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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 #2

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The paper presents the development of three irrigation parametrization schemes in WRF and evaluates these schemes for the Po Valley (Italy). The paper fits well with the scope of GMD. Showing the irrigation cooling is not something new at all. But, showing that atmospheric and soil variables are not very sensitive to the parametrization assumption for irrigation timing and length is both interesting and questionable. Overall, the paper requires a significant revision to (a) bring the present study into the context of previous studies (i.e., address the novelty or difference of this study), (b) improve the figure quality and presentation quality (to reduce clutter and improve he text flow), and (c) discuss how the assumption and surface canopy may affect the results.

1. Introduction. The introduction, as it written, is a bit narrow and doesn't reflect the state of science in understating and/or modeling of irrigation effects. More references (or referring readers to other papers) should be added.

a) The introduction mentions little about the central pivot irrigation that is the main method for many parts of framing in United States. In addition, over the central Great plains, underground water is pumped for irrigation, and that water can be 10-20C lower than the surface water. Can the three irrigation schemes be applied to the central pivot irrigation from ground water?

b) The irrigation has important secondary effect on atmospheric dynamics, clouds, precipitation and infrared radiation (as water vapor is a greenhouse gas). The introduction lacks the summary of these secondary effects, or at least should refers the readers on these effect to previous work (ie., see the literature review by Aegerter et al., 2017 already cited in the manuscript). In addition, L45-48, page 2: Aegerter et al. 2017 actually find the surface cooling by irrigation can lead to regional subsidence and so decrease of cloud fraction, which is different from Qian et al. (2013).

c) There are already lots of irrigation schemes. A table summarizing these schemes and comparing/contrasting these schemes with the three schemes developed in this study should be made. Note, in reality, the soil moisture is never 100% in its field capacity for the whole growing season. In average, 50% is more to the norm. See Aegerter et al., 2017. This should be pointed out after the text in L55-60.

d) The last paragraph should also talk about the canopy effect as a result of irrigation, which is addressed in several past studies (such as Qian et al., Aegerter et al.). Without irrigation, there would not be canopy/crops, and the surface albedo would change. The schemes and experiments in this paper don't seek to address that, but this should be made clear and discuss the likely impacts (based on the past work).

2. Irrigation parameterization.

a. How does the development here differ from the paper by Lawston et al., 2015, J. Hydrometeor., 1135–1154?

b. Does the scheme consider the evaporation of water on the leaves (and so the cooling effect)? Does the temperature of irrigated water matter?

c. Irrigation mask field. The work here is similar as Aegerter et al., 2017 in which MODIS-based USDA irrigation database was used. However, it remains unclear how the fraction/percentage of irrigation in a model gridbox is factored into the Noah Land Surface Model in terms of surface properties for that gridbox as whole.

d. Does the crop types matter over the irrigated area? Aegerter et al. designate that as irrigated cropland and pasture for CLM. How the albedo, leaf area index or NDVI are specified for crops over the irrigated area in NOAH? Obviously, these are the parameters/questions that the present manuscript is not trying to address, but it is important to be clear about it.

3. Method

a) The method section only briefly mentioned that Noah LSM is used. It is unclear how the soil moisture responses to the rainfall/irrigation. Is the reference of Ek 2003 the most recent paper for Noah LSM? How surface energy budget is modeled in general terms? What surface type database is used? In Figure 2, there are 12 croplands. Are all these croplands irrigated? Does Noah treat these 12 croplands differently in terms of their albedo, leaf area index or NDVI?

b) L240-245. Where does 7mm/day come from? Should there be more irrigation in the early stage of growing season?

c) Control and sensitivity experiments didn't consider the canopy effect. Is this important? Note, if just irrigation (with no crop growing), should the surface be warmer or cooler? No irrigation and no crops should be the baseline experiment on top of which the irrigation effect can be fully studied (Aegerter et al. 2017).

4. Validation and results. The presentation here and the text flow are difficult for readers to comprehend.

a. Table 3. Should this monthly bias? The title says "the monthly values of mean T2". If so, why the minimum temperature (xˆbar) can be below 0 (the right most column) during the growing season?

b. Figure 5, 6, and 7 have significant repeating. The font size is too small; the labels are misleading – should it be labeled as \delta max T2 in figure 6, and \delta min T2 in Figure 7? The legend for the dot size is all shown in blue color, but the actual data dots show the red color as well. This is very confusing. Can these figures be summarized by showing area-averaged temperature as a function time, separated for upper left t part of stations and lower right of the stations? Some figures can be moved to the supporting materials.

c. Figure 8. Why the colors are different between legend and bar color?

d. Fig. 10 and Fig. 11 look similar. Again, the font size is too small for this reader to read. So is Figure 12.

e. Figure 13. Mean difference of what? Soil moisture? At which level? What are the differences between top left and top right panel? Font size is too small here again.

f. Figure 14. What is shown here is the difference with respect to the control? How about the difference with respect to the observed T (averaged over all stations)?

g. The irrigation efficiency does depend on the leaf area. In the early growing season, the crop height is low and leaves are small. The efficiency should be similar. With all the assumptions made, it is questionable if the parametrization schemes here have the fidelity to address the issue of irrigation efficiency. From an economic point of view, farmers use irrigation to grow crops, and so, the irrigation amount is unlikely uniform throughout the growing season (as assumed by the model here). Taller crops may need more water, and so, what is the point of evaluate irrigation efficiency if the specific crop

types are not considered? In addition, there are issues about the cost for each irrigation method. Overall, section 5.4 is cursory and is recommended to be removed from the text.

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