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Discussions

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

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I thank the authors for this interesting paper on the relationship between meteorological forcings and soil moisture in the Mediterranean region. The manuscript is well written, well organized and the different modelling tools are adequately applied. The first important result is that the increase in temperature is not the main driver of the changes in soil moisture, but seems to be precipitation characteristics. The second important contribution is methodological since this study shows how a soil moisture model and meteorological scenarios can be used to assess the sensitivity of the soil moisture to these forcings. I have two major comments (see below) regarding how rainfall scenarios are generated. The authors simulate changes of intermittency using the parameter lambda of the Neyman-Scott model. This lambda parameter is the master Poisson pro-

cess parameter and is directly related to the frequency of rainfall events. I think that the interpretation of 'intermittence' is misleading, which is annoying since the main results of the paper rely on this interpretation. My main recommendation is thus to change the way rainfall scenarios are generated. In my opinion, the best option for the generation of scenarios would be to recalibrate the NSRP model for each set of rainfall statistics (the observed ones + the perturbed ones). In the current version of the manuscript, it must be clearly understood that when one parameter (e.g. lambda) is modified, it affects all rainfalls statistics, which complicates the interpretation of the main factors leading to changes in the soil moisture.

Major comments:

#1 Due to its structure, the different parameters of the Neyman-Scott rectangular pulse model are not directly interpretable in terms of rainfall statistics. In the current version of the manuscript, parameters lambda and xi are loosely interpreted in terms of "intermittence" and "mean intensity". In my opinion, this interpretation is incorrect and misleading: - The parameter lambda, which governs the master Poisson process, represents the rate of rainfall events (storms). As such, the mean intensity (for any aggregation duration) is linear in lambda (Eq. 2.5 in Cowpertwait, 1998). It is also true for the covariance for any lag (Eq. 2.6 in Cowpertwait, 1998). This means that when lambda decreases (in this paper the inverse of the storm frequency), the mean rainfall intensity increases in proportion. - The parameter xi is the parameter of the exponential distribution for raincell intensity. The mean rainfall intensity (for any aggregation duration) is linear in lambda. When xi increases, the mean rainfall intensity increases in proportion (Eq. 2.5 in Cowpertwait, 1998). An augmentation of 50% in lambda is directly compensated by an augmentation of 50% in xi, which is indicated in Section 4.1 (l. 20). However, an increase of xi with the same increase in lambda leads to the same annual rainfall but also to an increase of the mean intensity of the rainy days (which is indicated at l. 21 but not clearly since the authors refer to the "mean rainfall intensity"), and to an increase of the number of dry days. - Intermittency is not clearly defined in the paper. I strongly suggest proposing a definition in terms of rainfall statistics. A stronger intermittence could be, for the same annual rainfall, a higher number of dry days. It could be parametrized with lambda and xi, but also with the other parameters. Note also that the theoretical proportion of dry days can be easily obtained with the NSRP model (see Eq. 9a-9b in Cowpertwait, 1991), using a numerical integration. The two quantities that would be perturbed could thus be "the total annual rainfall" and "the proportion of dry days" (or equivalently the number of dry days), which would have a direct interpretation. - As said above, in my opinion, the only valid option for the generation of scenarios is to recalibrate the NSRP model for each set of rainfall statistics (the observed ones + the perturbed ones). When lambda or xi is modified, it affects many rainfalls statistics at the same time, which complicates the interpretation of the main factors leading to changes in the soil moisture. As the proportion of dry days is important in this study, it should also be included in the set of rainfall statistics used to estimate the parameters. Cowpertwait, Paul S. P. 1991. "Further Developments of the Neyman-Scott Clustered Point Process for Modeling Rainfall." Water Resources Research 27 (7): 1431–38. https://doi.org/10.1029/91WR00479. Cowpertwait, Paul S. P. 1998. "A Poisson-Cluster Model of Rainfall: High-Order Moments and Extreme Values." Proceedings: Mathematical, Physical and Engineering Sciences 454 (1971): 885–98.

#2 Many parameter estimates seem to indicate a failure of the estimation method. For eta, the raincell duration parameter, many zero values appear (e.g. Pezenas, June to August) associated to very high values of xi and 1 for beta (the initial value of the optimization I guess). In Pezenas, in September, eta reaches the highest value of 10 I guess, and lambda is very high (666.7). It affects maybe 10 months for all the stations, but the problem should be addressed. I cannot trust these simulations with these unrealistic parameter estimates. Possible solutions are:

1. Try different starting values for the optimization, 2. Change the objective functions (weighted sums, relative/absolute differences between observed and simulated statis-

tics), 3. Smooth the estimation from one month to another, there is no strong reason to have a big difference between two consecutive months.

Minor comments: p.2, l.14: Repetition of "soil moisture", "For soil moisture" could be removed. p.2, l.32: For your information, a recent reference of scenario neutral approaches is "Keller, Luise, Ole Rössler, Olivia Martius, and Rolf Weingartner. 2019. "Comparison of Scenario-Neutral Approaches for Estimation of Climate Change Impacts on Flood Characteristics." Hydrological Processes 33 (4): 535–50. https://doi.org/10.1002/hyp.13341". p.3, l.5: with -> and. Figure 1: Missing labels (Longitude / Latitude). Figure 2: Please increase the font size. p.7, l.3: "of" should be removed. p.7, l.4-8: I think it would be fair to indicate that it is the standard version of the NSRP model, many more elaborate versions have been proposed in the last decades. p.7: The mean number of raincell per storm is often denoted by the Greek letter nu, as is actually done in the manuscript in Section 3.4.1. p.8, l.1: I suggest indicating the statistical properties used for the estimation of the parameters. For these statistics at least, we should have a good agreement between the observations and the simulated values. p.8, l.11: +4C the symbol "degree" is missing. p.10, l.4: there is a space after "+4" that should be removed. p.10, l.11: There is a slight overestimation of the annual number of dry days for some stations (e.g. Barn), it could be noticed. p.12: m3.m-3 seems to be a strange unit (adimensional actually), is it correct?

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