The authors thank the reviewers for their 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 1

Format: #Page-#line

General: With pleasure I have read this very interesting manuscript looking at a multi model / multi scenarios approach to simulate the hydrology of the Dry Chaco region in South America. It reads well and the results are supported by good quality figures. It is perhaps a bit long at some parts where results are mainly described and perhaps not enough explained / discussed. I have enjoyed the evaluation section and the diverse dataset used to evaluate your different scenarios (including the use of SMOS data!). The title focuses on deforestation pressure which we don't find much in the main body of the manuscript. Sometimes some numbers from the literature are provided but it is unclear either or not they match your own evaluation. To me this work leads to many questions towards data assimilation but also model development and this point could be stressed out in you manuscript. Perhaps a list of the next steps to work on could be clearly formulated in the conclusions. I suggest that this manuscript goes under major revision before to be considered for publication. I have annotated a pdf version of the manuscript with 58 (positive) comments and suggestion as a attempt to help.

We thank the reviewer for the positive feedback and comments. We revised the title and added an extra part in the discussion to clarify the novelty of our research, and the implications of data assimilation (see our replies to specific comments below).

Correction:
New title: Land surface modeling over the Dry Chaco: The impact of model structures and updated soil, vegetation and land cover parameters.

1-17: Ambiguous (at least to me), are you talking about the possible combination of the output of a LSM with satellite data or of the use of satellite data integrated into the LSM leading to the output? Integrating observations into models covers several aspects: (1) the dynamic integration of observations into models through data assimilation techniques, (2) the use of observations for model validation and evolution and (3) the mapping of the model parameters used to characterize the representation of land properties within the model (e.g., soil properties, land cover). In that respect I assume that most (if not all) LSMs are using satellite data.

Correction:
Line 17: The output from LSMs is used for many applications such as the monitoring of water resources, floods and droughts, and their impact on natural hazards, biomass production, ecology or soil salinity. In many cases, LSM performance is improved by the inclusion of remotely sensed observations through (i) the dynamic integration of observations into models through assimilation techniques, (ii) the mapping of model parameters to characterize the representation of land properties within the model (e.g., soil properties, land cover) and (iii) the use of observations for model validation and development. In addition, contrasting model output with remote sensing is a powerful method to identify unmodelled processes in a LSM,

1-19: Not only human-induced, more generally it permits to highlight missing processes in models

Good remark, the sentence was corrected (see corrections at 1-17).

1-23: Please add references

We added following references at line 24:

Clark, M. P.; Fan, Y.; Lawrence, D. M.; Adam, J. C.; Bolster, D.; Gochis, D. J.; Hooper, R. P.; Kumar, M.; Leung, L. R.; Mackay, D. S. & others. Improving the representation of hydrologic processes in Earth System Models Water Resources Research, Wiley Online Library, 2015, 51, 5929-5956

Wood, E. F.; Roundy, J. K.; Troy, T. J.; Van Beek, L.; Bierkens, M. F.; Blyth, E.; de Roo, A.; Döll, P.; Ek, M.; Famiglietti, J. & others. Hyperresolution global land surface modeling: Meeting a grand challenge for monitoring Earth's terrestrial water Water Resources Research, Wiley Online Library, 2011, 47

2-56: Rank by chronological order (?)

Thanks for the remark. The order of the references was corrected

3-63: It is perhaps only me but using future ("will") seems odd (?)

<u>Correction:</u>
Line 63: The performance of three LSMs with various soil, vegetation and land cover parameters, is evaluated over the Dry Chaco to test this hypothesis.

3-81: Don't you have more recent reference this this?

<u>Correction:</u>
Line 81: The ecoregion has a semi-arid climate with a north-south gradient in the mean annual temperature (from 24°C to 19°C) and annual rainfall (from 898 mm/year to 712 mm/year) (Marchesini et al., 2020). Minetti et al. (1999) reports precipitation values up to 1000 mm/year in the eastern and western parts of the region and 400 mm/year in the central Dry Chaco.

.4-93: Does the use of the ESA CCI Land Cover dataset permit to retrieve the 20% loss from Hansen et al., 2013, Vallejos et al., 2015 mentioned above?

Thanks for the suggestion. We compared the upscaled and reclassified ESA-CCI land cover change map (as shown in figure 1) with the deforestation dataset composed by Vallejos et al. (2015). Note that both products cover a different period. The ESA-CCI land cover product covers the period 1992-2015 upscaled to 0.125° (±12.5 km), whereas the Vallejos dataset covers the period 1972 to 2013 at 30 m resolution. When analyzing the overlapping period, we found that the general spatio-temporal of deforestation patterns match very well, but that the area of deforestation differs (23% of the Dry Chaco based on the rescaled ESA CCI-map and only 15%

for the Vallejos dataset). The main reason for the discrepancy is the different resolution of both products.

Correction:
Line 94: Figure 1 shows the location of the Dry Chaco, together with the spatial and temporal extent of land cover changes for the period 1992-2015 derived from the European Space Agency-Climate Change Initiative (ESA-CCI) land cover product upscaled to a 0.125° resolution (see section 2.5). The derived spatio-temporal pattern of deforestation agrees well with the 30 to 60 m resolution deforestation product of Vallejos et al. (2015) (not shown). The deforested area of the Dry Chaco in the overlapping period (1992-2013) of both products is 23% for the 0.125° ESA CCI data and only 15% for the finer-scaled dataset of Vallejos et al. (2015). The main reason for this discrepancy is the different spatial resolution of both products.

4-105: Would it make more sense to spin up 10 times the year 1992 then?

This is a valid remark. There are indeed two possibilities to spin up the models: (1) spin up 10 times the year 1992 (correct land cover, but at the risk of using anomalous meteorological conditions) or (2) spin up the models for the period 1982-1991 (incorrect land cover but including using climatological meteorology). We decided in favor of option 2.

4-106: Please provide more details, which variables are you considering?

Correction:
Line 106: The meteorological forcing data (precipitation, temperature, specific humidity, radiation, wind and surface pressure) were extracted from…

4-115: Please add reference

The relevant references were added.

Correction:
Line 115: In this study, vegetation, land cover and soil input data was revised using the Global Land Surface Satellite (GLASS) LAI (Liang et al., 2013; Xiao et al., 2016), the Global Inventory Modelling and Mapping Studies (GIMMS) NDVI (Tucker et al., 2005; Pinzon and Tucker, 2014) the ESA-CCI land cover product (Kirches et al., 2014; ESA, 2017) and HWSD soil properties (FAO and ISRIC, 2012; De Lannoy et al., 2014).

4-119: Is it an average? what about extended period of high cloud cover?

The GLASS product by itself provides already a spatially complete LAI-map every 8 days and was used as input in the different LSMs. Cloud-contaminated data are removed and gap-filled using an optimum interpolation algorithm to obtain continuous and smooth surface reflectance values (Xiao et al., 2016). The MODIS surface-reflectance data has an 8-day temporal sampling period. The maximum-value composite approach is used to composite the daily AVHRR surface-reflectance data into 8-day intervals to maintain a temporal resolution consistent with the MODIS surface-reflectance data. The approach selected the AVHRR reflectance data with the highest normalized difference vegetation index over each eight-day time (Xiao et al., 2016).

The GIMMS product is assembled from AVHRR NDVI (and does not include SPOT or MODIS data as initially mentioned in the manuscript) and provides spatially complete NDVI-map every

15 days. The maps are 15-day maximum value composites. Cloud or snow contaminated pixels are retrieved from either spline interpolation or average season profiles.

Correction:
Line 120: Cloud-contaminated data are removed and interpolated using and optimum interpolation algorithm (Xiao et al. 2016).

Line 128: The maps are 15-day maximum value composites and cloud contaminated pixels are replaced by NDVI-values derived from either spline interpolation or average season profiles.

4-121 : such as?

Correction:
Line 121: According to Liang et al. (2013) and Xiao et al. (2016), the GLASS LAI features more realistic and smoother seasonal variations than the MODIS LAI product (MOD15) (Knyazikhin et al., 1998) and the first version of the Geoland2 (GEOV1) (Baret et al., 2013) LAI product.

4-121 : Isn't the development proposed by Kumar et al., 2019 JHM on LAI data assimilation already implemented in LIS? see: (https://journals.ametsoc.org/view/journals/hydr/20/7/jhm-d-18-0237_1.xml) This would allow a better description of inter/intra annual LAI?

Yes, the LAI data assimilation is already implemented in LIS, but only for NOAH-MP, which has a dynamic vegetation module. The CLM and CLSM versions in LIS do not have a dynamic vegetation module. Our study focusses on long-term trends and parameter sensitivity. We believe that the implementation of 8-day LAI and 14-day GVF data is sufficient to capture most relevant intra/inter annual vegetation changes.

5-130: Better to put Land Cover in the section title (?) It is my impression than LC is not as much wide spread as LAI / NDVI (and could also have another meaning)

Thank you for the suggestion. The LC abbreviation was changed to land cover everywhere in the text to improve the readability of the manuscript.

5-139: Please also refer to ESA, 2017 Land Cover CCI Product User Guide 2 (https://climate.esa.int/media/documents/CCI_Land_Cover_PUG_v2.0.pdf)

Thanks for the extra reference. The reference was added.

5-141: Does the use of the ESA CCI Land Cover dataset permit to retrieve the 20% loss from Hansen et al., 2013, Vallejos et al., 2015 mentioned above? The analysis leading to figure 1 deserves to be enhanced further.

The question was addressed in 4-93.

6-156: Mention SMOS if appropriate. This is interesting as I have done something like that few years ago, please see: Albergel, C., Balsamo, G., de Rosnay, P., Muñoz-Sabater, J., and Boussetta, S.: A bare ground evaporation revision in the ECMWF land-surface scheme: evaluation of its impact using ground soil moisture and satellite microwave data, Hydrol. Earth Syst. Sci., 16, 3607–3620, https://doi.org/10.5194/hess-16-3607-2012, 2012.

We now mention SMOS in the paragraph. The reference to the paper was added in line 303 and following line was added:

Correction:
Line 303: For an integrated evaluation of the model sfmc, surface temperature and LAI of the various experiments, a zero-order tau-omega microwave Radiative Transfer Model (RTM) was used to convert these modeled variables into L-band Tb [K] estimates. The modelled Tb were compared to SMOS Tb observations, similar to the approach presented by Albergel et al. (2012).

6-166: Is this useful?

The' ©Stevens' was removed.

7-187: How do you then disentangle the impact of soil, vegetation and land cover parameters? I would have praised for a more incremental approach.

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 indeed updated simultaneously (see also below, comment 8-233). We claim that an incremental approach would have resulted in at least one set of unrealistic simulations (the ones whereby or land cover or vegetation would remain static), would lead to more questions (which set of parameters does remain static in an incremental approach?) and also further complicate our evaluation.

To disentangle the impact of land cover and vegetation parameters, sensitivity experiments were conducted. We believe that the sensitivity experiments are more relevant to get insights in model behavior related to one specific set of parameters than an incremental approach. For example, they allow for larger (even unrealistic) LAI/GVF ranges to better understand model behavior to specific parameter changes.

8-231 : Please correct me if I am wrong but my impression is that 8 and 15 days are the best possible scenario (?) what is the longest time gap between 2 observations you encounter? This is important when it comes to daily interpolation (although I assume that this is likely to happen in e.g. winter time when vegetation is dormant hence not varying a lot).

See our answer to comments 4-119

8-233: This is a very interesting procedure as models do not need directly 2D land cover information, they need 2D parameters, and the models or pre-processing uses the land cover as predictors of the parameters, this ought to be discussed at one point.

The point was already briefly mentioned in the introduction (line 45), but might not have been clear. We added some extra lines in the section to clarify the issue.

Correction:
Line 233: LSMs only use land cover information to define model-specific parameters associated to each land cover type, i.e. ESA-CCI land cover is used as predictor of parameters such as rooting depth, stomatal conductance and surface roughness. The exact values and implementation of each parameter is model-dependent.

8-241: Just a suggestion: wouldn't it make more sense to present this set of synthetic experiment before 3.2.1 and 3.2.2?

Thanks for the suggestion. We understand the confusion. We decided not to restructure any sections but to better clarify the reasoning behind them. Following sentence was added:

Correction:
Line 241: Our results will indicate (see section 4.1.3), that the various LSMs react differently to the simultaneously updated interannually varying vegetation and land cover parameters. To disentangle the impact of vegetation and land cover parameters separately for each LSM, two sets of synthetic sensitivity experiments were conducted

9-253: But then it makes no sense with the land cover? Please clarify.

To disentangle the impact of land cover changes only, the vegetation had to remain static/climatological. The experiments are synthetic and are only conducted to understand the model-behavior to different parameter-changes.

9-265: Please provide more information on how to read the results (e.g. Figure 9)

Correction:
Line 289: For each model, we plotted both the Q and ET efficiency in function of modelled soil moisture to visualize the different soil moisture dependencies. […] The resulting 'efficiency space' plots (showing the obtained ET efficiency in function of Q efficiency) show how evaporation and runoff efficiencies vary with each other, as the soil gets drier or wetter.

10-290: I thought it (=P) was an input

Correct. It was decided to also evaluate the precipitation input as it is a crucial input variable in the calculation of soil moisture, evapotranspiration and other hydrological variables. To test the quality of the MERRA2-precipitation product, we claim that an evaluation with in situ data is crucial. Insights in the quality of the input-precipitation is also important for the ensuing evaluation. (Poor precipitation quality would result in poor evapotranspiration, soil moisture and Tb evaluations, when compared to independent data).

Correction:
Line 290: For each experiment, the quality of model input P, and the model performance in terms of output surface soil moisture content (sfmc) and ET were evaluated against independent data.

10-302: In a previous study we have used CMEM to do similar evaluation of a new parametrization in the HTESSEL LSM: Albergel, C., Balsamo, G., de Rosnay, P., Muñoz-Sabater, J., and Boussetta, S.: A bare ground evaporation revision in the ECMWF land-surface scheme: evaluation of its impact using ground soil moisture and satellite microwave data, Hydrol. Earth Syst. Sci., 16, 3607–3620, https://doi.org/10.5194/hess-16-3607-2012, 2012.

We referred to the study in line 303. See our reply on comment 6-156.

11-321: Interesting - is it in agreement with Minetti et al., 1999 used in the section describing the studied area?

Yes, the yearly mean values are in agreement with values mentioned in literature (Marchesini et al., 2020, Minetti et al., 1999). In addition, the observed spatial gradients (dry in the central part, and wetter eastern and western parts) are in agreement with literature.

<u>Correction:</u>
Line 321: …, which is in line with the findings of Marchesini et al., (2020) and Minetti et al. (1999).

11-331: Interesting to see/note that for each model the distribution is respected amongst the 2 configurations (e.g. NOAH as higher Ev than the other)

Indeed.

12-349: Does it corresponds to the number of 20% presented above?

The deforestation patterns based on the ESA-CCI land cover product are similar to the ones mentioned by Vallejos et al. (2015). See our reply on 4-93.

12-359: Perhaps you could provide lon/lat?

<u>Correction:</u>
Line 357: The impact of vegetation changes on the temporal evolution of LAI and moisture content in the first two meters of the soil (mc2m) is illustrated in Figure 6 for a representative pixel (28.0625° S, 63.6875° W; marked in Figure 7a with a red circle).

12-359: OK - this is why the initial values are not the same (?)

Yes, as the climatological and inter-annually varying LAI are not the same, also soil moisture values between the REV_S and REV_{SV} simulations can differ from each other, even before the deforestation period.

12-361: It is indeed something we observed after large fires, LAI will go back to high value only few years after but not VOD or above ground biomass. Is the latter an output of your system as well?

Good point and unfortunately, VOD and above ground biomass are no output of the systems included in this study.

13-371: Is this Figure really useful? One may say that you have many figures and some are only briefly described. Perhaps that some could be moved into a supplementary file?

We agree that there are already many figures and removed figure 7 together with lines 371.

13-400: Please revise the order of the figure (?). Also it is interesting to see that the 2 configurations (SV/S) leads to different behavior for different models

We hope that by our edits described in 8-241, the order of the figures makes more sense.

15-432: Not sure that this evaluation is adding much to the study (which already contains a lot of material)?

See our answer on 10-290

15-449: Did you use daily or monthly data for the evaluation?

Daily data. This important detail was added to the text.

Correction:
Line 449: The skill of daily simulated total ET relative to that of GLEAM-based ET estimates is shown in Figures 12a-d for the period 1992-2015 over the entire Dry Chaco.

15-457: Please remind the reader what are they

Correction:
Line 457: Time series of the simulated 40° Tb at horizontal polarization (Tb_H) with NOAH BL, REVS and REVSV input, and SMOS Tb_H are shown in Figure 12a. The inputs used in the RTM for the simulated Tb include simulated surface 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 BL and REV_S, interannually varying in REV_{SV}), land cover (static in BL and REV_S , yearly updated in REV_{SV}) and the associated literature-based look-up RTM parameters.

16-481: You have to clarify what you mean here.

Correction:
Line 481: For NOAH, the ΔR values do not increase everywhere. At some pixels with reduced REV_{SV} performance, we noticed unexpected trends in the LAI time series (not shown), i.e. LAI would not show the expected decrease during the dry season. This possibly deteriorated the Tb simulations.

17-507: please consider rephrasing

The text between brackets (= "often hardwired") was removed from the text.

17-510 : Does that mean that this model can not be trusted? needs further development?

Correction:
Line 511: LSMs could benefit from further development towards a more realistic response to vegetation changes and advances in including dynamic vegetation phenology. This should lead to more realistic simulations of the interaction between the carbon and water cycles.

18-526: But Evap is (negatively) affected right, but not through water extraction by vegetation (?)

Correct, the decreased ET in CLSM (figure 5) is caused by land cover parameter changes as indicated in line 531. An extra line of clarifications is added in line 531.

Correction:
This is related to the distinct implementations of root distribution and root water uptake (controlled by the stomatal conductance and rooting depth) in the various LSMs, impacting the E_V and related water extraction from the soil.

18-530: Perhaps this could be briefly described in section 2.2 on models

Extra information was added in section 2.5 (see our reply on question 8-233).

18-547: I am not convinced that this is adding much to your manuscript (?)

See our answer on 10-290

20-617: From what is described in the manuscript, DA may not be the answer and it is rather an improvement of the different models processes that is needed (?) Perhaps a line or two could be added in the discussion to reflect this idea?

We fully agree. See correction to 17-510. We removed one sentence (line 618: "This optimization still requires more research") in the conclusions and replaced it as follows.

Correction:
Line 618: Vegetation data assimilation can only have the desired impact, if the sensitivity of simulated hydrological fluxes to vegetation changes is realistic and this may need further research, especially at the global scale.

Figure 3: Figures must be self-explanatory, please add what Qsb. Qs...are

The necessary explanations of abbreviations were added to all figures.

Figure 4: Please indicate panels (a) and (b)

Thanks for the comment, this was a small mistake in the figure caption.

Correction:
Maps of long-term (1992-2015) NOAH mc_{1m}, obtained with (c) BL and (d) REV_S parameters.

Figure 6: LAI does not impact CLSM soil moisture content (?)

Correct, the reason why is explained in the sensitivity-analysis and shown in figure 8f. We hope that with the extra clarifications, this is clear now.

Figure 8: Very difficult to see

The sub captions of figure 8d-i were moved up (above the subplots) to improve readability.

Table 2: Although presented in the caption this is not very clear

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.

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.

Correction:

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. Most large-scale LSMs may lack the ability to correctly represent the ongoing deforestation processes in this region, because 5 most LSMs use climatological vegetation indices and static land cover information.

Line 14: Our results suggest that 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

Correction:

Line 22: Furthermore, LSMs are an essential part of weather forecast systems and climate system models that 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

Correction:

Line 34: In line with previous studies, our study assumes that interannually satellite-derived vegetation indices can be used to improve the vegetation representation 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.

Correction:

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 is compared, using either their default setup or using updated soil parameters, interannually varying satellite-derived vegetation indices and yearly updated land cover. The study domain is the South-American Dry Chaco, a well-suited area to test our hypothesis. 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 chose to use the values proposed by Gutman and Ignatov (1998), i.e. 0.04 for NDV Imin and 0.52 for NDVImax, and the GVF is restricted to the 0-1 interval. Multiple other approaches exist to derive GVF from NDVI, each with their own advantages and limitations (Jiang et al., 2010).

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 indicate (see section 4.1.3), that the various LSMs react differently to the simultaneously updated interannually varying vegetation and land cover parameters. To disentangle the impact of vegetation and land cover parameters separately for each LSM, two sets of synthetic sensitivity experiments were conducted.

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:

<u>Correction:</u>
Line 266: The relative differences in the various LSM behaviors were further analyzed using efficiency curves, without associating any performance assessment.

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

Sure, thanks.

<u>Correction:</u>
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) 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 QS fluxes are small. 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: This setup includes the effect of vegetation changes
(such as deforestation) 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.

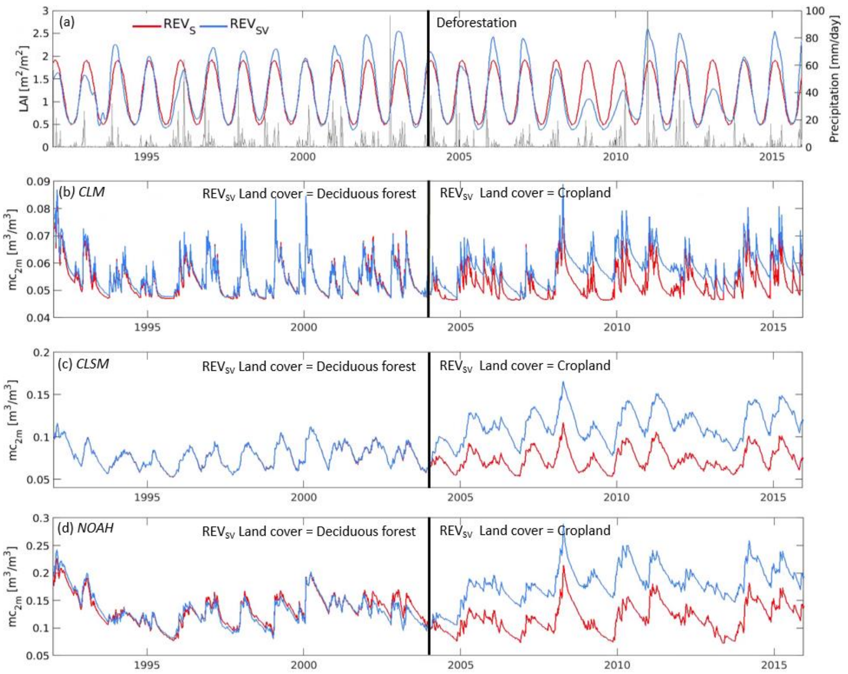

Figure 6: Along the lines of the previous comment: could it be misleading to plot only LAI in Fig. 6 and not mention any other land cover parameter changes in the model because the reader may assume that the soil moisture changes are only due to LAI?

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 an imposed decrease in LAI (while assuming a persistent vegetation type) 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, LAI 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° Tb at horizontal polarization (Tb_H) with NOAH BL, REVS and REVSV input, and SMOS Tb_H are shown in Figure 12a. The inputs used in the RTM for the simulated Tb include simulated surface 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 BL and REV_S, interannually varying in REV_{SV}), land cover (static in BL and REV_S , yearly updated in REV_{SV}) and the associated literature-based look-up RTM parameters.

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.