The authors thank the reviewer for his constructive comments. The comments are shown in ***italic bold fonts***, our responses are in regular fonts, and adjusted text from the manuscript is marked with "Correction:".
Please note that we use 'past' tense for our corrections (e.g. "this was added"), instead of future tense (e.g. "this will be added").

Reviewer 2

Format: #Page-#line

General remarks:

General 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.

We first would like to thank the reviewer for the positive feedback and comments. The first reviewer also had some comments on the title. We revised it and abstract revisions are shown below.
Title correction: Land surface modeling over the Dry Chaco: the impact of model structures and updated soil, vegetation and land cover parameters.

General 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

See our implemented changes below.

General 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.

The main novelty of our research is that deforestation is implemented by updating land cover and time-varying vegetation parameters simultaneously (in contrast to the mentioned studies whereby land cover or vegetation parameters are updated separately). In addition, we also made clear how models react differently to the implemented changes. This illustrates that the choice of a certain LSM matters when trying to understand hydrological changes after deforestation. We hope that this point is clarified in the proposed edits below.

1-3: 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.

<u>Correction:</u>
Line 1: In this study, we tested the impact of a revised set of soil, vegetation and land cover parameters on the performance of three different state-of-the-art land surface models (LSMs) within the NASA Land Information System (LIS). The impact of this revision was tested over the South-American Dry Chaco, an ecoregion characterized by deforestation and forest degradation since the 1980s. As most large-scale LSMs use climatological vegetation parameters and static land cover information, they may lack the ability to correctly represent the ongoing deforestation processes in the region. The default LIS parameters…

Line 14: The different hydrological response of various LSMs to vegetation changes may need further attention to gain benefits from vegetation data assimilation.

1-22: add that climate models are used to predict future climate

<u>Correction:</u>
Line 22: Furthermore, LSMs are an essential part of weather forecast and climate system models to simulate past, present and future climate. (Pitman, 2003; Clark et al., 2015)…

3-60: 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

<u>Correction:</u>
Line 34: In line with previous studies, our study assumes that satellite-derived vegetation indices can be used to represent large-scale vegetation changes in LSMs. However, updating these vegetation parameters alone is not sufficient. Besides vegetation indices,…

Line 51: Our study aims at implementing large-scale vegetation changes, including deforestation, in LSMs by feeding them with both temporally varying vegetation indices and land cover parameters.

3-60: 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.

Indeed, the strong deforestation in this area should make it abundantly clear (more so than another region) that the water balance performance should improve with the implemented changes. Further research will focus on the environmental consequences of deforestation and how disruptions in the hydrological cycle and soil water balance lead to widespread dryland salinity.

<u>Correction:</u>
Line 60: It is our hypothesis that by supplying LSMs with the best available soil parameters, together with time-varying vegetation, and land cover, the most accurate spatial and temporal representation of the regional water distribution can be obtained, especially over regions characterized by land cover changes. To test this hypothesis, the performance of three LSMs, using their default setup (outdated soil information, climatological vegetation and static land cover parameters) is compared to the performance with updated soil parameters, satellite-derived vegetation parameters and yearly updated land cover. The study domain is the South-American Dry Chaco, a well-suited area to test our hypothesis, due to its large-scale deforestation history. The region covers parts of Argentina, Bolivia and Paraguay…

5-132: 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.

This is a good remark. We agree that deriving GIMMS-based NDVImax and NDVImin values would indeed be a better solution. However, there are some challenges related to the derivation of GVF based on NDVI data. (In the manuscript, the reasoning behind the used approach was only briefly explained to not further complicate and elongate the paper).

Different approaches exist to calculate GVF from NDVI (a relevant paper describing the various issues is Jiang et al., 2008). They conclude that the method proposed by Gutman and Ignatov (used in this study) is one of the most suitable when deriving GVF from real-time satellite observations due to its simplicity and direct relation to real-time observed NDVI. When applying the Gutman and Ignatov method, there are different methods to calculate NDVImax and NDVImix. The values proposed by Gutman and Ignatov are global constants, independent of vegetation and soil type, and are corresponding to the NDVImin and NDVImax of the desert and evergreen clusters, respectively. Other studies proposed not using global values, but derive them over the specific region of interest of the study or using different NDVImax and NDVImin for different land surface types (Zeng et al., 2000, Miller et al., 2006). The disadvantages of the latter two methods are that the obtained values for NDVImax and NDVImin depend on the (size of) the region of interest and used land cover product. To make our methodology easily comparable with other studies and applicable over other areas, we decided to work with global constants. There are also different methods to calculate these global values (for example, Jiang et al. (2008) proposed to use the 5th and 95th percentiles to calculate NDVImax and NDVImin respectively, from the probability distribution function of the adjusted global weekly NDVI maps). This illustrates that there is no straightforward approach to derive GVF from NDVI data and that accompanying uncertainties are inevitable, even if global constants would be derived from the GIMMS-product itself. Given the fact that the values reported by Gutman and Ignatov are also AVHRR derived (just as the GIMMS NDVI), we believe that biases between both products are minimal. Also, note that the main goal of our research is to see if there are differences in modelled hydrology whether climatological or dynamic vegetation data is used. As long as the climatological and satellite-derived GVF are calculated the same, the question can be answered correctly. Lastly, our results indicate that small changes in LAI and GVF barely affect the modeled hydrology (and that different NDVImax and NDVImin would not cause significantly different results).

<u>Correction:</u>

Line 132: For simplicity, we used the global values proposed by …

7-192: I recommend adding the reason why parameters where computed using different parts of the soil for CLSM

We added the reference to the papers that describes the soil layering, parameters and relevant soil water processes: (Ducharne et al., 2000; De Lannoy et al., 2014)

Correction:
Line 192: By design, CLSM uses the 0-30 cm soil texture to compute parameters related to surface water transport, whereas the 0-100 cm soil texture is used for computation of all other parameters (Ducharne et al., 2000; De Lannoy et al., 2014).

8-233: 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?

We think that the discussion on albedo-schemes is relevant to clarify that albedo is implemented differently in each model. Albedo is directly affected by vegetation changes, and as shown by Yin et al. (2015), LSM output is also sensitive to albedo.

8-241: 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?

The impact of updated soil parameters on the water budget components is explained in paragraph 4.1.2. When comparing the REV_S water budget components with the ones of the baseline simulations, the impact of soil parameters can be analyzed.

In the REV_{SV} simulations, vegetation and land cover parameters are updated simultaneously. To disentangle the impact of land cover and vegetation parameters separately, sensitivity experiments were conducted.

Correction:
Line 241: Our results will reveal (see section 4.1.3) that the models react differently to the implemented dynamic vegetation and yearly updated land cover. To better understand this different behavior and disentangle the impact of vegetation and land cover parameters separately, different sensitivity experiments were conducted. The sensitivity of the various LSMs to LAI and GVF, or land cover changes was tested by synthetically varying the corresponding parameter values.

9-262: Why partition the evaporation components? Is the partitioning accurate enough in these land surface schemes to look beyond just the total evaporation?

Our results suggest indeed very large differences between the models in the partitioning of evapotranspiration components. However, if we would only look at the total ET, these differences would be hidden and we would not be able to make any conclusions about the quality of the used partitioning-schemes.

9-265: 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.

Indeed, the idea of the efficiency curves is to remove the dependence of soil moisture in ET-simulations. We agree that the ET-evaluation with GLEAM data still includes the soil moisture biases but it still gives a general idea on the quality of the ET-simulations. Under ideal circumstances, the combination of in situ datasets for runoff and ET would be used for evaluation, but these are not available.

10-280: If there is no independent, external dataset to evaluate runoff and evaporation 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?

In our study, the efficiency-space curves were not used to evaluate whether updated parameters improve the land surface representation, but only to describe the LSM differences. The main goal of the efficiency-space curves was to compare and visualize 1) the hydrological behavior of the three selected models and 2) the effect of the implemented parameter changes. To avoid confusion, we prefaced section 3.4 with the following:

Correction:
Line 266: Various LSM behaviors were further summarized and compared using efficiency curves, without associating any performance evaluation to them. Total runoff…

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

Sure, thanks.

Correction:
Caption table 2: Long-term (1992-2015) distribution of the BL water budget components [mm] for CLM, CLSM and NOAH over the Dry Chaco, year-round (Annual), for the months April-September (Dry season) and the months October-March (Wet season), respectively.

Line 321: The Dry Chaco receives an average annual P of 809 mm with most P (643 mm) falling during the wet season (October-March). All LSMs confirm a water storage (ΔS) deficit for the dry season (April-September), which is compensated during the wetter months with a water surplus.

12-340: 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. ~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?

Good remark. We believe that in the Dry Chaco, the water retention capabilities (represented by different SHPs) of different soil types are mainly determining the deeper soil moisture content. Due to a specific set of SHPs, sandy soils can hold less water than a silty soil and will be drier. In addition, the specific hydrological circumstances over the Chaco make that the largest fraction of the precipitation does not reach the deeper soil layers in most places due to the very high ET or Q_s-fluxes (low infiltration rates). This means that the deeper soil moisture is barely impacted by water fluxes and that the equilibrium soil moisture is mainly driven by the SHPs. ET and Q_S-fluxes are mainly determined by meteorological forcing.

Correction:
Line 344: Despite the relatively large difference in long-term mean soil moisture between BL and REV_S, the differences in ET and Q_S-fluxes are small. This is most likely because the meteorological forcings are the primary drivers of these fluxes, whereas the SHPs only have a secondary impact.

12-347: 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

Thanks for this suggestion. We understand the confusion and will better explain the reasoning behind both sections (see earlier comment on 8-241). You are correct that in Figure 6, soil moisture is mainly affected by a change in land cover and barely by LAI. We agree that figure 6 could be misleading by only plotting LAI. The figure was adapted by mentioning the land cover changes. The sensitivity analysis was indeed conducted to answer your questions. We hope that by our edits the structure of the paragraphs is now clear.

Correction:
Line 196: This specific setup with climatological vegetation and static land cover does not account for major vegetation changes, such as deforestation.

Line 233: The use of time-varying satellite-derived vegetation parameters together with yearly updated land cover parameters include the effect of vegetation changes (such as deforestation) as it inherently account for vegetation changes compared to the use of climatological vegetation parameters and static land cover in the BL-simulations.

Line 350: Keep in mind that the REV_S has a climatological vegetation and static land cover map that does not account for vegetation changes. The REV_{SV} simulations include the effects of vegetation changes by the implementation of satellite derived dynamic vegetation data as well as yearly updated land cover parameters.

Figure 6: The land cover change for the REV_{SV}-experiment are now clearly marked on the figure.

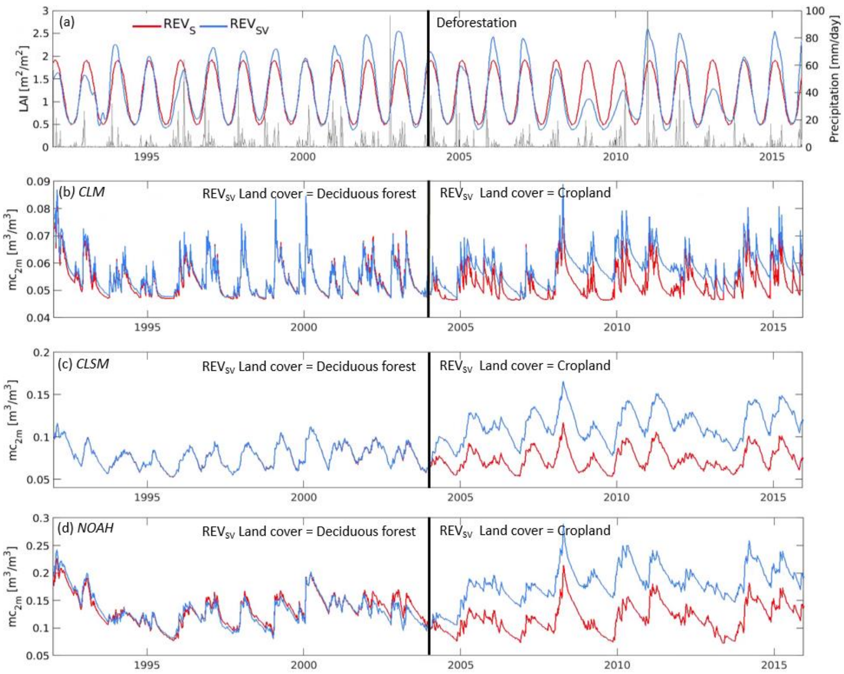

Figure 6: Along the lines of the previous comment: could it be misleading to plot only LAI in Fig. 6 and not mention any other land cover parameter changes in the model because the reader may assume that the soil moisture changes are only due to LAI?

We agree, please see our proposed changes in our reply above.

12-350: Be sure to reference Fig. 5 somewhere.

Line 351: Figure 5 summarizes the water budget components for the REV_S and REV_{SV} simulations after deforestation, i.e. for the period 2007-2015.

12-352: 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.

We agree, please see our proposed changes in 12-347.

13-384: 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?

Another very good remark, thanks, and this is indeed a good explanation.

In our LSMs, vegetation growth is not modeled and there is only a one-way dependency between soil moisture and vegetation, whereas in reality the dependency is two-way and not accounted for.

(To make sure that we are all on the same page: we inserted dynamic vegetation parameters, and did not dynamically assimilate them to update any state variables.)

Correction:
Line 384: The increase in soil moisture climatology for a decrease in vegetation is partly an artifact resulting from a one-way dependency of soil moisture to vegetation in the absence of a dynamic vegetation growth module. In nature, vegetation and soil moisture evolve together.

Line 584: Fourth, our LSM simulations did not include any dynamic vegetation growth module, which would couple soil moisture and vegetation two-ways, instead of only one-way.

14-406: "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.

See or reply on comment 10-280.

15-460 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)

Correction:
Line 457: Time series of the simulated 40° TbH with NOAH BL, REV_S and REV_{SV} input, and SMOS TbH are shown in Figure 13a. The used inputs in the RTM include simulated soil moisture (using FAO texture and related SHPs in the BL-simulations, HWSD based texture and SHPs in the REV_S and REV_{SV} simulations), temperature, LAI (climatological in $REV_{S,}$ dynamic in REV_{SV}) and land cover (static in $REV_{S,}$ yearly updated in REV_{SV}).

20-603: 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?

We hope that with our replies and corrections on the previous comments clarified the reason for both updating LAI/GVF and land cover simultaneously.