Hydrol. Earth Syst. Sci. Discuss., 12, C4869–C4877, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C4869/2015/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Comparing CFSR and conventional weather data for discharge and sediment loss modelling with SWAT in small catchments in the Ethiopian Highlands" *by* V. Roth and T. Lemann

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Received and published: 16 November 2015

General comments:

The paper entitled "Comparing CFSR and conventional weather data for discharge and sediment loss modelling with SWAT" assesses the relevance of using continuous modelled CFSR rainfall data compared to the use of measured (but scarce) rainfall data when running an hydrological model, namely the SWAT model.

The authors joined extensive Supplementary Information with modelling input and out-

put.

The paper is interesting in itself for 3 reasons: (1) CFSR data are more and more used within the SWAT users' community, including without comparison to any measured dataset, (2) the SWAT model has not been yet often applied to small ($\sim$1 km^2) river basins, and (3) published literature on Ethiopian case studies is scarce.

Specific comments:

However, the paper lacks of rigor in some sections (see my detailed comments hereafter) and I still have this question pending: what is the added value of hydrological modelling to show that CFSR data are not reliable enough to model small river basins? The Figure 2 is enough to show that both modelled and measured rainfall datasets are strongly different. The paper would gain much if this particular point could be more discussed.

Overall, the method should be revised to merge similar topics together and avoid repetition (see e.g. section 2, section 2.1.1, section 2.2, section 2.3).

Another key point I want to raise is the potential improvement of the discussion. For instance, in the conclusion the authors claim (P11069 L12-13) that "CFSR data may not be applicable for small-scale modelling". Based on my own experience of CFSR data I totally agree with it, but the authors should previously extend their discussion comparing their results to the results of the other studies using CFSR data, including the size of the case-study basins. It seems intuitive that for larger basins, CFSR data errors are balanced and the hydrological modelling achieves better quality, but the authors should explore it through the literature.

Minor corrections:

P11054 L11: minimal surface is not consistent with Table 1 and with P11057 L8.

P11054 L24-25: "is one of the most important input parameters. . ." that is true, but needs to be supported by adequate references. In addition, this is probably true for

all other hydrological models, it could be good at that point to broaden the introduction before introducing SWAT.

P11054 L26: references to SWAT papers would be welcome here for non-users e.g. Arnold et al., 1998, 2012 (see ref list below)

P11055 L4: “for SWAT website” what do the authors mean? The sentence should be rephrased to avoid ambiguity.

P11055 L11: “;” are not appropriate here

P11055 L12-14: this sentence is hazardous: if the work has already been done, why doing it again?

P11055 L18-etc: “Several studies evaluating the CFSR data. . .” this section is interesting since it gives examples of successful and unsuccessful uses of CFRS data. However, key information is missing for the authors/readers to compare the present study to the previous studies: what were the sizes of the modelled catchments? Additional literature assessing CFSR data: Bressiani et al. 2015; Alemayehu et al. 2015.

P11056 L11: “35 years” -> 33 years? This is however not consistent with P11059 L21 and Table 1, and not all the 3 stations have the same “year of construction”.

P11056 L12: “annual, interannual, and seasonal cycles” this is one of the added value of the paper and I recommend the authors to better highlight it, for instance by extending the introduction on this particular point.

P11056 L19: Method: somewhere the authors have to describe the material used for rainfall and temperature measurements, their frequency, the spatial resolution of the CFSR data as put into the model, the number of measurement stations respect to the number of CFSR stations, etc.

P11056 L25 and L27: “calibrated”: what is the time-step? What about validation process mentioned in Table 1?

P11057 L8: minimal surface is not consistent with Table 1 and with P11054 L11.

P11057 L9: altitudes are not consistent with Table 1.

P11057 L12: “divided. . . into one. . .” is not much elegant, I guess the sentence can be rephrased.

P11057 L15: why embedding section 2.1.1 within 2.1?

P11057 L16: I doubt ArcSWAT “was used to assess the impact of different rainfall patterns. . .” but I believe SWAT was instead used for it and the version of SWAT used for it should be mentioned. ArcSWAT is just the interface to build the SWAT model to be run. To make it clear, the authors should separate what ArcSWAT does and what SWAT does.

P11057 L19: “other regions” could be introduced before “Ethiopia” to make the references clear.

P11057 L22: “specific” what do you mean? You could also say that SWAT is a semi-distributed hydrological model.

P11058 L3-4: Which method did the authors use?

P11058 L5-6: Which method did the authors use?

P11058 L28: “. . .heat unit function. . .” this is not “Spatial data”, the authors could rename the section title or move the sentence in the section where they describe the model (section 2.1.1)

P11058 L28: what is “Teff”? More details would enlighten the reader who is not familiar with African inter-tropical agronomy.

P11059 L6: “satellite images” which ones? How many? Were images captured during low flow or high flow?

P11059 L7: “The sub-basin sizes were fixed at 2 ha” what do the authors mean? Is it

the minimal drainage area?

P11059 L8: “All HRUs were defined. . .” The authors should explain why they kept such accuracy. Did for instance the authors use a detailed land cover map?

P11059 L13: Which data where used as input into the SWAT weather generator? Only measured? Only CFSR? Both depending if measured or CFSR rainfall data was used? Did the authors compare their temperature measurements to the CFSR temperature?

P11059 L14: “solar radiation, and relative humidity” Why mentioning those since Hargreaves PET method was used?

P11059 L16: “Daily river flow and sediment concentration. . .” What is the sampling material? What is the sampling frequency? This information may be useful to later broaden the discussion on the modelling quality.

P11059 L19: Can sediment concentration be “visible”? Aren’t the authors talking about turbidity? Then what was the turbidity threshold to describe it “visible”? Was it kind of experts’ knowledge?

P11060 L1: The section title is a kind of mix up that the authors should clarify according to what the section is dealing with. See also my previous comment: merge similar topics together.

P11060 L2: The Abbaspour reference should point to SWAT-CUP, not to SUFI2.

P11060 L14: “Nash-Sutcliff” -> Nash-Sutcliffe; “standardized Root. . .” -> “the standardized Rood. . .”

P11060 L15: “All are very commonly. . .” the sentence is slightly clumsy! The authors may give a stronger justification for using those criteria, instead of just considering it’s good to use them because everybody do so. The authors could also just remove the sentence.

P11060 L22: “and a better accuracy of observations. . .” this is not clear

P11060 L22: “RMSE” the authors should detail RMSE here.

P11060 L27: “. . .model simulation to a large. . .” -> “. . .model simulation, to a large. . .” a comma is needed here to make the sentence clear, or the sentence should be rephrased.

P11061 L10: “Legates and McCabe” these authors don’t need to be called twice.

P11061 L17: “Belg” and “Kremt” timing and durations should be described in the method section.

P11063 L23 and P11065 L23: Table 5: What about calibration and validation results? In Table 5 I guess only overall goodness-of-fit indices are given, what about the specific values for calibration and validation? Is hence the model truly validated? The authors should discuss it.

P11063 L24: “Each model. . .” this is method.

P11064 L8: “were maximized, but still inside SWAT absolute values” what do the authors mean?

P11064 L16: “. . .rainfall data proved impossible” what do the authors mean?

P11065 L8: “:” are not appropriate

P11065 L10-13: “The hydrograph. . . increasing.” This sentence is not clear and needs to be rephrased.

P11065 L19-22: “Sediment loss modelling. . .untouched” this is method. However, one can criticize this method: since sediment loss is driven by the hydrology, then calibration process could also be more reliable when calibrating both discharge and sediments at the same time. The authors could explain their method choice.

P11067 L5: Conclusion: the 7 first paragraphs are an extended summary of the results that is not strictly appropriate for a conclusion. Key outputs from this study are coming

in L28 and should be highlighted.

Tables:

Table 1: What does “Year of construction” refer to? The year of construction of the gauging station? Did the measurements started just after construction? What is the meaning of “field scale” for land use and soil maps? About “daily sediment loss”: what was measured, the concentration or the load? In guess the concentration in the river (at the gauging station) is slightly different from the load lost from the hillslope. Sources could be given as a table footnote.

Table 2: The title should explain the meaning of “a” and “v” in SWAT-CUP. The table should also show the initial value ranges to remind section 2.4. A “_” is missing to a_CN2.mgt and “hu” needs to be corrected to “hru”. What is the meaning of the very small changes (e.g. -0.0038, 0.0023 for ESCO, -0.084 for SURLAG...) ?

Table 3: To my opinion, this table is useless. If one wants the detail of Moriasi’s paper, then he can read his paper. But what to my opinion would be really useful, is to highlight (using bold, italic or whatever) the values that meet the satisfactory criteria values in Table 5.

Table 4: The title should be more detailed. What are the 2 rainfall datasets compared? What is their duration, which periods are compared? Which region of the world are we talking about? What are Kremt and Belg? In general, the title of a Table or a Figure should give enough information to the reader that he does not necessarily need to go through the manuscript to understand the table/ figure.

Table 5: See my comment to Table 3 and my comment to P11063 L23 and P11065 L23. Bold/Italic highlights should be explained in the title. The title should also remember that calibration and validation periods are given in Table 1.

Figures:

Figure 1: It’s difficult to get an idea of the relative scale of the 3 small sub-catchments

of interest. Reporting the shapes in the main figure could enlighten the reader. If the sub-catchments are too small then another representation should be considered. Berha, Kolla, Dega, Wurch are not described in the manuscript. The title should say the map is a land use map (I guess ?) and give the year corresponding to the land use shown in the Figure.

Figure 2: WLRC stations are not starting in 1979. Why not putting Dry Season, Kremt and Belg in those figures and referring to it throughout the manuscript?

Figure 3: Again, the title should be more concise. The figure shows both observed and modelled discharge, discharge is simulated from both WLRC and CFSR rainfall datasets, etc.

Figure 4: See comment to Figure 3.

References:

Alemayehu, T., Griensven, A., Bauwens, W., 2015. Evaluating CFSR and WATCH Data as Input to SWAT for the Estimation of the Potential Evapotranspiration in a Data-Scarce Eastern-African Catchment. J. Hydrol. Eng. , 10.1061/(ASCE)HE.1943-5584.0001305, 05015028.

Arnold, J.G., Moriasi, D.N., Gassman, P.W., Abbaspour, K.C., White, M.J., Srinivasan, R., Santhi, C., Harmel, R.D., Van Griensven, A., Van Liew, M.W., others, 2012. SWAT: Model use, calibration, and validation. Trans. ASABE 55, 1491–1508.

Arnold, J.G., Srinivasan, R., Muttiah, R.S., Williams, J.R., 1998. Large area hydrologic modeling and assessment. I. Model development. J. Am. Water Resour. Assoc. 34, 73–89.

Bressiani, D., Srinivasan, R., Jones, C.A., Mendiondo, E.M., 2015. Effects of different spatial and temporal weather data resolutions on the streamflow modeling of a semi-arid basin, Northeast Brazil. Int. J. Agric. & Biol. Eng. 8, 1–16.