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Interactive comment on “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.

Anonymous Referee #2

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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

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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. 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 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.

Interactive comment on Geosci. Model Dev. Discuss., 7, 5295, 2014.

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