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Earth System Dynamics Discussions

Interactive comment on "Life time of soil moisture perturbations in a coupled land-atmosphere simulation" *by* T. Stacke and S. Hagemann

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We thank the referee for the positive review of our manuscript. In the following we will repeat the referee comments in squared brackets before answering them.

[The manuscript "Life time of soil moisture perturbations in a coupled land-atmosphere simulation" by T. Stacke and S. Hagemann evaluates the memory of initial soil moisture perturbations using a global coupled land-atmosphere-ocean model.]

Reply: No, the ocean is not part of our modeling framework but prescribed using AMIP2 boundary conditions for sea surface temperature and sea ice concentration of the respective time periods. We put more emphasize on this fact by adapting the abstract and adding the reference to the AMIP2 data to the experiment setup section.

[However, it is not clear how realistic are the results, particularly re-occurrence of soil moisture memory. The results are purely model based and no observational data are used in order to support any of the findings, which is the weakest point of the manuscript. [. . .]

1) Use of observed soil moisture or other land surface parameter (available for some European and north American stations), will make the results more acceptable to the research community rather than relying only on the model simulation.]

Reply: We fully agree with the referee that study is purely model based. However, this is done on purpose, for two reasons. First, we are interested in the potential of soil moisture initialization to improve predictions done by models. For this task it is actually not critical whether the concept of soil moisture memory is realistic or not. Instead, we want to demonstrate that it exists in our modelling framework in a way that could ultimately be utilized to improve its predictive skill. Nonetheless, soil moisture memory in general is found in observations (see Vinnikov and Yeserkepova, 1991 already cited in our manuscript and Shinoda and Nandintsetseg, 2011, now added to the introduction) although such studies are very rare. Second, there are no observations available that are directly comparable with our experiment. We actively perturb the soil and analyse the temporal characteristics of its response to an ensemble of extreme events by comparing to the undisturbed case. This cannot be derived from time series of observed soil moisture as there is naturally no unperturbed reference data for the same climate conditions available. Thus, we did use the discussion for a general comparison of soil moisture patterns and length found by other studies. But rather than comparing our results against observations that in the best case could be a vague proxy for our definition of soil moisture memory, we hope to motivate others to set up a comparable laboratory or field-site experiment to verify our findings.

[2) Figure 12, 13 suggests that initial soil moisture perturbation is too strong, perhaps far away from the real level? I am wondering if the perturbations are too strong in some regions, despite authors have chosen a good method to do so. Is there any particular region (climatic condition), where re-occurrence of memory, as evident in leaf carbon content (Fig. 12) is very large? This appears to me the middle and high latitude region, where strong seasonal effect persists. Any observational evidence/reference of previous observation finding?]

Reply: We cannot follow the referee here. How do figures 12 and 13 suggest perturbations are too strong? While perturbations might be unrealistic compared to reality, they are extreme but fully realistic in terms of model variability as they are based on the statistics of our reference simulation. Concerning leaf carbon memory recurrence, it is strongest for the transitional soil moisture regime (see Fig. 14). We explain this with the combined effect of seasonality and soil moisture sensitivity (see 2nd paragraph in Sec. 5). Actually, a connection between soil moisture and vegetation memory is also proposed by an observation based study (Shinoda and Nandintsetseg, 2011), which we now added to the discussion.

[3) In section 4. "The largest impact of soil moisture perturbation is expected for surface and soil moisture .... " Figure 9: this is simplified assumption. There are strong non-linearity in the atmospheric state variable, which is evident in the spread of surface air temperature anomaly. Are the found anomalies are statistically significant?]

Reply: In fact, we stated that we expect the largest effect on surface and soil temperature, not moisture. Anyway, the referee raises a very valid point about whether the observed spread is due to soil moisture perturbation or atmospheric interactions. Indeed, all anomaly data Fig. 9 comes from those points where at least 1 day of memory is evident, which also is the vast majority of points. This demonstrates a strong control of soil moisture over surface temperature in the model. However, the memory is usually quite short, which we attribute to compensating effects via atmospheric states. We added a short note to the discussion:

"Likewise, for most other surface variables, like humidity and pressure, only short memory is diagnosed. Soil moisture control seems strong enough to induced some anoma-

lies, however their memory is quickly dissipated by strongly non-linear processes in the interacting atmospheric states."

[4) As mentioned, this model does not have freezing/thawing of soil moisture, how reliable are the found memory over high latitude/permafrost region? Some discussion is required.]

Reply: We assume the general pattern won't change too much when melting and freezing processes are considered. Especially permafrost regions are dominated by long periods of frozen conditions where memory would be similar in both soil schemes, and only during the relatively short periods of melting and freezing soil memory might be affected due to the water and energy coupling. As we already stated we need to test whether or not this additional process would significantly effect memory.

[5) Many coupled model show drift, which last for several years/decade. As this experiment used only two years of spin-up, the results may be affected by the model drift.]

Reply: Thanks for making us aware of our quite misleading statement. As with most land surface models, spin up especially of soil moisture can regionally take up to 30 years and more. For this reason, our model was not started from scratch but all states where taken from a similar simulation running over several decades. We just added the two more years of spin-up to allow the model to adapt to small differences in the forcing. We added this information in our draft:

"The first set consists of one reference simulation (REF) for the period 1995-2008. It's initial states were taken from an earlier spin-up simulation running over several decades. [. . .] No INI are started prior to December 1996 as this time is regarded as additional spin-up for the model to adapt to minor differences in the forcing between the REF and the spin-up simulation."

Added references: Shinoda, M. and Nandintsetseg, B.: Soil moisture and vege-

tation memories in a cold, arid climate, Glob. Planet. Change, 79, 110–117, doi:10.1016/j.gloplacha.2011.08.005, 2011.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 1743, 2015.