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Geoscientific Model Development Discussions

Interactive comment on "Evaluation of three new surface irrigation parameterizations in the WRF-ARW v3.8.1 model: the Po Valley (Italy) case study" *by* Arianna Valmassoi et al.

Anonymous Referee #1

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MAJOR REMARKS

The authors present a sensitivity study on simulating the effects of irrigation in the Po valley with the WRF model. They conducted a range of sensitivity simulations using three types of simplified irrigation parameterizations and different assumptions about the timing and frequency of the irrigation. In addition, they considered two different WRF resolutions over the domain of interest. The topic of the paper is interesting and the results of the study have the potential to increase our knowledge on the importance of irrigation for the regional climate of the Po valley and on the suitability of the three parameterizations for its usage regional climate modelling. Unfortunately, the paper

suffers from several flaws that need to be removed before the paper may be published.

Many details are provided with regard to the effect of irrigation and partially it seems that they are sold as new results. However, effects are neither new nor surprising as they can be expected from process knowledge and previous studies. For example, it is well known that irrigation increases soil moisture and latent heat flux, lowers sensible heat flux, surface and 2m temperatures and often reduces related model biases that originate from not representing irrigation in respective areas. Also, a reduced irrigation effect can be expected for the DRIP and Sprinkler irrigation due to the regarded canopy interception and related evaporation. Interesting and new are the differences in magnitude of these effects between the different irrigation types and the different assumptions. While the irrigation effects on surface variables is well generally known, the impact of the irrigation on the climate of the Po valley would be a rather intriguing information, but unfortunately such effects are moved to follow up studies. I found this somewhat puzzling, as many other studies that applied irrigation in climate modelling consider effects on surface and climate at once in the respective publication.

One major obstacle in reviewing and understanding the paper is the low quality of the English. Several times, I was not able to understand a sentence. Usually, I also provide some suggestions for text edits and sentence improvements, but the high amount of unclear sentences, wrong gramma and unusual sentence structures let me refrain from doing this. I strongly recommend a careful proof reading from a native English speaker.

In addition I am wondering why the study's results have been submitted to GMD. The study comprises the results of a set of sensitivity studies that have been conducted with an existing regional climate model using three types of simplified irrigation parameterizations. I would not consider the respective simplified equations as new model development. Moreover, even though the total amount of irrigation may be consistent with actual irrigation amounts, the water used for irrigation is taken out of nowhere, i.e. water conservation dos not play a role, which I would expect if something is introduced as a new model. Therefore, I believe that other journals such as HESS may be more

appropriate for the publication of the study.

Consequently, I suggest focusing on the new results of the study and reducing the details in describing known and expected effects. The paper would strongly benefit from including irrigation effects on climate variables, especially precipitation. In summary, I suggest making major revisions and resubmitting the paper to a more suitable Copernicus journal.

Minor remarks

In the introductory section, some links to previous studies on irrigation seem to be missing. As the present study is dealing with uncertainties related to the representation of irrigation in a climate model, it should be noted that there have been a few studies, which aim at the effects related to differences in the representation of irrigation within models as a key factor of uncertainty. In these studies, the focus was mainly on the extent of the irrigated areas or timing and mode of delivery, e.g. Sacks et al. (2009) and Yoshikawa et al. (2013). However, recently deVrese and Hagemann (2018) investigated uncertainties related to the representation of irrigation characteristics with respect to irrigation effectiveness and the timing of the delivery. Further, it is written on p3, line 60-61, that “Moreover, most importantly the scheme proposed do not account explicitly for irrigation water amount as an input.” I do not agree with this statement as several climate model studies exists where the water for irrigation is withdrawn from existing reservoirs within the respective model framework (e.g., from reservoirs of the river routing scheme such as in Guimberteau et al. 2012, deVrese et al. 2018), or where the amount of irrigation is limited by using information from observed river runoff (Saeed et al. 2012). When interpreting the results of the present study, it should be kept in mind that the impact of irrigation on simulated climate may also differ between models or simulations despite identical assumptions about the irrigation characteristics (Tuinenburg et al. 2014; Krakauer et al. 2016).

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Krakauer NY, Puma MJ, Cook BI, Gentine P, Nazarenko L (2016) Ocean–atmosphere interactions modulate irrigations climate impacts. Earth Syst Dyn 7(4):863–876

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Saeed F, Hagemann S, Saeed S, Jacob D (2012) Influence of mid-latitude circulation on upper Indus basin precipitation: the explicit role of irrigation. Clim Dyn. doi:10.1007/s00382-012-1480-3

Tuinenburg O, Hutjes R, Stacke T, Wiltshire A, Lucas-Picher P (2014) Effects of irrigation in india on the atmospheric water budget. J Hydrometeorol 15(3):1028–1050

Yoshikawa S, Cho J, Yamada H, Hanasaki N, Khajuria A, Kanae S (2013) An assessment of global net irrigation water requirements from various water supply sources to sustain irrigation: rivers and reservoirs (1960–2000 and 2050). HESSD 10(1):1251–1288

p. 5 – eq. 1 and line 127-128, Table 2 I don’t understand the definition of ïĄĎT1. Why a frequency is characterized with the difference symbol ïĄĎ? Why a frequency is expressed in number of days and not number per days? If irrigation is conducted once per weak, I would expect a frequency of 1/7 days, and not 7 days such as it is defined.

p. 5 – line 134-135 It is written: “Therefore, the water accumulated on the canopy is imposed to zero when irrigation is activated.”

Does this mean that (as irrigation water is added to precipitation) also the actual precipitation is not intercepted? This would introduce an erroneous model change that makes the comparison to the control simulation invalid as effects on model results are not only caused by irrigation itself.

p. 7 – line 180-181 Why a non-reproducible option is given as default, and not the reproducible one? This makes a potential code debugging more difficult.

p. 7 – line 189 Do you need to pay regard to a buffer zone near the boundary in which spurious boundary effects may occur?

p. 8 – line 195 Information of NCEP is not relevant for this paper. → Remove!

p. 8 – line 206-208 Redundant information in these two sentences. → Merge and remove redundancy!

p. 12 – Fig.4 - caption Panel description is missing.

p. 13 – line 311 In Fig. 3, no biases are plotted. I assume you mean Fig. 5?

p. 14 – line 332-333 It is written: "presents a bigger increase in the negative biases in highly irrigated areas, and a lesser decrease of the positive ones in area with low percentage of irrigated land." This is unclearly written. Do you mean that negative bias get bigger (as it is written), or do you mean that you get more points with negative biases (what I infer from Fig. 5).

p. 16 – line 354-355 Why do you not use the coarsest scale of the model, i.e. the 15 km resolution?

p. 17 – Fig. 17 and related text The MODIS PET is an estimated product, and do not comprise real observations. What are the uncertainties of the MODIS PET data? I assume that there are larger uncertainties. Consequently, I suggest writing clearly that results change with respect to MODIS, and not that simulated PET becomes better. For example, in line 365, I suggest writing "PET values are closer to MODIS in all

experiments" instead of "PET values are improved in all the experiments".

p. 18 – line 379 What are "spatial aware" differences? Please define thoroughly!

p. 18 – Fig. 9 - caption "...percentage changes " of what? p. 21 – Fig. 11 and 12 Figures are too busy. Showing 28 different curves, the curves are not distinguishable.

p. 22, 23, 24 – Fig. 13 (upper panels) and 14, 15 (upper panels) Figures are too busy and blurry with all these curves. The light-dashed lines do not provide any additive value and strongly distort the figures.

p. 25 – Table 4 and related text Why do you express the efficiency as a percentage change of soil moisture? In this way, the efficiency inherently depends on the absolute saturation/wetness of the soil, so that the efficiency cannot be adequately compared with studies from other regions, years or season. For example, if an irrigation method leads to 10 mm more water in the soil over a certain period, its efficiency will vary depending on the initial soil moisture, e.g. for soils with 20 mm or 30 mm initial content, this would yield 50% or 33% efficiency. However, I would expect the efficiency of the method itself being the same for both soils. More commonly used efficiencies are defined based on the total irrigation water amounts. For example, you may define an efficiency as the ration of the soil moisture change and the irrigated water. For example, if the latter is 40 mm, then for both soils mentioned above, the efficiency would be 25%.

In addition, please clearly define EVDR and CANW. I assume, CANW = DRIP-CHAN and EVDR=SPRINKLER-DRIP?!

p. 28 – Table A1 The character size is much too small.

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