

## ***Interactive comment on “Does drought alter hydrological functions in forest soils? An infiltration experiment” by K. F. Gimbel et al.***

### **Anonymous Referee #3**

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The study of the effects of drought on altered functions of soil is of great interest for the HESS community. The authors combine an elaborate experimental setup at three sites in Germany with a hydrological model to study the impact of a moderate drought with a repeat time of 40 years on soil water repellence/wettability and infiltration patterns. Unfortunately the current manuscript suffers from large gaps in the explanation, making the manuscript arduous to read. It is almost imperative to first read Gimbel et al (2015) in Biogeosciences to be able to understand this manuscript. Below my concerns and comments.

Main concerns:

1. The Material and methods are not complete. While I do not expect to see a com-

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plete repetition of Gimbel et al. (2015) it should not be necessary to read that paper first before grasping the nuances in this manuscript. This is already clear from comparing Fig 1 in both manuscripts. Statements like P7693L12-13 “similar with respect to topography and soil type (Fig 2) but differ in tree species composition” do not do justice to what can be seen in Fig 2. Also, in the discussion I would like to read about possible differences in infiltration as a result of a rock fraction of 80% occurring 10 cm lower in the deciduous plot in Schwabische Alp compared to the coniferous plot, but nothing is mentioned. P7694L7 “a level equivalent to annual drought with a return period of 40 years” is vague wording, please give amounts.

2. The soil moisture model is not well described. Reader needs to read Hammel and Kennel (2001) for any specifics of the model. Input parameters are not given. There is no indication of use of or comparison with data from Gimbel et al. (2015). Values for water retention curve, soil hydraulic functions, and vegetation parameters are not given. Also, given the title of the manuscript, do the authors expect the soil hydraulic functions to change? And if so, did they accommodate for this in the model? And why did the authors use pedotransfer functions if they had such a laborious experiment and could have sampled to measure these soil hydraulic functions? In the results section the performance of the model is only described by “additional soil moisture measurements on the plots support the modelling results (not shown)”. No validation. A Nash-Sutcliffe coefficient would also be appropriate. Differences at the start of the simulation in Fig 4 between the deciduous and coniferous plots are not mentioned.

3. The manuscript is overly qualitative when it comes to describing results. For example P7699L13-15 “By comparing the pre-drought pattern and the pattern for the control plots time dependent changes as a reason for differences in pre-drought and drought treated dye pattern can be excluded”. How was the comparison done? How different are these patterns? And why are inherent spatial differences between different sampling locations within the same plot not mentioned here? The authors chose 3 samples within one treatment, is this enough? P7700L22 “showed only small differences” Can

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these be quantified? The rest of the manuscript follows a similar style in qualitative statements.

4. The discussion mainly focuses on water replenency, but the rationale for the paper, namely drought, is only mentioned at the last five lines. Considering the justification for this study (moderate drought with a 40 yr return instead of 100yr or 1000yr) it would enhance the impact of this particular study to include discussion on aspects of drought.

Comments

P7690L15: Do not use abbreviations in the abstract

P7692L9-16: The sentence is confusing by using occurrence equivalents, I suppose the authors mean drought events equivalent to those occurring maybe once every 100 to 1000 years?

P7692L26-30: The hypothesis give away the conclusions, and not referred back to at the discussion except in one place P7705L23-24, but further not proven or falsified except when the reader tries to deduce it from the results/discussion. I do not entirely agree with the phrasing of hypothesis one; it refers to soil hydraulic properties, but to me this is too broadly formulated, the wettability and infiltration of the soils will be "tested" as mentioned in line 26.

P7694L19-20: " was sprayed with a backpack nozzle for even distribution" Was even distribution achieved? From what I know of dye tracer experiments it is quite hard to achieve an even distribution. Perhaps a backpack nozzle sprayer does spray rather homogeneous, but it also depends on the persons handling the sprayer. Did the authors test evenness in a test setup beforehand?

P7695L24: What is IDL?

P7700L8: "medium to high stone content" vague wording.

P7703L24-25: that instead of which

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P7709L6: soil wettability and infiltration capacity, soil properties is too broadly stated.

P7709L12-15: Given the authors used a model it would justify discussion of the model results to substantiate this statement.

Figure 1: Needs more detail. Where are the meteorological stations located mentioned on P7697L12-13?

Figure 2: Indicate the unit for scale for soil depth, I suppose it is cm.

Figure 5: Unit of the time axis? Is it possible to make boxplots instead of mean and maximum?

Figure 6: The scaling seems suggestive here. If the plots were 80 cm wide, then the x direction should be larger than the y direction. Or is this a scaled width as Figure 7 suggests with the 0-1 axis, and why was it scaled?

Figure 7: Does 0-1 on the x-axis refer to a scaling? Why did the authors use this?

Figure 6&7: It would greatly help if the authors could remediate the comparison by eye between two plots.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 7689, 2015.

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