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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Simulating the Dry Chaco Hydrology under Deforestation Pressure" *by* Michiel Maertens et al.

Anonymous Referee #2

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The study evaluates three LSMs within NASA LIS and their performance when updating the models with newer observations of soil texture, vegetation parameters, and land cover in the Dry Chaco region of South America. The study is certainly interesting and includes a thorough analysis. I primarily have positive comments, but recommend major revisions based on comments that elicit clarifications below.

Three major comments: 1) The title and abstract feel like one will learn new information about the hydrology of the Dry Chaco and how the specific region is responding to land use change. However, it turns out that the paper is almost entirely about LSM performance and the Dry Chaco is only a selected testbed. I think this could do the authors a disservice in not drawing in the right readership. Either the authors should revise the title and abstract to be more clear that the study is about LSM performance or

the authors should consider adding a section about what is learned specifically about the Dry Chaco hydrology and not only about LSMs parameterization.

2) I got a bit lost in Section 4.1.3. which seems to be a major section and motivation of the paper. It is possible that the results are clear to the authors, but the LAI time variations are confusing me on their effect in evaluation deforestation. It seems to me that there are other land cover parameters (Section 3.2.2) that were changed as well that are doing more work in accounting for hydrology changes due to deforestation than the LAI time variations. See my more specific comment below.

3) I think the paper could strengthen its context within the literature and do better at explicitly stating the main novelties. What is really new here compared to the current discussion in improving LSM performance with SHPs and vegetation parameters? The authors point out the main motivation in lines 25-29, but then seem to show that many other studies have done what the study did here and found improvements such as in lines 42-44.

Other comments: Abstract: it seems the authors set out to determine how land cover change influenced the water balance in the Dry Chaco (line 3), but the remainder of the abstract is about LSM performance and parametrization. To help interested leaders, please align the objective and discussion of main findings here. After reading the paper, it seems this may involve making it clearer that the goal is about LSM performance, not primarily about evaluating the Dry Chaco hydrology.

Line 22: add that climate models are used to predict future climate

Line 60: I am having trouble seeing the distinction in this hypothesis from many of the previous studies using observed parameters to improve LSM representations. Has this hypothesis already been tested in many of the previous studies? Be clearer about what is different about this study.

Line 60: I am still not clear why the Dry Chaco was specifically chosen. Is it because

the deforestation will make it abundantly clear (more so than another region) that the water balance performance suffers without proper vegetation representation? Would it be more difficult to test the hypothesis in an alternative natural landscape less impacted by land use change (such as in natural bush of Australia)? I recommend being more explicit about this reasoning. It seems that the study is more focused on the LSMs than in studying the Dry Chaco itself; the Dry Chaco is only a testbed.

Line 132: are these parameters from this 1998 paper derived from this same GIMMS dataset? Values derived from GIMMS might be best to avoid biases between different NDVI products.

Line 192: I recommend adding the reason why parameters where computed using different parts of the soil for CLSM

Line 233: I am not following why there is an discussion of surface albedo here. Surface albedo will certainly influence the water balance at least through energy effects on evaporation. Regardless, why discuss specifically the albedo schemes here in detail if they aren't going to be evaluated? Are they a major source of sensitivity in other studies?

Line 241: Can it be clearer what is being tested for in the sensitivity analysis? What would high sensitivity of the results to a given parameter (i.e., LAI) indicate? Why only vary the land cover and vegetation data and not the soil parameters?

Line 262: Why partition the evaporation components? Is the partitioning accurate enough in these land surface schemes to look beyond just the total evaporation?

Line 265: Is the idea to attempt to remove dependence of the evaluation on soil moisture? This makes sense, but this somewhat casts doubt on comparing ET outputs to ET from GLEAM as discussed in Section 3.5 since this comparison will likely be highly influenced by the soil moisture biases. Perhaps reconcile/clarify this issue.

Line 280: If there is no independent, external dataset to evaluate runoff and evapora-

tion efficiencies, what criterion will be used to assess efficiencies across the different models in the different scenarios in the context of the research goal to evaluate whether updated parameters improves the land surface representation?

Table 2: Be sure to include how seasons were defined here or in methods

Line 340: I find it strange that soil moisture changed so drastically with sandier soils with the updated SHPs, but the flux components did not in Fig. 3. $\sim$0.05 cubic meter moisture changes are quite large to not show compensation in flux components. Perhaps more water is infiltrated to deeper moisture instead of lost to runoff or evaporation? Am I missing something?

Section 4.1.3: See my major comment #2. I am a bit lost looking at the results in this section because I am not sure what the effect of the LAI dataset is in Fig. 6 on the soil moisture (it seems more of an effect of a constant CCI land cover map shift discussed earlier in the methods in section 3.2.2). It seems the authors set out to evaluate effects of deforestation on water balance components (line 348). However, it seems to be realized after the fact that the LAI data does not entirely show deforestation and there are confounding effects of mean moisture between years. Can the land cover maps from ESA-CCI compensate for this? What might the effect of LAI time variation and the static CCI maps separately be? It seems like CCI maps are doing most of the work in causing much higher soil moisture between the scenarios. I also realize the following sensitivity section does discuss some of these issues. It might be helpful to make sure that these points are addressed in both of these sections and/or perhaps combine these two sections (4.1.3 and 4.1.4) to have a more organized discussion of the effects of land cover and LAI data.

Figure 6: Along the lines of the previous comment: could it be misleading to plot only LAI in Fig. 6 and not mention any other land cover parameter changes in the model because the reader may assume that the soil moisture changes are only due to LAI?

Line 350: Be sure to reference Fig. 5 somewhere.

Line 352: Can it be made more explicit here that (1) the REV_SV simulation will include the effects of deforestation because REV_SV includes assimilated observations that inherently account for less vegetation cover due to deforestation as well as (2) REV_S has a default vegetation climatology that does not account for deforestation? It took me some time to understand these ideas which I think can be stated more explicitly. This comment goes back to my question about why the Dry Chaco was chosen as the testbed. This idea could also be more explicit in the methods in describing REV_SV.

Line 384: Could there be a compensatory effect in the LSM in assimilating LAI: usually higher LAI in one year in a water limited ecosystem like Dry Chaco could be associated with high soil moisture as observed from satellites. However, if the high LAI is assimilated into the three LSMs it could be that the LSM is not able to account for the dynamic vegetation component (that higher soil moisture would result in more growth) and therefore the LSMs assume that this results in lower soil moisture due to more water use and more interception loss? Do the authors think an artifact like that is resulting here?

Line 406: "To compare the BL hydrology behavior. . ." this is a bit too general. Again, I am not sure what criterion the authors are planning to evaluating in looking at this behavior from the three models. One cannot say which is "better" with no benchmark data or criteria. I suppose there are qualitative features about land-atmosphere coupling that should be seen. Here or in the methods, the authors should make it clearer why they are looking at the efficiencies and what they expect in the plotted relationships.

Line 460 and Figure 13: Again, it might be helpful to point out that land cover parameter changes in REV_SV can make changes in addition to those from LAI (as discussed in lines 526-531)

Line 603: Again, I might not be clear on this, but both the updated LAI and land cover changes were made in REV_SV. Was it expected that time varying LAI would fully account for deforestation? If that was the case, why did the authors also change the

land cover parameter as well in REV_SV?

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