 'desert scrub' surfaces, and not to explicitly simulate the observed 4 km<sup>2</sup> plantation in terms of detailed spatiotemporal phenomena. To clarify this, the following text is now included in the text in Section 4.2 'Irrigated plantations in NOAH' at line 301:

*"It should be emphasized that the intention is not to simulate detailed spatiotemporal phenomena over the actual 4 km<sup>2</sup> observed plantation, where a more explicit resolution could be*

*more appropriate. Rather we seek a good statistical representation of the diurnal fluxes over a homogeneous plantation.”*

In order to simulate the energy balance, the model was first parameterized based on literature on relevant plant species and their resistances, soil surveys and data on irrigation. The model energy balance is verified against surface observations from existing plantations and a desert to make sure they are of a reasonable magnitude and to further adjust parameters.

The primary use for our verified model is to simulate large scale plantations (order of  $100 \times 100$  km) within a  $400 \times 400$  km domain and to investigate their impacts on convection (in a later study). To draw robust conclusions from such an impact study, it is vital that the fluxes are of the correct order of magnitude within the simulations. However, validation with plantation data at these scales is not really feasible since plantations of such sizes and with similar irrigation, do not yet exist. With our methodology, we bridge the gap between a reasonable verification study and the large scale simulations that we want to execute. Doing so requires that the model configuration is consistent between the verification and the impact runs, in terms of grid and domain. Otherwise, if we simply upscale parameters which are validated at fine scales to coarser grids, the results may not be reliable or even physically consistent. This is the reason that using very fine scale grids for this study is not appropriate i.e. they cannot be feasibly scaled up to domains of  $400 \times 400$  km<sup>2</sup> or more, especially for seasonal time scales.

The 2 km grid spacing used here, approaches an explicit ‘convection permitting scale’ and is likely to be fine enough to simulate the mesoscale processes we are ultimately interested in, e.g., mesoscale circulations and single convective cells. At the same time, by not decreasing the grid spacing more, we largely avoid the ‘grey zone’ scenario of simultaneous parameterization and explicit resolution of turbulence. This configuration is also feasible in terms of resources when multiple runs need to be carried out over a seasonal time scale, whereas nested approaches using a series of finer scales would not be possible over such periods.

A  $2 \times 2$  km<sup>2</sup> patch of vegetation can only be represented by one grid cell at this spacing, but Pielke (2002) suggests that at least four grid cells across are desirable to simulate any one feature (Pielke reference added at line 301). For this reason, and also to allow for some spatial averaging we chose an area of  $5 \times 5$  grid cells to represent the vegetation. These cells are completely homogenous in terms of land surface properties and soil moisture, to reflect an irrigated monoculture. We have now added the following section at line 314 to mention informal comparison tests we carried out to assess representativeness:

*“To check our assumptions on representativeness we later compared fluxes from the  $5 \times 5$  plantation with those from a  $1 \times 1$  plantation (over a week), and the diurnal cycles and variability were not significantly different.”*

I would also suggest the language of the manuscript go through a further refinement.

The main author of the document is a British native English speaker, so we hope and maintain that the language should be clear and understandable. British English does have some distinct phrasing, which is sometimes not used by others, even by American English speakers. However, every effort has been made to keep the language as international as possible to all readers. If there are any specific edits which are recommended to aid understanding though, then we will of course address them.

Specific comments:

1) Line 6, “land surface atmosphere feedbacks” should be “feedbacks between land surface and atmosphere”

Agreed. This has now been amended accordingly.

2) Line 6, “the 2012 summer season” should be “the summer season of 2012”

This has been amended.

3) Line 36-39, please reconstruct that sentence.

This has been amended by splitting the sentence and clarifying the content. It has been changed from:

*“Furthermore, the high plantation T2 magnitudes highlight the importance of considering diurnal dynamics, which drive the evolution of boundary layers, rather than only on daily mean statistics which often indicate an irrigation cooling effect.”*

to the following:

*“Furthermore, increased daytime T2 over plantations highlight the need for hourly as well as daily mean statistics. Daily mean statistics alone may imply an overall cooling effect due to surplus nocturnal cooling, when in fact a daytime warming effect is observed.”* (line 36-39)

4) I would suggest the authors provide a concise abstract. For instance, the statistics of validation results do not need to be mentioned.

The authors agree with the reviewer that the abstract was too long. However we would like to keep a summary of the results in the abstract if possible. Nevertheless, in line with the reviewer’s suggestion, we have now streamlined the abstract from 473 words down to 341. It now reads:

*“A 10 × 10 km irrigated biomass plantation was simulated in an arid region of Israel, to simulate diurnal energy balances during the summer of 2012 (JJA). The goal is to examine daytime horizontal flux gradients between plantation and desert. Simulations were carried out within the coupled WRF-NOAH atmosphere/land surface model. MODIS land surface data was adjusted by prescribing tailored land surface and soil/plant parameters, and by adding a novel, controllable sub-surface irrigation scheme to NOAH. Two model cases studies were compared - Impact and Control. Impact simulates the irrigated plantation. Control simulates the existing land surface, where the predominant land surface is bare desert soil. Central to the study is parameter verification against land surface observations from a desert site and from a 400 ha Simmondsia chinensis (Jojoba) plantation. Control was verified with desert observations and Impact from Jojoba observations. Model evapotranspiration was verified with two Penman-Monteith estimates based on the observations.*

*Control simulates daytime desert surface 2m air temperatures (T2) to 0.2 °C deviation, vapour pressure deficit (VPD) to 0.25 hPa, wind speed (U) to 0.5 ms<sup>-1</sup>, and surface radiation (R<sub>n</sub>) to 25 Wm<sup>-2</sup>, soil heat flux (G) to 30 Wm<sup>-2</sup>, and 5cm soil temperatures (ST5) to 1.5 °C. Impact simulates T2 over irrigated vegetation to 1 - 1.5 °C, VPD to 0.5 hPa, U to 0.5 ms<sup>-1</sup>, R<sub>n</sub> to 50 Wm<sup>-2</sup>, G to 40 Wm<sup>-2</sup> and ST5 to within 2 °C. Latent heat curves in Impact correspond closely with Penman-Monteith estimates, and magnitudes of 160 Wm<sup>-2</sup> over the plantation are usual. Sensible heat*

*fluxes, are around 450 Wm<sup>-2</sup> and are at least 100 - 110 Wm<sup>-2</sup> higher than the surrounding desert. This surplus is driven by reduced albedo and high surface resistances, and demonstrates that high evaporation rates may not occur over Jojoba if irrigation is optimized. Furthermore, increased daytime T<sub>2</sub> over plantations highlight the need for hourly as well as daily mean statistics. Daily mean statistics alone may imply an overall cooling effect due to surplus nocturnal cooling, when in fact a daytime warming effect is observed.”*

5) Line 106-113, please provide necessary references for this paragraph.

Six new references and two new citations have been added in this section (line 107-108) to source the information on effects of land surface and plant properties on albedo and flux partitioning:

Beringer, J., 2010. Ecological Climatology: Concepts and Applications, 2nd Edition – By Gordon B. Bonan. pp.158–160. Geogr Res. doi:10.1111/j.1745-5871.2009.00640.x

Burt, P.J.A., 2002. Introduction to micrometeorology. By S. Pal Arya. Academic Press. Second edition, 2001. [pp 381-382]. xxv + 420 pp. ISBN 0 12 059354 8. Q J R Meteorol Soc, 128(581), pp.1039–1040. Available at: <http://dx.doi.org/10.1256/0035900021643665>.

Ingwersen, J., Steffens, K., Högy, P., Warrach-Sagi, K., Zhunusbayeva, D., Poltoradnev, M., Gäbler, R., Wizemann, H.-D., Fangmeier, A., Wulfmeyer, V., Streck, T., 2011. Comparison of Noah simulations with eddy covariance and soil water measurements at a winter wheat stand. Agr Forest Meteorol, 151(3), pp.345–355. Available at: <http://linkinghub.elsevier.com/retrieve/pii/S0168192310003114> [Accessed January 21, 2014].

Raupach, M. and Finnigan, J., 1996. Coherent eddies and turbulence in vegetation canopies: the mixing-layer analogy. Boundary-Layer Meteorol, pp.351–382. Available at: <http://www.springerlink.com/index/Q0N5241788821X61.pdf> [Accessed July 17, 2012].

Silva, E. N., Ribeiro, R. V., Ferreira-Silva, S. L., Viégas, R. A. and Silveira, J. A. G., 2010. Comparative effects of salinity and water stress on photosynthesis, water relations and growth of *Jatropha curcas* plants. J Arid Environ, 74(10), pp.1130–1137. Available at: <http://linkinghub.elsevier.com/retrieve/pii/S0140196310001783> [Accessed March 10, 2013].

Zhang, Y., Wang, X., Pan, Y., Hu, R., 2013. Diurnal and seasonal variations of surface albedo in a spring wheat field of arid lands of Northwestern China. Int J Biometeorol, 57(1), pp.67–73. Available at: <http://www.ncbi.nlm.nih.gov/pubmed/22392839> [Accessed January 21, 2014].

6) Line 121-122, please rephrase the sentence.

This has now been rephrased from:

*“The use of coupled 3D models is desirable because, unlike uncoupled models they can simulate bi-directional feedbacks, from the soils to the entrainment zone.”*

to

*“The use of coupled 3D models is preferable to the use of uncoupled models, where a land surface model is forced uni-directionally with atmospheric forcing data. This is because uncoupled models neglect the simultaneous feedbacks which occur between the surface, boundary layer and entrainment zone.”*

(see line 121)

7) Since the authors devised a new irrigation scheme within Noah LSM, I would suggest the authors provide more information or comment on the existing irrigation schemes in the Noah model in the introduction part. The readers might find it difficult to capture the highlights of the paper in the present form.

There has not been to date, an official release of WRF with an irrigation scheme implemented in NOAH or the newer NOAH-MP (multi-physics) model. Irrigation is partially addressed by the optional USGS (US Geological Survey) land surface dataset through the use of various land surface physical parameter sets designed to account for irrigation, but this is not a controllable irrigation scheme per se. There have been one or two recent studies, where irrigation schemes have been simulated by modification of WRF-NOAH although using different methods to our own (Harding and Snyder 2012; Sridhar, 2013). These contributions are now cited in our paper.

The scheme we have implemented is novel because of its sub-surface mechanism i.e. water is added only to sub-surface layers, unlike for instance Sridhar’s scheme which simulates surface watering. Sub-surface irrigation is used in the actual Jojoba plantation and means that ostensibly there is no direct surface evaporation and that the only significant source of latent heat comes from transpiration. The scheme is based on a minimum water stress value as proposed by Choudhury and DiGirolamo (1998). This mechanism is described in the paper and we have now added the following text at the beginning of section of 4.2 (line 298) to incorporate this information:

*“At the time of our simulations there were no official releases of WRF with irrigation schemes implemented in the accompanying NOAH or the newer NOAH-MP (multi-physics) land surface models. Schemes have been devised by others independently though and incorporated into WRF for impact studies (see Harding and Snyder 2012; Sridhar, 2013). Sridhar for instance simulated two kinds of surface irrigation: flood and sprinkler systems. In our case, a controllable sub-surface scheme was required, to reflect the sophisticated system used in Israel and so this was developed as a sub-routine and incorporated into NOAH for this study.”*

8) Line 159-160, latitude/longitude, please save to only two decimal place.

This has been amended.

9) The readers could not interpret the diurnal variations of T and RH from Fig 2. I would suggest the authors change the presentations of this figure. I would also suggest only show the “mean” curves.

Fig.2 is presented to assess the evolution of mean, maximum and minimum temperatures and relative humidity over the season. The purpose of doing so is to assess whether aggregating the whole season into diurnal curves (and variance) is a valid approach. For instance, if the temperatures in June are very much colder than in August, there would be a good deal of variability in the diurnal curves, the origin of which would not necessarily be apparent. We can see that the mean temperatures rise and peak in July, but the shift is not so great as to invalidate

the use of diurnal curves which are presented in Fig.4. Fig.2 also allows us to further examine the differences between the sites in more detail.

In light of the reviewers comments however, these points have now been further emphasized with the following amended text in section 3.2 (line 220):

*“A summer time series of T2 and RH (Fig. 2) was examined from the three stations to assess the seasonal evolution of mean temperatures and relative humidity along with maxima and minima. The purpose of doing so is to reveal any major seasonal shifts, to assess the validity of examining seasonal diurnal curves (Fig. 4), and to explain some of the hourly variance.”*

Furthermore, small changes have been made in the caption of Figure 2.

10) Please modify the order of the subfigures in Fig 3, so as to match the text of Line 227-240.

Instead, the text has been re-ordered instead to match Fig.3 (line 227-250)

11) Please specify how to obtain the albedo in Fig 4.

Albedo values in Figure 4 is calculated from the individual observed radiation components as:  $SW_{UP} / SW_{DOWN}$ . The net radiation  $R_N$  is calculated from  $SW_{DOWN} - SW_{UP} + LW_{DOWN} - LW_{UP}$ . This information is now included in the text in Section 3.1 (line 213-218). This section title has also now been renamed from ‘Site description’ to ‘Site description and meteorological observations’.

12) Line 263-265, this sentence is misleading. The ultimate goal for this paper is apparently not as what the authors said. Please reconstruct it.

Agreed. Upon reflection it is perhaps a little ambiguous. Therefore this section has now been amended to clarify that the eventual goal of “assessing impacts on convection processes” is not in the scope of this paper, but will follow in a subsequent publication.

13) Line 272, “Fig 2.” probably should be Fig 5.

Thank you for spotting this error. It has now been amended.

14) Line 275, please specify what physics schemes were determined by sensitivity tests and how to determine that.

The following text has now been added to Section 4.1 ‘Modelling Configuration’ (line 267)

*“Model physics schemes were chosen with consideration to:*

- how relevant processes are dealt with and relevance to arid regions, land surface/atmosphere feedbacks and convection*
- experience and sensitivity tests within the working group and within the WRF model community*
- which variables are explicitly calculated by, and are output from the scheme.*

*Additionally some schemes are designed to be paired e.g. the SW and LW RRTMG schemes. The YSU (ABL) and Morrison 2-moment (microphysics) schemes have been used by our group for various applications in both arid regions (Wulfmeyer et al., 2014; Becker et al., 2013) and temperate regions (Warrach-Sagi et al., 2013). YSU is the default WRF ABL scheme. It is non-local, explicitly handles entrainment, and is generally thought to perform well in unstable*



*convective conditions (e.g. Shin et al., 2011; Hu et al., 2010) which is most relevant for examining the daytime fluxes. The MM5 surface layer scheme which computes surface exchange coefficients of heat, moisture, momentum using Monin Obhukov stability functions, and is to be paired with the YSU (or MRF) scheme.*

*The Morrison 2-moment microphysics predicts total number concentration of ice species and may improve the representation of ice crystal aggregation and ice cloud radiation representation (Morrison and Gettelman, 2008). One study (Molthan and Colle, 2012) which used Morrison with WRF and cited that it gave the minimum difference between simulated and accumulated precipitation during a convective storm when compared to 5 other schemes. However, it is not known if this improvement in representation of ice number concentrations would really improve simulations within our region of interest.”*

15) Line 286-287, please provide necessary references regarding to the spin-up period.

The following citations have been added:

Lim, Y.-J., Hong, J., Lee, T.-Y., 2012. Spin-up behavior of soil moisture content over East Asia in a land surface model. Meteorol Atmos Phys, 118(3-4), pp.151–161. Available at: <http://link.springer.com/10.1007/s00703-012-0212-x> [Accessed January 21, 2014].

Du, C., Wu, W., Liu, X. and Gao, W., 2006. Simulation of Soil Moisture and Its Variability in East Asia. In W. Gao & S. L. Ustin, eds. Soc Photo-Opt Instru (SPIE) Conference Series. p. 62982F–62982F–6. Available at: <http://adsabs.harvard.edu/abs/2006SPIE.6298E..67D> [Accessed May 14, 2013].

16) Fig 6. Where is cell X? Please clarify.

Cell X indicates the center cell of the 25 cell box which covers the plantation footprint. This is actually marked in green and not black as stated in the Fig.6 caption. This has been corrected and the term “Cell X” has now been replaced with “center grid cell”. This is hopefully now more understandable.

17) Line 307-316, the authors put forward the assumptions and also realized the uncertainty at the same time. This part is what I’m most concerned about, as stated in the general comments.

The authors believe that our methodology is consistent, given the constraints of the eventual upscaling of the model and of the availability of plantation size for observations and validation (please see the first comments made). Furthermore, we have stated our chain of reasoning to arrive at our chosen method. Limitations and assumptions by definition are present in all modelling studies, and we have tried to state any sources of uncertainty that we can identify and areas where further research would be beneficial. At the same time we maintain that the results have great value and move us in the direction of a more realistic simulation of irrigated arid plantations.

18) Line 320, I would also suggest the authors add more references, which might be useful and informative for the readers.

The following new references on species characteristics have been added here, along with more citations from already referenced literature:

Rajaona, M., Brueck, H. and Asch, F., 2013. Leaf Gas Exchange Characteristics of *Jatropha* as Affected by Nitrogen Supply, Leaf Age and Atmospheric Vapour Pressure Deficit. *J Agron Crop Sci*, 199(2), pp.144–153. Available at: <http://doi.wiley.com/10.1111/jac.12000> [Accessed January 24, 2014].

Niu, G., Rodriguez, D., Mendoza, M., Jifon, J., Ganjegunte, G., 2012. Responses of *Jatropha curcas* to Salt and Drought Stresses. *Int J Agron*, 2012, pp.1–7. Available at: <http://www.hindawi.com/journals/ija/2012/632026/> [Accessed January 27, 2014].

19) Line 405-407, according to the authors, only the soil moisture within 5\*5 cells were modified, while the other surrounding cells kept the same. In that case, the soil moisture field might not be continuous any more, will that influence the results? Please clarify this.

The sub-surface soil moisture fraction ( $\Theta$ ) within the plantations was re-initialized to wilting point ( $0.047 \text{ m}^3 \text{ m}^{-3}$ ) in order to ensure it starts below the irrigation target for soil moisture ( $0.18 \text{ m}^3 \text{ m}^{-3}$ ) at the start of the model run. The irrigation scheme adds water, to soil layers 2 and 3 based on the following logical statement: IF  $\Theta < 0.18$  THEN add water, IF  $\Theta \geq 0.18$  THEN do not add water. Given the very slow drainage of the soil (because of the homogenous wetting of a huge soil volume it would take many days for the soil moisture to decrease to that level, so increasing the spinup time. In any case, the soil moisture had reached 0.18 within the first day of the model run so the effect of drying the soil artificially was erased very quickly. We neglect lateral movement which is likely to be minimal at scales of 2km especially in this area of very low relief, and the NOAH model uses in any case a 1 dimensional bucket transport method.

Disparity between the plantation soil moisture and its surroundings is reasonable because the non-irrigated soils outside the plantation are in any case drier. The surface soil layer however was not re-initialised and was left at the initial surface soil values given by the forcing data. This layer would need to spin up freely.

The authors agree that perhaps this irrigation mechanism was perhaps not stated clearly enough and therefore the following text and the logical statement was added to the text in section 4.2 (Line 371) to emphasize how the water was added. The text now reads:

*“The  $\Theta$  level was replenished every 7 days to each sub-surface layer independently using the following logical statement: IF  $\Theta_{2,3} < 0.18$  THEN add water, IF  $\Theta_{2,3} \geq 0.18$  THEN do not add water”*

Furthermore, the following text was extended in section 4.3 ‘Soils in the plantations’ (line 405), so it now reads:

*“The 2<sup>nd</sup> and 3<sup>rd</sup> soil layers within the plantation boundary were re-initialised to wilting point (0.047)... “to ensure that initial levels are below the levels prescribed by the irrigation scheme ( $0.18 \text{ m}^3 \text{ m}^{-3}$ ). Otherwise it may have taken some time for the soil moisture to decrease to that level which would increase the spin up time.”*



20) Line 444-445, why the U is simulated well in CONTROL case, considering the “height” is not the same at all, 6m VS 10m? I would suggest weaken the validation of the wind simulation section.

Both reviewers have mentioned this. The authors agree with the reviewers and the U raw data has now been height extrapolated from 6m to 10m using the following neutral stability log profile method from WMO:

$$\frac{\overline{U}_1}{U_{ref}} = \ln\left(\frac{Z_1 - d}{Z_{0m}}\right) / \ln\left(\frac{Z_{ref}}{Z_{0m}}\right)$$

The Desert diurnal mean curve does not alter significantly from the uncorrected data, due to the low roughness and still agrees with the WRF estimates to within approximately  $0.5 \text{ ms}^{-1}$  over 24 hours. The Jojoba curve is shifted upward significantly and now matches the WRF curve more closely, to around  $0.5 \text{ ms}^{-1}$ . This method and reference has now been included in the manuscript in a new appendix (Appendix C) and the validation plot and caption has been amended (Fig. 8).

21) Line 521, “see 5.1...”, please redirect. Similarly, Line 528, “see 5.1.1...”

Thank you for noticing this error. It should redirect the reader to the appendices and has now been amended.

22) Line 530-532, please rephrase the sentence.

This has now been rephrased to:

“They tested methods with and without stability correction, and obtained fairly similar results, which corresponded closely with lysimeter and flux observations.”

23) Line 686-688, why not directly compare HFX over plantations and adjacent desert in the IMPACT scenario?

A new 2D plot (Fig.12) has now been added showing sensible and latent heat magnitudes in WRF Impact to directly compare fluxes over the plantation with the surroundings:

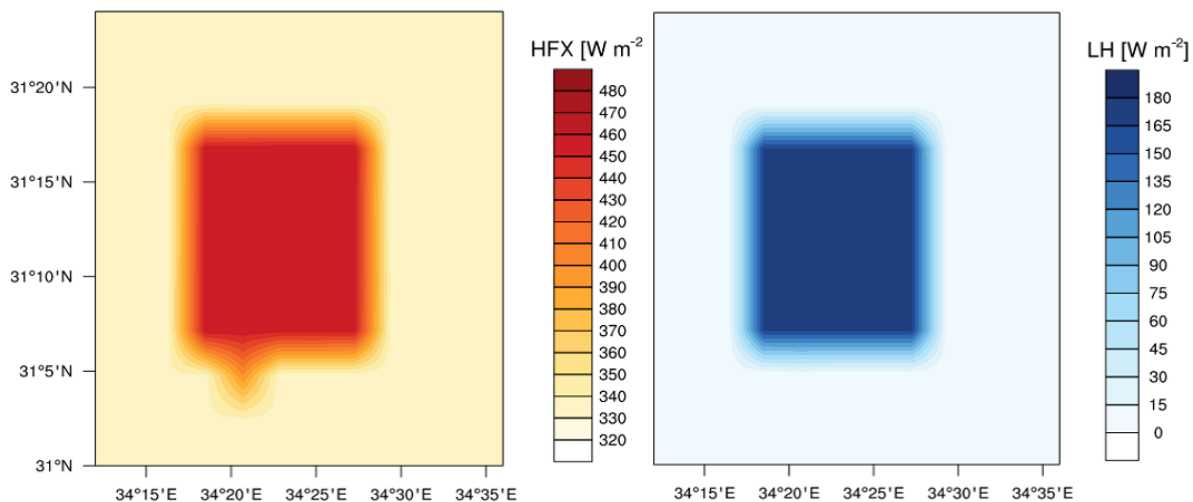


Figure 12. Mean daily maximum of sensible and latent heat flux (JJA) in WRF Impact to show the spatial gradient between the plantation and the surrounding desert. Values of HFX over plantation and desert are around 460 and 330  $\text{Wm}^{-2}$  respectively (a 130  $\text{Wm}^{-2}$  gradient). Values of LH over plantation and desert are around 165 and 0  $\text{Wm}^{-2}$  respectively (165  $\text{Wm}^{-2}$  gradient).

24) I would suggest merge section 6 and 7.

Thank you for this suggestion. This has been done and the text flows more smoothly now.

25) Fig 5. "care was taken...", this sentence should be removed from the caption.

Agreed. This has been amended.

We sincerely thank the reviewer for the useful expertise given and hope that we have addressed and clarified the main points and made all changes required to make the manuscript suitable for publication in HESS.

## References used in the response to reviewer 1

Pielke Sr., R.A., 2002: Mesoscale meteorological modeling. 2nd Edition, Academic Press, San Diego, CA, 676 pp.

Choudhury, B.J. and DiGirolamo, N.E., 1998. A biophysical process-based estimate of global land surface evaporation using satellite and ancillary data I. Model description and comparison with observations. *J Hydrol*, 205(3-4), pp.164–185. Available at: <http://linkinghub.elsevier.com/retrieve/pii/S0022169497001479>.

Molthan, A.L. and Colle, B.A., 2012. Comparisons of Single- and Double-Moment Microphysics Schemes in the Simulation of a Synoptic-Scale Snowfall Event. *Monthly Weather Review*, 140(9), pp.2982–3002. Available at: <http://dx.doi.org/10.1175/MWR-D-11-00292.1>.

Morrison, H. and Gettelman, A., 2008. A New Two-Moment Bulk Stratiform Cloud Microphysics Scheme in the Community Atmosphere Model, Version 3 (CAM3). Part I: Description and Numerical Tests. *J Clim*, 21(15), pp.3642–3659. Available at: <http://journals.ametsoc.org/doi/abs/10.1175/2008JCLI2105.1> [Accessed March 15, 2013].

Becker, K., Wulfmeyer, V., Berger, T., Gebel, G., and Münch, W., 2013. Carbon farming in hot, dry coastal areas: an option for climate change mitigation. *Earth System Dynamics*, 4(2), pp.237–251. Available at: <http://www.earth-syst-dynam.net/4/237/2013/esd-4-237-2013.html> [Accessed November 14, 2013].

Hu, X.-M., Nielsen-Gammon, J.W. and Zhang, F., 2010. Evaluation of Three Planetary Boundary Layer Schemes in the WRF Model. *Journal of Applied Meteorology and Climatology*, 49(9), pp.1831–1844. Available at: <http://journals.ametsoc.org/doi/abs/10.1175/2010JAMC2432.1> [Accessed January 22, 2014].

Shin, H.H. & Hong, S.-Y., 2011. Intercomparison of Planetary Boundary-Layer Parametrizations in the WRF Model for a Single Day from CASES-99. *Boundary-Layer Meteorology*, 139(2), pp.261–281. Available at: <http://link.springer.com/10.1007/s10546-010-9583-z> [Accessed January 22, 2014].

Warrach-Sagi, K., Schwitalla, T., Wulfmeyer, V., Bauer, H.-S., 2013. Evaluation of a climate simulation in Europe based on the WRF–NOAH model system: precipitation in Germany. *Clim Dynam*. Available at: <http://link.springer.com/10.1007/s00382-013-1727-7> [Accessed May 31, 2013].

Wulfmeyer, V., Branch, O., Warrach-Sagi, K., Bauer, H.-S., Schwitalla, T., and Becker, K., 2014. The impact of plantations on weather and climate in coastal desert regions. *Journal of*

*Applied Meteorology and Climatology*, p.140117143424003. Available at:  
<http://journals.ametsoc.org/doi/abs/10.1175/JAMC-D-13-0208.1> [Accessed January 27, 2014]