

## ***Interactive comment on “Quantification of dynamic soil – vegetation feedbacks following an isotopically labelled precipitation pulse” by Arndt Piayda et al.***

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*Referee comments – Author response*

**General Comments:** *The manuscript presents a nice  $H_2^{18}O$ -labelling study in a Mediterranean oak forest. Authors traced the fate of recent precipitation water in soil and understory vegetation and inferred from the respective partitions of this water for evaporation and transpiration on the use of recent precipitation for understory plants including the effects of tree shading on infiltration and water use. The study is generally well written and methods used seem generally sound. However, the discussion section at the moment is in parts confusing and gives room for improvement, as authors discuss many theories on e.g. hydraulic lift, competition for water between trees and*

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*understory, facilitation of infiltration through tree shade etc., but presently do not relate their results very well to these theories, which at the moment hampers the conclusion that they indeed disentangled all these processes. In addition, I believe that the study would benefit from a literature evaluation on the role of tree interception on infiltration and water use, a topic that has so far been disregarded in the study. The conclusions section and the abstract at the moment include deductions that either cannot be directly seen from the results, or are not well enough discussed yet. I am confident that after revision of these issues this topical field study will be acceptable for publication and appeal to the BGS readership.*

The authors are thankful for the general appreciation of the submitted manuscript and the recommendation for publication in Biogeosciences by Anonymous referee 1. The authors highly appreciate the thorough review of the manuscript and the very constructive comments. The authors have reviewed the manuscript with special focus on the discussion and conclusion section and include the mentioned literature evaluation on the role of interception.

### **Specific Comments: Abstract**

*The abstract is well written, but would benefit from a revision of the conclusions.*

The authors are thankful for the appreciation of the referee and incorporated the revised conclusions in the abstract.

*Page 1 Line 26: “unproductive water loss” odd wording*

Changed to unproductive evaporation. *Page 1 Line 27: this sentence should be removed, as no information on biomass production, carbon sequestration or nitrogen fixation is given in this paper.*

The sentence was removed.

*Page 1 Line 28: “Light to medium precipitation events” Only one precipitation event*

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*was studied with 20 mm. I would not consider this light or medium, also this sentence sounds as if you would compare between precipitation events of different magnitudes, which was not the case in this study.*

The authors agree that 20 mm of rain during one hour of watering can be considered as high precipitation intensity compared to the natural precipitation regime of the study site. We can consequentially be very certain about the fact, that naturally occurring light to medium precipitation events during drought periods have no effect on root water uptake, since the high precipitation intensity of the experiment had little impact either. We omitted the latter part of the sentence.

*Page 1 Line 28: "This forces plants..." Too general: In this context this sounds, as if plants were generally forced to compete for water with trees in this system. You observed only a short period of the year, for which this is probably true. Reformulate to a more differentiated conclusion considering results of this study.*

The statement was related to the drought period of the experiment and the onset of summer.

*Page 1 Line 33ff: a bit too thick, see comments to conclusion section*

The sentence was shortened.

### **Introduction**

*Generally nicely written, the introduction would benefit from some hypotheses on tree and open side effects on water infiltration, E and T.*

The authors are thankful for the appreciation of the referee. The authors agree that working hypotheses will enhance the structure of the manuscript and incorporated the following hypotheses in the introduction, discussion and conclusions: I. Presence of understory vegetation increases evapotranspirative water loss compared to bare soil, but foster infiltration due to shading. II. Preferential root water uptake depth of understory plants is unaffected by changes in soil water availability after rain pulses during

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drought. III. Tree shading fosters infiltration of event water and reduces evapotranspiration generating favourable soil moisture conditions for understory plants.

*Page 2 Line 7: context: the use of "thus" is not indicated, I suggest removal of this term*

The term was removed.

*Page 2 Line 17: context: the use of "for example" is not indicated, I suggest removal of this term*

The term was removed.

*Page 2 Line 20: wording: consider rewording "stable water isotopes"*

The authors consider "stable water isotopes" as a common term for D<sub>2</sub>O<sub>16</sub> and H<sub>2</sub>O<sub>18</sub> isotopes in literature (c.f. Sturm et al. An introduction to stable water isotopes in climate models: benefits of forward proxy modelling for paleoclimatology, Climate of the Past, 2010) and insist of using it consistently with existing scientific publications.

*Page 2 Line 26ff: "most data sets were limited..." Some references for limited data sets would be adequate*

We now cite the works of Kurz-Besson et al., 2006 and Asbjørnsen et al., 2008

*Page 2 Line 33: "evaporative water use" Consider rewording, water that evaporates is not really used*

The term was changed to soil evaporation.

### **Material and methods**

*With small exceptions this part seems sound and methods and calculations are described adequately. However, a section on statistical analysis should be added, as the estimation of frequently mentioned significant effects in the results and discussion section cannot be inferred from the MM part.*

The authors are thankful for the appreciation of the referee. Section 2.8 was added,

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reporting the error propagation to the results as follows: All results are reported as replicate mean with associated standard error to achieve comparability between different sample sizes. All model calculations were applied to single replica and averaged afterwards. Observed effects were considered statistically different when no overlap of standard errors was observed.

*Page 3 Line 16: Please expand on possible effects of meshes used for bare soil plots on water infiltration*

The requested information was added: meshes were installed vertically, circumventing the undisturbed soil. The sites were kept vegetation free just by regular weeding. We expect no influence of the mesh on infiltration, since the plots were installed one year before the experiment and processes like preferential flow along the mesh is unlikely.

*Page 3 Line 19: Irrigation was conducted how and over what time span?*

The requested information was added: After a base line observation, all plots were watered with 20 mm water within one hour using watering cans. The water showed an oxygen isotopic signature of -139.5‰ to trace the influence of different vegetation components on water infiltration. All plots and the surrounding soil were watered equally to avoid lateral gradients and possible differences between trenched and control plots.

*Page 3 Line 28, 30 and Page 4 Line 3: replace "in a logger" by "by a logger"*

The term was corrected.

*Page 4 Line 6: fresh material was harvested, what was the proportion of already dry material, particularly in comparison to previous study of Dubbert et al. during a non-drought year, and the different effects of plant cover on infiltration reported in the discussion. This may have also reflected on the event water use in transpiration.*

In this particular year the proportion of dry material was minimal owing to the fact that due to the additional severe dry period between January and March 2012 the biomass development in general was very low and developed only following the start of the

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drought release in March. Dead biomass from the previous season was removed from the plots at the end of summer 2011.

*Page 4 Line 8 and 11: Presenting Fig. A1 is ok to characterize biomass and species composition differences of the sites. However, it could be redundant, as this information is only presented in the two lines here and 1 line in the discussion. Biomass and species composition effects on event water use are not discussed much later. However, the tree site being dominated by grasses and the open site being dominated by forbs and potential effects on water use may be worth discussing, which would give presentation of this figure some more impact.*

We agree and now discuss this effect in the discussion section (see page 12 line 32 to page 13 line 9)

*Page 4 Line 17: Calculating gt is presented as a method, but there is no data on this in the paper. I suggest removal.*

The sentence was shortened by removing total conductance.

*Page 5 Line 5: Leaf sampling did not affect ET in the vegetation plots? How big was the reduction of leaf area through sampling? Could this have affected the temporal progress of T from event water? Please elaborate on this here.*

This is a very important issue indeed. Our leaf sampling protocol did ensure that leaf biomass sampling for isotope analysis was affecting the overall living biomass to an extent less than 5%, as we did not sample species specifically but took representative samples of the vegetation. Accordingly, we argue that the effects of destructive sampling were minimal in particular regarding the effect of event water use.

*Page 7 Line 8: depths used showed negligible root density, please add information on estimating root density in different depths to "Environmental and plant parameters" Below ground biomass was sampled with soil cores in -5, -15, -30, and -60 cm depth. Oven dried soil was sieved and root biomass was determined gravimetrically. 80% of*

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root biomass was distributed between -5 to -15 cm depth. Only 5% was distributed above -5 cm and 15% between -20 to -35 cm depth.

## Results

*This section is nicely written!*

The authors are thankful for the appreciation of the referee.

*Page 8 Line 14: Consider exchanging figure numbers 3 and 4 to achieve ascending order of figures mentioned in the text.*

The authors ordered the figures 2 and 3 (we assume that the referee was not referring to 3 and 4, since they are not mentioned in the particular position of the manuscript) from measured to modelled data in order to show results in a logical order of retrieval. We therefore keep the current ordering.

*Page 9 Line 12: correct "along with the lines of evaporation"*

The term was corrected.

*Page 9 Line 15: "Root water uptake allocation" sounds odd, Fig. 7 shows root water uptake from different depths over time but no allocation. Consider rephrasing.*

The term was rephrased in the entire manuscript to "preferential root water uptake depth".

## Discussion

*The discussion could still be improved by further increasing the implementation of own results in the theories discussed and enhancing the clarity of some statements made.*

We appreciate the constructive suggestions and revised the discussion section in accordance with the suggestions.

*Page 9 Line 28: remove "was"*

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The term was removed.

*Page 9 Line 29: add comma after "Mediterranean soils"*

The sentence was corrected.

*Page 9 Line 31: remove "significantly"*

The sentence was corrected.

*Page 10 Line 3: add "This is" before "in contrast"*

The sentence was corrected.

*Page 10 Line 4ff: Dubbert et al. 2014 "reported beneficial effects of vegetation cover on soil water infiltration year-round" Fig. 2 in this paper shows indeed vegetation plots showing mostly higher infiltration than soil plots. However, it would be good to compare data specifically for the transition period between the wet and the dry year here. From Fig. 2 by Dubbert et al. 2014 one can infer that vegetation enhanced infiltration as compared to bare soil, particularly with large water pulses. The only data point comparable to your data shows a rain pulse of 10mm as compared to the 20mm you gave, with only little benefit of vegetation cover. Does that enhance or reduce the significance of your reversed results? In addition, how did you apply water? On the spot irrigation can hardly be expected to yield same infiltration results as a rainfall event over a certain amount of time? This may be good to discuss here.*

The precipitation data displayed in Dubbert et al. 2014 (Fig. 2) represents daily sums of precipitation. Even though the daily sum of precipitation at the comparable data point end of May shows 10 mm of rain, the precipitation intensity could have been very different to the experiment conducted in this study. A low intensity of, e.g. 1mm per hour, would change soil moisture conditions and air moisture conditions in the boundary layer in the very beginning, fostering different processes during infiltration for the last 9 hours of the event. The results shown here are only valid for short term rain events with high intensities and thus not contradictory to the results of Dubbert et al.

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2014. However, the authors agree with the referee that the topic of intensities need to be discussed. The authors changed the respective discussion section to: This is in contrast to previous studies, which reported beneficial effects of plant cover on daily sum of infiltration during the same period at the onset of drought in 2011 (Dubbart et al., 2014c). However, (Dubbart et al., 2014c) only observed precipitation events of light intensity during the period of interest. The present study reports on high intensity precipitation events. This unexpected turn in effect direction with increasing precipitation intensity, which depends on plant cover and atmospheric evapotranspirative demand, potentially plays a strong role for the water balance of the ecosystem in the course of ongoing climate change scenarios since the occurrence of extreme precipitation events is expected to increase (IPCC, 2013).

*Page 10 Line 13: "anyway" reword*

The sentence was corrected.

*Page 10 Line 14: add "by" after "unaffected"*

The sentence was corrected.

*Page 10 Line 16: "effects of soil hydraulic properties beneath tree crowns" In what way were properties affected? Did that also apply to your study? Please elaborate further on the potential importance of this.*

The respective discussion section was changed to: Previous studies reported similar, positive feedbacks of tree cover for the hydrological cycle in savannah-type ecosystems related to shading effects (Eldridge and Freudenberger, 2005). Effects of altered soil hydraulic properties beneath tree crowns, like the amount of preferential flow fostering infiltration (Bargués Tobella et al., 2014) could not be identified in this study.

*Page 10 Line 17: remove brackets before reference to Bhark and Small, 2003*

The sentence was corrected.

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*Page 10 Line 19ff: The positive effects of tree crown cover on infiltration may be lost by interception, as the authors state. Could you try to infer the role of interception for cork oak trees from literature values to better describe the significance of the climatic advantages in the shade for infiltration?*

The amount of interception loss by the tree canopy and stem bark of cork-oaks (or trees in general) is highly variable, depending on meteorological variables like precipitation intensity, wind speed, relative air moisture and stand properties like tree density, branch geometry, leaf angle and shape. The authors included results from David et al. 2006 in the discussion, which were derived in an ecosystem with comparable stand and climatic conditions in order to give the reader a feeling for the magnitudes of the interception loss and infiltration enhancement. However, directly relating tree interception loss results from other studies to the infiltration effect results of this study is highly prone to misleading conclusions due to different boundary conditions and settings of the experiments. The authors therefore desist from direct deductions by comparisons with previous studies.

*Page 10 Line 24: consider deleting "close to trees"*

The term was deleted.

*Page 10 Line 28: correct "overall"*

The sentence was corrected.

*Page 10 Line 29: reword "shortcoming", odd in this context*

The term was replaced by negative effect.

*Page 11 Line 10: odd "productive water", consider rewording*

The term was replaced by transpiration water.

*Page 11 Line 11: rephrase to "... from the longer time response lag of T., on the other hand from only little event water reaching deeper soil layers, where..."*

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The sentence was reformulated.

*Page 11 Line 12: remove “prior to the precipitation pulse”*

The term was removed.

*Page 11 Line 13: “Event water use of the understory vegetation was overall low” Again the question, of how much living biomass was there? Is it possible that understory plants were on the verge of senescence and therefore did not use the water or readjust water uptake depths?*

At both sites the understory vegetation was indeed already past the peak of biomass development. There were, however, differences between the two regarding the productivity evolving during the experimental period. At the open site, the understory still showed a significant net uptake of carbon throughout the entire experiment, while decreasing NEE and even a net release of carbon at the final day of the experiment could be observed at the tree site. Since we agree, that this information is rather important for the interpretation of the site specific difference and also explains the overall differences in ET and T throughout the experiment rather well, we added an additional graph A2, informing on the development of NEE over the experimental course. See also page 12 line 32 to page 13 line 9.

*Page 11 Line 18: Competition with tree roots. This can this be inferred from delta 18O signals of soil water being more depleted in the tree site but this depletion not being visible in transpiration? Higher infiltration at the tree site must thus have been of no use for understory plants, because of competition with trees. Could you elaborate on this more?*

It is indeed true that leaf and transpirative isotopic signatures did not show a such significant depletion at the tree site compared to the open site as could be observed for the soil isotopic values. This is mostly due to the lesser general uptake of water (i.e. lower T rate) below the trees compared to the open site. Whether this is due to

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competition with trees is not provable with the current data set, mostly because we are missing isotopic data on tree root water uptake (tree xylem). Moreover, the current approach of spacially explicit labelling of the discreet plots did not allow for estimation of tree reaction to the irrigation pulse.

What can be clearly seen is, that the vegetation below the trees was already at the verge of senescence (see above). Previous data by Dubbert et al. (2014) however suggests, that the phenological shift and earlier senescence might very well be strongly related to tree understory competition.

*Page 11 Line 22ff: “Hydraulic lift” This point is contrary to the previously discussed competition for water. If water from hydraulic lift was up in the layer of understory roots you would expect 1) a dilution of the event water signature, and 2) a higher soil moisture. You do not find any of this. Thus, I think from your data you can infer that hydraulic lift was not a major factor here. Roots preferentially taking up water in this depths may be due to hydraulic lift, but you find the same in the open site, so I would take out this argumentation here.*

The authors agree with the opinion of the referee and removed this discussion section.

*Page 12 Line 2: context: the use of “therefore” is not indicated, I suggest removal of this term*

The term was removed.

*Page 12 Line 8: remove “the” before “type”*

The sentence was corrected.

## **Conclusions**

*The conclusions at the moment seem overstated considering the results presented, and should be rewritten. The study itself is nice enough and does not need this thick laid conclusion.*

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The authors incorporated the recommendations of the referee into the conclusions section to a large degree in order to make it more adequate for results presented in the study.

*Page 12 Line 13: I do not really agree that your study disentangled and quantified tree and understory interactions. As such you compared sites with and without trees, but do not go into much depth regarding tree understory interactions. For this statement to stand this topic should be more thoroughly discussed on base of the results presented. Either adapt the discussion to really try and disentangle the role of hydraulic lift vs. competition vs. enhanced interception, or be more modest here.*

The authors reformulated the sentence: In this study, the various interactions between understory vegetation and trees of a Mediterranean cork – oak woodland affecting the ecosystem water flows could be quantified.

*Page 12 Line 18: Consider removing “or just bare soil”*

The term was removed.

*Page 12 Line 19: The sentence “Thus, the amount of unproductive water loss...” is a large overstatement and should be removed. This study did not show any data on nitrogen fixation, carbon sequestration or biomass production, for this statement to hold true.*

The sentence was modified to: Thus, the amount of unproductive evaporation is largely reduced, in favour of transpiration.

*Page 12 Line 21: I would not consider a 20mm precipitation pulse as light or medium.*

The sentence was changed accordingly.

*Page 12 Line 22ff.: “Therefore, these understory plants were forced into competition....However, the understory plants could profit from tree root induced soil water redistribution.” Both statements do not hold true, the first point I can agree upon, but it*

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*should be included in more detail in the discussion with better implementation of own results. The second statement, I don't believe that this was shown!*

The statement was removed.

*Page 12 Line 23: “Cork oak trees foster infiltration...” I would not make this statement without considering interception of rainfall.*

The sentence states that the study could show a strong increase of infiltration due to favourable climatic conditions under tree crowns. That is true independent of a possible negative effect of interception losses on throughfall. However, the authors agree with the referee that the effect of throughfall interception was not investigated in this study and plays a major role in the overall ecosystem water balance. Therefore the authors keep this important statement, but reworded the sentence by removing the emphasis of the infiltration part in the revised version.

*Page 12 Line 26: that is too laid on thick, given the study's outcome. I would not use this sentence.*

The sentence was removed.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/bg-2016-451/bg-2016-451-AC1-supplement.pdf>

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-451, 2016.

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