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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Impacts of future deforestation and climate change on the hydrology of the Amazon basin: a multi-model analysis with a new set of land-cover change scenarios" *by* Matthieu Guimberteau et al.

Anonymous Referee #1

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The study of Guimberteau et al. applied three land surface models forced with three different GCM's in combination with different land use change scenarios to the Amazone basin. The authors show that due to climate change alone temperature increases, as well as precipitation and evaporation. Under the different land use change scenarios, transpiration decreases and runoff increases. In general, the article is very interesting, but some clarifications might be needed.

General:

1) One point of confusion is introduced in Line 4-5 from page 6, Section 2.3. As I

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understand correctly, all the percentages and differences for the scenarios with future land use change (LODEF, HIDEF and EXDEF) are relative to a future scenario with climate change only (NODEF). This would mean that all the percentages and differences mentioned in, for example, Section 3.3 and Figures 9 and 10, are referring to the difference between two future scenarios at the same point in time (the year 2099). In this way, figures like Figure 15 are not very fair, as apples and oranges are compared, with a different benchmark. It may be more interesting to compare all the results with the same benchmark (thus, HIST and the year 2009). Now, it becomes hard to answer, for example, the question posed in the title of Section 4.1, as the percentages only reflect the isolated effect of deforestation. Thus, it can only be concluded that evaporation with a forest land cover is higher compared to non-forest. Considering this, I would like to point at other land use change experiments, which in general show that water yields increase after deforestation, in line with the findings presented here (e.g. Hornbeck et al., 2014; Rothacher, 1970; Swift and Swank, 1981). Overviews of these experiments are given by, for example, Bosch and Hewlett (1982), Andréassian (2004) and Brown et al. (2005). I may misunderstand the percentages used in the manuscript, but, in that case, please try to clarify what you are actually comparing and try to state clearly what the percentages are relative to.

2) I find the discussion in section 3.3.3 very interesting. Nevertheless, the modelled and observed river discharges in Figure 14, may add several discussion points. It can be noted that Orchidee is much closer to the observations (HIST-scenario) compared to LPJml (for subfigure MAIN, AMAZ). In this way, it can be argued that Orchidee may better represent the current processes, and may (but not necessarily) also better reflect what happens in the future scenarios. The opposite reasoning may also hold. Why trust a model that is not able to capture the historical series? Anyway, it may be interesting to reflect on these issues.

3) Throughout the paper, the term evapotranspiration (ET) is used, whereas the more general term ‘evaporation’ or ‘total evaporation’ may be more clear. I would like to

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refer to Savenije (2004) for some arguments to not use the term evapotranspiration as well. Briefly, transpiration is a rather different process compared to, for example, interception evaporation. Especially with regard to deforestation, it is important to make this distinction, as it is probably transpiration that decreases.

Detailed comments:

P4.L30. What do you mean with “business-as-usual”? The current situation?

P12.L1. Idem

P23.Fig3. Define the abbreviation of ASO (now only later in the text)

P33.Fig.14. Why is the third model not shown?

Concluding, I think the work is interesting and worth publishing in HESS. Nevertheless, some efforts will be needed to clarify the issues stated above.

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