

Interactive comment on “The evolution of root zone moisture capacities after land use change: a step towards predictions under change?” by Remko Nijzink et al.

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We would like to thank Anonymous Referee 2 for his/her feedback. We will try to improve on the raised issues.

General comments

“In general, I find the paper too long. Maybe some details of the methodology can be moved into the Supplementary Material”

Agreed. We will shorten some parts of the manuscript.

“I suggest to be more precise in the title. First, ending the title by “under change”

C1

seems quite strange to me. Is it still land use change, or climate change or other ? (same remark at line 10 of page 2). Then, “predictions” is too vague because it can be applied to many processes (prediction of discharge, of flood, of vegetation dynamics...). In addition, more discussion on the potential applications with this kind of method is needed in the conclusion and perspectives.”

We rephrased the title to: “The evolution of root zone moisture capacities after deforestation: a step towards hydrological predictions under land use change?”. In addition, we will add a discussion on practical applications of the method in conceptual modelling (also suggested by Referee 1).

“The results and the figures, which include many hydrological signatures, are not always simple to read and to analyze. Then, the interest of the discussion can be lost during the reading of Section 4. Thus, I would recommend to split this section in 2 sections to distinguish Results and Discussion.”

We decided to merge the results and discussion in order to avoid repetition and to make the article more concise. We still prefer to keep it like this, also with regard to the first comment (the paper is still rather long). Nevertheless, we will have a critical look at the figures and discussion, and will try to clarify wherever we can.

Specific comments

Abstract

1/ “long-term data” => you can be more precise

2/ line 24 of page 2: “better representations of high flows and peak flows” => what about the low flows ?

1. We changed it to “long-term data (30-40 years of observations)”
2. The low flows improved for the Hubbard Brook catchments, whereas the low flows

C2

did not show improvements in the HJ Andrews catchment. See also page 24, lines 13-26.

Introduction

3/ *To be more precise, the vegetation partitions first precipitation into interception, stemflow and throughfall. Then, the fraction of rainfall that reaches the surface is partitioned into evapotranspiration, drainage and also surface runoff.*

4/ *line 28 of page 3: the year is missing for Vose et al. and also in the References section.*

5/ *line 10 of page 4: interception/soil evaporation/transpiration and surface runoff/drainage*

6/ *line 21 of page 4: "system" is unclear. Please reformulate.*

7/ *lines 30-32 of page 4: The sentence is difficult to read. Please rewrite.*

8/ *lines 6-7 of page 5: SR has already been defined in page 3, line 15. The best is to combine "sometimes also referred to as plant available water holding capacity" with the text in line 15 of page 3.*

9/ *lines 18-21 of page 5: the sentences are very unclear. Please reformulate.*

10/ *lines 3-4 of page 6: words are missing in the 2nd hypothesis formulation, please check.*

3. We fully agree, and we rephrased the first sentence to be more correct.

4. We corrected this.

5. We rephrased it into "runoff components and evaporation", as we tried to lump the terms together that you refer to.

6. We changed it to "hydrological system"

C3

7. We rephrased this.

8. We changed this and placed the text at page 3, line 15.

9. We rephrased this.

10. We checked and rephrased the sentence.

Section 2

11/ *In each sub-sections, the references to Table 1 for watershed characteristics should be merged and written once in the section, just before sub-section 2.1. Then, the references at lines 12, 19-20 of page 6 and lines 1-2 of page 7 can be removed.*

11. We agree with the suggestion and changed this.

Section 3

12/ *lines 14-17 of page 9: For long-term mean variables: Et => Et. The same for Q and Ep.*

13/ *line 5 of page 10: "obtained by equation 6" => "obtained by equation 7"*

14/ *lines 7-9 of page 10: this is a strong assumption, especially under climate change where the water storage changes. This point should be more discussed when the method based on the water balance is applied.*

15/ *line 11 of page 11: "FLEX-based model" => "The FLEX-based model"*

16/ *line 1 of page 12: this process is not represented in Figure S2.*

17/ *line 9 of page 12: what are the fluxes ? Moreover, transpiration is indicated in the text but "Evaporation" is written in Figure S3. Please, check the coherency between the text and the Figure.*

18/ *line 11 of page 13: what is n ?*

C4

19/ line 4 page 14: Z95 should be Zp95

20/ line 2 page 16: "Table 2" => "Table 3"

12. We changed this.

13. We changed this.

14. We agree with this and will add a discussion on this (see also the Response to Referee 1)

15. We changed this.

16. This is not the case for the current set-up. We will remove the sentence.

17. We rephrased it to make it more consistent.

18. n is a weighing exponent. We will clarify this in the text.

19. We changed this.

20. We changed this.

Section 4

21/ lines 23-24 of page 17: this is not particularly obvious in Figure 2f.

22/ lines 20-21 of page 24: I do not see this improvement on Figure 10, maybe due to the scale of the plots.

21. We do agree that the pattern is rather variable over time, but comparing the highest peaks before deforestation with the peaks after deforestation show that the values were higher before deforestation. The same applies to the lower values. Calculating the mean autocorrelation before deforestation and after also confirm this; 0.65 before deforestation and 0.58 after deforestation.

C5

22. More specifically, we are referring to the parts of the hydrograph at the end of June until August. Please note the white space between observation and model in the case of a constant root zone storage capacity, whereas for the dynamic model they overlap.

Table/Figures

23/ Table 1:

- I would add a column for the abbreviations of each catchment, as used in figure 9 (see my comment hereafter for the whole text).

- "Precip" should be "Precipitation".

- what is "Pot." ? It is the potential evaporation?

- remove "%" from 87

24/ Table 3: the reference for Jothityangkoon et al. (2001) is missing in the References section.

25/ Figure 1: in the label of y-axis, "P" should be "PE"

23. We agree with the suggestions/corrections and will change it. "Pot" refers indeed to potential evaporation.

24. We corrected this.

25. We corrected this.

Supplementary material

26/ Table S1: please check the lmax values (Min=Max=0 !)

27/ Figure S2:

- replace "Snow" term in the figure by "S".

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- *Peff and interception are not represented in the Figure.*

- *q3 should be replaced by q2 in the figure.*

28/ *Table S2: the wilting point cannot be higher than the field capacity. Please check the max values.*

29/ *Figure S3:*

- *replace "Snow" term in the figure by "S".*

- *q3 should be replace by q2 in the figure.*

- *Q should be replace by Qf.*

- *what is dq ?*

30/ *Figure S4: the surface runoff is missing.*

26. This should be 0 – 5 mm

27. We changed this

28. These percentages should be added up (they do not represent the actual wilting point and field capacity). Thus, when wcep is 0.2, and wcfc 0.5, the wilting point is at 0.2 of the soil depth and the field capacity at 0.7 (0.2+0.5).

29. We corrected this and added the missing description of dq.

30. Correct, this model structure does not take overland flow into account.

In the whole text

"choose between "parameterization" and "parametrization"

We changed it throughout the whole manuscript to "parameterization"

"I suggest to use the abbreviations of the catchments in the text, as used in figure 9.

C7

It will facilitate the reading of the paper."

We will consider this, though this is just a matter of taste. Personally, a text with too many abbreviations may also become harder to read.

"there is a confusion all along the text when the term "evaporation" is used. The term "Evapotranspiration", which is the sum of soil evaporation, interception evaporation and transpiration, is more adequate."

We tried to be consistent throughout the manuscript and refer to evaporation when we mean all the evaporative fluxes. We actually believe that the term "evapotranspiration" should not be used and we would like to refer to Savenije (2004) for arguments to not use this term. Briefly, transpiration is a bio-physical process, with different timescales and characteristics thereby being distinct to all other evaporative fluxes, which are purely physical processes. The term "evapotranspiration" is therefore a misleading definition, adding up different kinds of processes.

References

Savenije, H. H. G.: The importance of interception and why we should delete the term evapotranspiration from our vocabulary, *Hydrological Processes*, 18, 1507-1511, 10.1002/hyp.5563, 2004.

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, doi:10.5194/hess-2016-427, 2016.

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