Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-302-RC3, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Modeling the response of soil moisture to climate variability in the Mediterranean region" *by* Louise Mimeau et al.

Ryan Teuling (Referee)

ryan.teuling@wur.nl

Received and published: 31 July 2020

The manuscript by Mimeau et al. addresses the important issue of changes in soil moisture conditions in the Mediterranean. The stochastic approach is a nice addition to existing studies, and the main findings are important. The topic also fits very well in the special issue. However I have some concerns regarding details in the Methods, the use of literature on stochastic approaches to soil moisture dynamics, and the presentation of the results. These are discussed below. I believe the concerns are best addressed in a major revision.

Introduction

"Only a few studies attempted to validate the soil moisture simulated by the GCM or

RCM land surface schemes" -> Maybe, but other studies (such as Stegehuis, GRL, 2013, doi.org/10.1002/grl.50404) have used flux observations which should have the same, if not better, effect.

"This is particularly true for the Mediterranean regions . . . land surface models" -> Ok, but next you claim this can be solved by using a simplified model. So are the other models all worse than the simple model used here? Or is the lack of calibration of higher importance than model structure?

"The only study that applied this method to soil moisture" -> There are at least several others, such as Teuling et al. (GRL 2007, doi:10.1029/2007GL031001), and Calanca et al. (WRR 2004, doi:10.1029/2004WR003254)

Literature: In general, I miss a discussion on the previous use of stochastic approaches in soil moisture modeling. These include for instance the work by Milly (WRR 2011, doi:10.1029/2000WR900337), Laio et al. (AWR 2001, 24, 707-723), and Rodriguez-Iturbe (1999, Proc R Soc Lond A 455: 3789-805). These (analytical) approaches use a more basic description of the precipitation process, so it should be motivated why a more complex Neyman-Scott representation is needed to address the research question.

Method

Table 2 mentions the "Monthly potential evaporation coefficient L". What is the role of this parameter, and how is it different from the coefficient for evapotranspiration Kc?

"a linear relationship between actual and potential evapotranspiration" -> Please provide more information. Is this linear between field capacity and wilting point? If so, this is a big simplification. Many other studies have shown that there is a considerable range in soil moisture over which ET is potential (above the critical moisture content), and that this unstressed soil moisture range is in fact required to explain observed soil moisture and vegetation dynamics and features such as strong bimodality (Salvucci,

2001 WRR 37(5), 1357–1365, Teuling et al. GRL 2005 doi:10.1029/2005GL023223, Denissen et al. JGR 2020, doi:10.1029/2019JD031672). It should be better motivated why this gross simplification is justified, and what the potential implications are for the simulated soil moisture dynamics (for instance, the higher stress could explain why most lines in Fig5 are above the 1:1 line around 20 Vol%)

“two additional calibrations were performed on subperiods . . . in order to analyze the stability of the calibration” -> For the stability it is more important to consider the variability in optimum parameters than the model performance itself (that is listed in Table 3). Please also provide the parameters for periods 1 and 2 so that the robustness of the calibration can be better assessed.

In the method, it is mentioned that the rainfall parameters are estimated for each month of the year. I assume that this also means that the model is run for every month separately? This is not mentioned. If so, this has some implications for the results, because in this way one doesn’t account for the month-to-month carry-over of soil moisture memory (i.e. going into summer the soil moisture will be slightly higher at the beginning of each month because of the on average wetter previous month). Please explain and discuss the potential impacts this approach has on the results.

Results

I miss an illustration of model performance, for instance a modeled and simulated time-series at one of the stations so that model performance can be visually checked (NSE tends to be high by default in strongly seasonal climates, so this alone might not be a good indication).

Figure 8: This is an important figure, but I find it difficult to extract any relevant information other than that intermittence is the most sensitive factor. This could more easily be shown by first averaging over all stations, and only show the stations if there a story to it. The most important aspect now is the comparison between the different rows, and this is not easy because the reader has to guess the values and compare visually.

Consider plotting the differences more explicit if this is where conclusions are based on.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-302, 2020.