Revision of Ms. gmd-2014-123, 2014 entitled "Predicting the response of the Amazon rainforest to persistent drought conditions under current and future climates: a major challenge for global land surface models" by E. Joetzjer et al.

We are grateful to both reviewers and Dr. Sato for their helpful and constructive comments. Please, find hereafter our point-by-point response in italic.

Dr. Sato and the two anonymous reviewers,

Following reviewer 2 advice, the manuscript has been partly rewritten and the flaws have been corrected. A revised manuscript with a track of all changes would be difficult to read. Thus, we send you the revised manuscript with no track changes.

Reply to Reviewer 1

The authors explore the ability of the ISBAcc land surface model to represent drought response of the Amazonian forest. A combination of four climate scenarios and four strategies of tree responses to drought are considered to match observations of two through fall exclusion field experiments. The main conclusion is that ISBAcc represents well the soil hydrology but poorly represents the response of the vegetation to drought due to a lack of mechanistic plant drought response mechanisms.

Please better define what are the physiological meanings of the linear and SiB3 water stress functions? Could you describe in few words how the plant responds to different soil moisture contents according to these functions?

Following reviewer 2 advice, the manuscript has been partly rewritten. The vegetation response to drought simulated by the linear and SiB3 WSF is now better explained (cf section 2.1.2 line 159-162). Note however that theses functions were not meant to represent a specific strategy to drought but a decrease in g_s, LE and GPP when the soil is getting drier.

Page 5309, line 10: the s of gs needs to be an index.
Corrected (line 332 in the revised version)

The meaning of gs is explained in the table 1 and 3 but not in the text and you are using it a lot. Maybe it could be nice to have it in the text too for readers less familiar with this this term, and to see it contrasted with mesophyll conductance.
The mesophyll conductance, as it is defined in $ISBA_{CC,}$ is explained line 130-131 and gs is now defined line 137.

Page 5309, line 11: please clarify that the LAI is also overestimated in Tapajós for 2002.
Ok, cf. line 332

Page 5311, line 17: is fo a typo or do you mean f0?
f_o, corrected (line 396 in the revised version)

Page 5313, line 11: typo: imporove instead of improve.
Ok

You start by saying in the second section that the two original water stress functions have been calibrated on saplings of Pinus pinaster and Quercus petraea. However in your discussion you simply say that the concepts of iso- and anisohydric plants are not suitable for ISBAcc.

This is a misunderstanding, and we have clarified the text accordingly (cf. Lines 390-396). Indeed, it is not the concepts of iso and anisohydric response to drought which is not suitable, but the parameterization of avoiding and tolerant WSF as originally implemented in $ISBA_{CC}$. *First, the tolerant and avoiding WSF are not consistent with the concepts that they meant to represent. Indeed, as observed, isohydric species show a strong control on* g_s*, while anisohydric species are able to exert little or no stomatal control in response to drought (Tardieu and Simonneau, 1998; van der Molen et al. 2011). But, if you look at the figure 1, there is little difference in the* g_s *response to drought when using the tolerant or the avoiding WSF, which was also pointed out by Gibelin et al. (2008). Second, as explained, there is no evidence (except the paper of Picon et al. 1998 based on sapling) for a relationship between* f_o *and SWI. That's why, we tested two functions similar in the gm response to drought but with a constant* f_0 *based on in situ observations.*

Please could you comment more on what the lesson's learned from this modeling exercise mean for how to parameterize, and measure in the field, a tropical WSF.

Ideally, to parameterize a process in a particular ecosystem, modellers should use measurements made on species from that particular ecosystem (as many as possible) and not from saplings that can have very different responses than mature plants. This is of course not always possible but should be done as soon as measurements become available. Since we did not have such observations, we chose simpler parametrizations that gave better results relatively to the observations. Second, in order to avoid the need of a water stress function, as discussed in section 4.1, modellers should start represent the water column pressure within the soil, roots and xylem. This approach could also be a way to represent cavitation and so represent one of the major processes linked to drought induced mortality. Besides, according to us, proposing experimental design and protocol to improve the WSF; or better, physiological processes in case of drought for most LSMs would require a separate review paper.

Reply to Reviewer 2

My feeling is that the author(s) made a reasonable effort for conducting this research and so, this paper is potential. However, the present paper looks just a report of the simulation results using some models without their analyses. I am not sure what the overarching scientific question is in this paper: new insights, new methods, new findings and something original. What are the findings from this investigations that the author(s) wanted to emphasize? I am also feeling "Haste makes waste": many typos, wrong manners to write a scientific paper, some references that didn't appear in the list (e.g., Noilhan and Planton, 1989 and so forth), a lack of explanation about new theories such as incorporating gm and f models into ISBA, defective figures and their captions, incomprehensible tables, too many figures, most of which are useless, resultant illogical development of story. I think most of flaws in the current manuscript can be fixed by discussion with co-authors. The author(s) should be more careful in writing a scientific paper before submitting it. Note that I could not list all points up because of too many flaws.

After re-reading the manuscript, we indeed found many typos, incorrectly referenced figures and some missing references. We corrected them but, as you mentioned, there were too many to list them all. To address your doubts about the manuscript, we rewrote parts of it, because most of your comments were probably due to a lack of clarity from our part. For example, you mentioned "new theories such as incorporating gm and f models into ISBA". Actually, we didn't implement g_m *and f in ISBAcc, neither the avoiding and tolerant WSF. This work was done by Calvet et al., in 1998,* *2001 and 2004. When you say incomprehensible table, it is very likely that you refer to Table 2, which is, we agree, quite intricate due to the highly parametrized avoiding and tolerant WSF implemented and described in Calvet et al. 2004. We modified the table to make it easier to read (removed references already in the text, used a bigger font, wider line spacing, added missing definitions). Note that the figure showing measured sap flow vs. simulated evapotranspiration was removed as it was not crucial. We believe this revised version is more concise, much clearer and let the reader focus on the scientific results.*

The critical points are: The studied models' performance to reproduce observations of soil moisture status must be fundamental in this paper. However, all the models could not succeed in simulating the soil moisture observations. This must result in ruining reliability of the following simulations such as SWI, gs, and ecosystem fluxes as the most important outputs, and further, the future projections. What you have to/ can do is just admitting the reproduction errors and mentioning what factors cause them. It might be interesting and useful to mention how the simulation errors for soil moisture propagate the errors in ecosystem fluxes (e.g., LE and GPP).

The issue of soil water content simulation is an interesting and important point. Indeed, to represent the vegetation response to drought, it is a pre-requisite to correctly capture the soil moisture content. We agree, as water and carbon cycle are coupled in land surface models, the evaluation of $ISBA_{CC}$ sensitivity to the WSF parametrization necessarily implies to look at the hydrology response.

First, note we evaluated the hydrology of $ISBA_{CC}$ with in situ data using the fluxtowers from the Amazon basin and Guyana (Santarem K83, K67, Reserva Jaru, Manaus and Paracou), in situ soil water content (Santarem K67 and K83) and river discharge at regional scale. With the original (tolerant and avoiding) WSF, $ISBA_{CC}$ systematically underestimates evapotranspiration, and consequently overestimates the soil moisture content and river discharge. Using a constant and in situ based f_0 at 0,74 (as for the linear and SiB3 WSF) lead to a much more reasonable hydrology. So even when there is no soil water stress (SWI >1), the new f_0 is more suitable than the one calculated with the original WSF. We believe that with the linear and SiB3 WSF $ISBA_{CC}$ reasonably represents the soil water content for both CTL and TFE plots with correlations around 0,8 (at Caxiuanã, at Tapajós due to the reconstructed forcing, correlations are lower), and bias are around 4%, and even smaller at Tapajós for the TFE experiment (Fig. 1). We are aware that these simulations are not perfect, but the lack of observed vertical profile of texture, wilting point, field capacity and so on prevent us to completely constrain the model.

As it now better explained in the manuscript, the original WSF fails to correctly simulate the reduction of the soil moisture. Consequently the vegetation impacts induced by the rainfall exclusion treatment are underestimated. If you take the year 2003 as an example when soil moisture is very well simulated at both sites with the linear and SiB3 WSF (Fig. 3), you' ll notice that fluxes (LE & GPP) reduction induced by the exclusion experiments differ (Fig. 7) between the linear and SiB3 WSF. This illustrates the importance of the choice of the WSF compared to the soil moisture content. Therefore, one aim of this study is to evaluate and validate a WSF regarding both ISBAcc hydrology and vegetation behavior in case of a persistent drought.

The Discussion section is a good review, but the author(s) should note that Discussion section should be addressed based on the results obtained in this investigations. In short, the current Discussion section is not one for an original article. I suggest that the author(s) should state: 1) there were many simulation failures, 2) why such failures were caused, and 3) how to overcome or minimize the failures. As a current status, this paper should be rejected, but I believe this study will be improved and I can see this paper again.

We believe this revised version of the discussion is clearer, we explicitly discuss the importance of the choice of the WSF and explain why the original WSF were not suitable. If the linear one allows to better reproduce both hydrology and fluxes drought-induced impacts, we also highlight missing processes in $ISBA_{CC}$ *such as drought induced changes in respiration.*

We chose not to restrict our discussion to $ISBA_{CC}$ *performances and broaden our results to other "state of the art" LSMs used in coupled simulations. Indeed failure found in this study are common to other LSMs. Therefore we discuss, through a short review of the literature, processes and uncertainties in tropical land surface modelling when in drought.*