<u>Reviewer 3: Ryan Teuling</u>

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.

Thank you for the revision of our manuscript. Please find below a point-by-point response to your comments and the modifications made to the revised manuscript.

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.

We note that in Stegehuis et al 2013, there is no evaluation of simulated soil moisture but only sensible heat flux at the surface and the 2-m mean temperature.

"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?

We do not claim that the large variability between climate model simulations of soil moisture can be solved with a simpler model than the land-surface schemes of the climate models. We just state that there are obvious discrepancies in soil moisture simulated by these different models, so we prefer to rely on a bottom-up approach based on observed data to estimate the sensitivity of soil moisture to changes in climate characteristics. We added: "As a consequence, the direct use of soil moisture from climate models may not be the best option to assess small scale soil moisture variability in relation with climate conditions."

"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.

Thank you for these additional references. We modified this section to include the proposed references, and provide a better review of previous studies applying stochastic methods to soil moisture. We do not think that a complex stochastic generator is necessarily required. For instance, Zhu et al 2020 used a rather simple elasticity approach or Guo et al 2018 applied a simpler weather generator to achieve satisfactory results. We used a Newman-Scott model to represent distinctly the changes in precipitation intermittence and intensity at the hourly time step but other approaches can be equally valid too, as soon as they are able to represent changes in these rainfall properties.

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?

L is the monthly percentage of total daytime hours out of total daytime hours of the year. This fixed parameter, computed based on the station's coordinates, enables to represent the monthly variations of the potential evaporation. The values of L are not calibrated.

Kc is a correction factor that is calibrated for each station to adjust evapotranspiration.

Brocca, L., Camici, S., Melone, F., Moramarco, T., Martínez-Fernández, J., Didon-Lescot, J.-F., & Morbidelli, R. (2013). *Improving the representation of soil moisture by using a semi-analytical infiltration model. Hydrological Processes, 28(4), 2103–2115.* doi:10.1002/hyp.9766

“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%)

This was an error in the model description. It is not a linear relation between actual and potential evapotranspiration but a linear relation between potential evapotranspiration and soil saturation, that is used to compute actual evapotranspiration. See equation 7 of Brocca et al., 2008. This formulation is quite standard and many models use it.

We modified the text to remove this error, thank you for noticing this mistake.

"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 Table3). Please also provide the parameters for periods 1 and 2 so that the robustness of the calibration can be better assessed.

We added the calibrated parameters (soil moisture model) on the 2 periods, see table below. It can be seen that the parameters values are within the same order of magnitude for the three calibration periods, with a stronger variability of the Ks compared to the two other parameters.

	Barn	Cab	Gra	Lez	Mej	Mou	Nar	Pez	Pra	Vil
Calibration on the total period										
K_s (mm.hr^{-1})	38.1	34.3	35.9	23.1	28.8	36.2	51.1	14.6	59.6	6.9
m	17.6	15.6	10.9	14.1	16.4	23.0	15.9	12.8	11.89	38.2
K_c	1.17	1.43	1.74	1.22	1.81	0.94	1.26	1.99	1.32	1.63
NSE	0.76	0.77	0.93	0.85	0.9	0.63	0.91	0.789	0.65	0.9
Calibration on the first sub-period										
K_s (mm.hr^{-1})	26.9	52.0	56.2	41.6	24.6	22.5	52.0	24.0	61.9	22.6
m	17.8	15.8	11.3	15.4	14.7	17.8	17.5	21.0	10.5	40.0
K_c	1.28	1.49	1.95	1.30	1.76	1.02	1.31	1.86	1.31	1.63
NSE	0.6	0.72	0.86	0.87	0.80	0.69	0.87	0.31	0.64	0.87
Calibration on the second sub-period										
K_s (mm.hr^{-1})	29.6	23.7	43.9	40.8	45.6	77.4	43.1	6.4	71.4	2.7
m	23.2	17.1	11.3	14.8	18.9	22.4	13.5	5.5	13.0	39.9
K_c	1.09	1.42	1.53	1.11	1.87	1.32	1.19	1.97	1.31	1.56
NSE	0.71	0.75	0.87	0.78	0.91	0.04	0.912	0.60	0.57	0.86

Table 3. Calibrated parameters of the SM model and NSE validation values while calibrating on the total period, the first and second sub-periods of the in situ data series.

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.

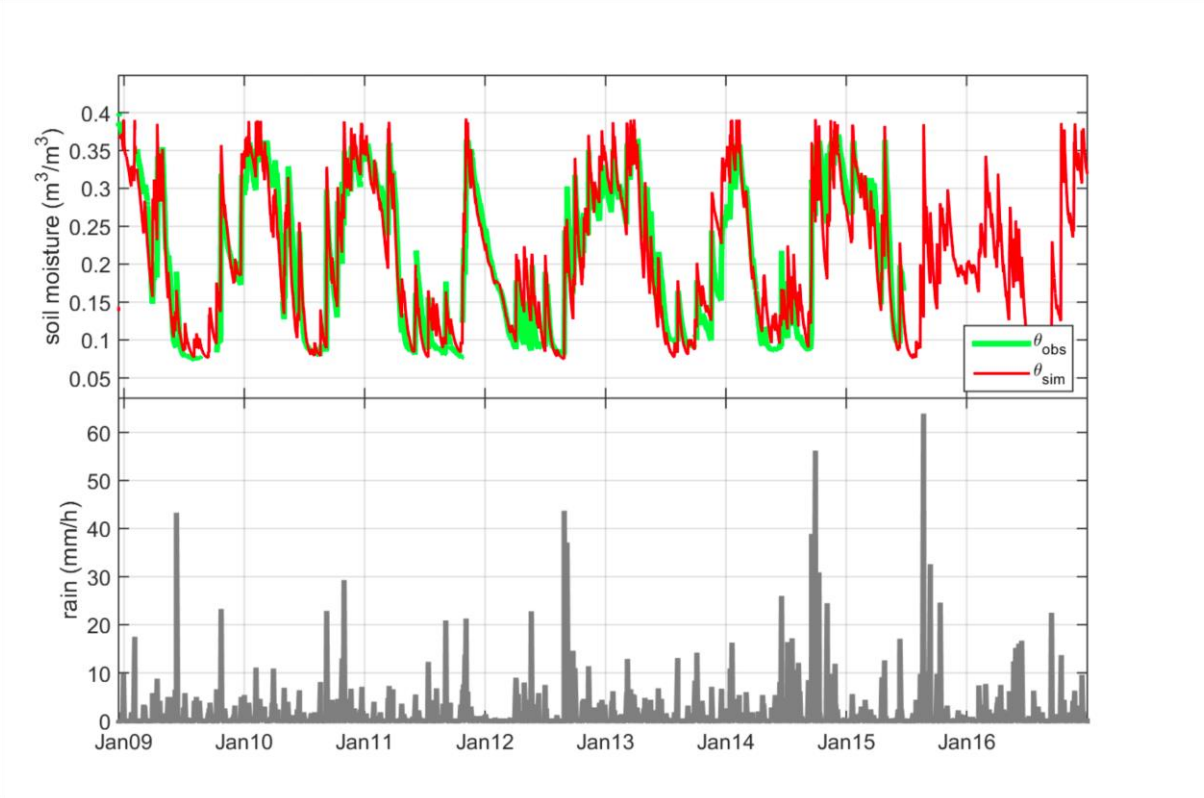
The NSRP model (the rainfall generator) is applied to each month separately, since the distribution of rainfall needs to be homogeneous (the distribution of hourly rainfall is obviously not the same in December or in August in these Mediterranean areas). This is a very standard practice when using this type of rainfall generator. If the distributions are estimated for each month, the generator then simulates continuous rainfall series, to be used as inputs in the soil moisture model and provide time series across all months/years.

We added page 8, line 2: "Once the model parameters estimated for each month, it is run to produce continuous simulations.".

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).

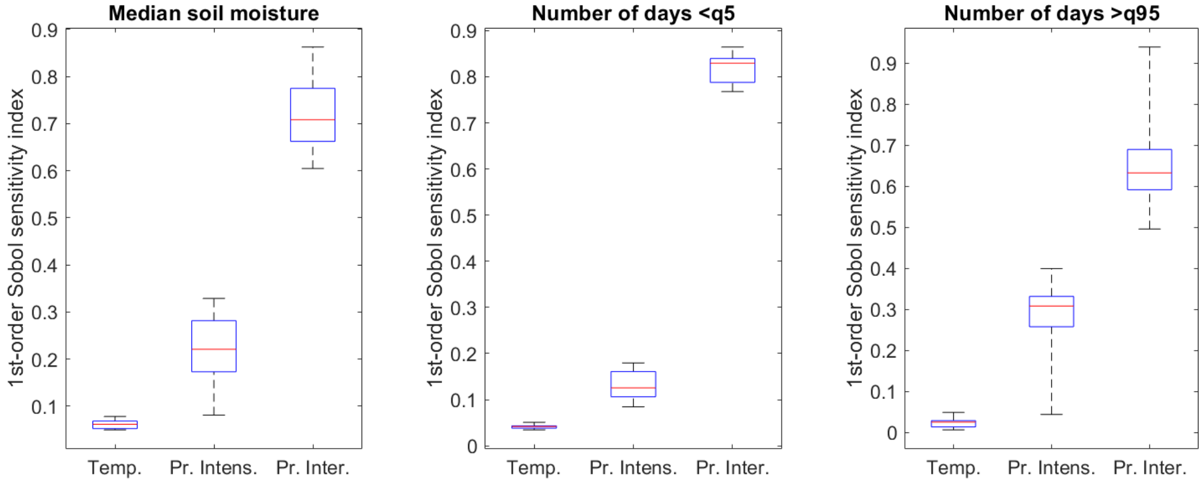
We added a new figure with the observed and simulated time series of soil moisture.



Simulated (green) and observed (red) soil moisture at the Villevieille station

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.

The fact that intermittence is the key factor is indeed the main message of this figure. We modified the figure according to your recommendation, showing boxplots of the Sobol indices for Temperature (Temp.), Precipitation intensity (Pr. Intens.) and Precipitation Intermittence (Pr. Inter.), see the new figure below:



Median soil moisture
Number of days <q5
Number of days >q95
1st-order Sobol sensitivity index
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
Temp.
Pr. Intens.
Pr. Inter.