

Interactive comment on “The effect of drought and interspecific interactions on the depth of water uptake in deep- and shallow-rooting grassland species as determined by $\delta^{18}\text{O}$ natural abundance” by N. J. Hoekstra et al.

J. Nippert (Referee)

nippert@ksu.edu

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Broad comments:

In this manuscript, the authors present data from an experiment manipulating summer rainfall to assess changes in source-water uptake of 4 species (2 shallow-, 2 deep-rooted) in monoculture and in mixture. The authors hypothesized that during drought, niche overlap among species would shrink, with deep-rooted species increasing reliance on deep sources, while shallow-rooted species increasing reliance on shallow sources. Their results varied from original predictions, as 3 species (1 deep and both shallow-rooted) shifted to deeper sources following drought, while 1 deep-rooted shifted to the shallowest soil layer during drought (in monoculture). Interestingly, shifts in source water use during drought were not related to ‘drought resistance’ [assessed by changes in aboveground productivity between drought and control].

In general, this is a well-written paper on a topic appropriate for Biogeosciences and for a broad audience. Many of us, myself included, have been using natural abundance stable isotopes to quantify changes in source water partitioning in grassland ecosystems for quite some time. I enjoyed the comparison of the two methods to assess ^{18}O data. I found it useful that for many applications, the direct inference approach was justifiable.

Response: We would like to thank the reviewer for his positive evaluation of the manuscript. We have formulated responses to all the comments below, and have made changes to the manuscript where required.

It is unclear how these results show ‘niche complementarity’ (line 16, and the discussion). Later in the same sentence, the authors note that this response contributed to ‘the diversity effect in mixtures’. What does this mean?

Response: see response below.

On page 4155, niche complementarity is posited to suggest that a shallow-rooted and deep-rooted species could maximize resource uptake. Couldn’t the same thing happen with a single species with roots throughout the profile?

Response: In functional biodiversity research belowground niche complementarity is an important and frequently invoked potential mechanism / concept to explain the better performance of mixtures compared to monocultures. Of course, if one single species would have high root density throughout the whole soil profile this would be ideal for resource uptake. However, shallow rooted species generally produce a very high root length density in the top soil layers (cm root / cm³ soil) but have a limited rooting depth. On the other hand deep rooted species have tap roots with a low root length density in the top soil layer but with the feature to get access to deep soil layers. The concept is that combining these different strengths by combining the respective species results in higher resource uptake in the mixed plant community. We have now re-emphasised the differences between these two functional types in our introduction.

In addition, it has been shown that for some grassland species, conductive root tissue declines with depth – and thus, the functional uptake of water from deeper soil layers is low regardless of the presence of deep roots. Thus, an assessment of ‘complementarity’ in terms of maximum resource

extraction would require an estimate of functional conductivity and specific root length by depth. Neither of these metrics are measured here. Please correct me if you disagree.

Response: In this paper we have already indicated that the presence of roots in itself is not a good indicator for root activity or functional uptake. By using the isotope method, we get a better idea of the relative depth of water uptake, and the different patterns between the species that are expected to be complementary. However, the weakness of the isotope method is the lack of quantitative uptake. Therefore, even though there may be differences in the relative depth of uptake between the species, this is no specific evidence that complementarity was the specific mechanism resulting in increased the total water uptake. Throughout the document we have taken care not to refer to niche complementarity, for which we have no direct proof. However, we are able to test important components of the concept (e.g. do species differ in their proportional uptake from deep layers (our hypothesis 2)? Does proportional uptake shift with growth conditions [drought (our hypothesis 1) and plant community (our hypothesis 3)]?)

Only in the final part of the discussion, we **suggest** that the observed diversity effect could have been related to vertical niche complementarity in relation to depth of water uptake based on the differential proportional contribution to water uptake of the different species (in addition to other factors including facilitation). In the discussion we have now re-emphasised that there is no direct evidence for this, and this section now reads as follows.

“Our results suggest that differences in the depth of water uptake between species may have resulted from vertical niche complementarity in the depth of water uptake between deep-and shallow-rooting species, which may have contributed to this over-yielding. However, due to the lack of data on quantitative water uptake from different soil depths, we cannot provide direct evidence for this. Additionally, other factors, such as vertical soil niche complementarity for nutrients, or interactions between legumes and non-legumes, soil-biotic factors or a combination of factors may have also contributed to the diversity effect.”

Additionally, we have removed the sections referring to this from the conclusion and abstract.

It was difficult for me to extend inference on the role of rooting depth (shallow and deep) broadly, since this experiment used 2 species per category. I would suggest minimizing the inference based on this functional classification (especially since the species compared had varied responses within this classification).

Response:

The grouping of the species into deep- and shallow-rooting species was part of the experimental design, and as such at the basis of our a priori hypotheses and we therefore cannot ignore it. There is substantial evidence to justify the selection of species based on their root morphology (we have added some references to the description of species selection in section 1.2). This experimental design was chosen to be able to test whether the species behave as expected from their morphology (deep / shallow rooted). Based on this test and the results obtained we do recognise that the effective rooting depth of species may very much depend on the conditions of the experiment. So indeed a deep-rooting species (*T pratense*) may under certain conditions take up the bulk of its water from a very shallow depth. We have added a paragraph at the end of section 3.2 discussing the limitation of grouping species according to rooting depth.

“This research shows that classification of species according to rooting depth may be of limited value, as the “effective” rooting depth depends on the specific conditions. Similarly, Durand et al (1997) demonstrated that *L perenne* could extract water from very similar depth as *F arundinacea*, a renowned deep rooted species.”

It isn't clear why this experiment was performed at two different sites. There is no comparison of a 'site effect' or a comparison of environments on a drought*rooting depth effect? Since most of the usable plant data from Tanikon was lost, I don't see the added utility of having this Tanikon in this manuscript.

Response:

- The experiment was carried out at two different sites because this is a much more severe test of the concepts, as it refers to two different sets of conditions in terms of soil, climate of the year, and establishment after sowing. Two sites deliver two independent datasets, whereas measuring two years on the same site / the same plots would mean repeated measurements (which are not independent from each other).

- By moving site, ideally we would have two full datasets for a year, which would have allowed site comparison. Because of the loss of data from Tännikon, this site comparison is not an option. However, we do think it is important to keep the Tännikon data in the paper, as it shows the robustness of the results found in Reckenholz. Especially, the unexpected but interesting response of *T pratense* (see below) was similar in both experiments.

One of the most interesting aspects of this paper (to me), is the response of *T. pratense*. This species had very little reduction in biomass, shifted its water uptake to surface layers during drought, and constituted the majority of biomass in mixtures. The Discussion section mentions the *T. pratense* results, but I would like to see a bit of extra discussion of the attributes of this species that set it apart from the other 3 compared. Does this species have unique vascular morphology? Isohydic or anisohydic stomatal control? What makes this species so different from the rest? What are the attributes that might lead to the source-water plasticity measured here?

Response:

Changes in pre-dawn leaf water potential (unpublished data) in response to drought were similar in *T pratense* compared to *T repens* so there is nothing there to suggest a difference in stomatal control. We were unable to find any explanation for the different behaviour of *T pratense* from literature. We have now included these observations in the discussion (section 3.1): "Of all the species, the dry matter yield of *T pratense* was least affected by the drought treatment (Table 2). Changes in pre-dawn leaf water potential in response to drought were similar for *T pratense* and *T repens* and provided no evidence for differences in stomatal control (unpublished data).

Specific comments:

1. Were the 'stem bases' (page 4158 – line 17) photosynthetic / green? For the water isotope technique to work using herbaceous plants, you have to use crown / non-photosynthetic tissue.

Response: the harvested material consisted partly of root crown and partly of stem bases. The stems of *C intybus* were white, but the stems of *T pratense* and *T repens* were green. However, Barnard et al. (2006) showed that for *T pratense*, there was no significant difference in the d18O isotopic signal extracted from the root crown and the stem. Therefore, we do not believe that the inclusion of some stem material in the sample for water extraction affected the isotopic signal.

2. Line 1, page 4166 states that uptake from deeper, wetter soils increased during drought in monocultures for 3 species. But for Fig 2g-i, it appears those bars overlap considerably. Are these statistically significant? Fig. 2b-d doesn't appear to show a shift in source between control-drought, especially for *T. repens* and *C. intybus*.

Response: The significances of the contrasts between drought and control for the individual species are reported in the results section (2.4.2), and show that the difference is significant for *L perenne*, for *T repens* only when grown in monoculture and for *C intybus* there is only a tendency. We have

now re-emphasised the difference between the drought response in monocultures and mixtures in discussion section 3.1.

3. In figure 2, I presume that the shaded bars are 'control' and open bars are 'drought'? You need a legend.

Response: This information has now been added to the legend.

4. The responses in Fig. 1c,d are hard for me to interpret. How/why did the $d^{18}O$ values become smaller during drought? Soil drying and evaporative enrichment should produce higher values indicative of drier soils, at least in the upper soil layers (0-10cm). Can you posit a mechanism for these atypical soil isotope results? Were the rainfall inputs in the 'control' of a heavier signature? You need rainfall isotopic data.

Response: This can be attributed to the heavier signature of rainfall inputs in the control plots. We now have included the monthly rainfall $d^{18}O$ isotopic composition which is available from the Swiss National Network for the Observation of Isotopes in the Water Cycle (ISOT)(Schürch et al. 2003). These show that the $d^{18}O$ of rainwater was less negative during the drought period compared to the preceding months (difference of 2.1 and 2.9 during 2011 and 2012, respectively). These data have now been included in the supplementary material (Fig. D1) and in the discussion.

5. Line 14, page 4167 – Nippert and Knapp 2007b has detailed soil moisture info throughout the profile – check Figure 1.

Response: We have now changed the text to reflect this.

6. Line 27, page 4167 – the work by Nippert and Knapp and by Asbjornsen et al. was conducted in temperate mesic grasslands. Not in 'arid systems' as this text states.

Response: we have removed the reference to 'arid systems'.

7. Line 23, page 4168 – Please clarify your intention by the statement 'makes sense'. Are you suggesting that uptake from shallower soil layers (compared to deeper) would be beneficial to the plant since there would be a shorter path length for transport, and therefore a reduced gradient in water potential required for movement? If so, this has nothing to do with "convenience" (line 24).

Response: we have rephrased this sentence to: "uptake from shallower soil layers would be beneficial to the plant since there would be a shorter path length for transport, and therefore a reduced gradient in water potential required for movement."

8. Line 26, page 4170 – How does nutrient availability affect drought resistance? Responses during drought might be impacted by nutrient availability, but the term 'resistance' implies some morphological or physiological attribute of the individual. I think this statement needs to be rephrased (or at least further elaboration).

Response: 'resistance to drought' has been replaced by 'impact of drought on herbage dry matter yield'.

9. I may have missed something obvious, but in Fig. 5 how was the category 'mixed' developed? It's unclear to me what this metric refers to.

Response: "Mixed" refers to species-pairs consisting of a shallow- and deep-rooting species. This term was first introduced section 1.5, and we have added an explanation both here and in the caption of Fig. 5

On behalf of all authors,

Nyncke Hoekstra

References

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J.-L. Durand (Referee)

jean-louis.durand@lusignan.inra.fr

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Main remarks. The paper deals with an essential topic, which refers to the ability of multispecific crops, and in this case, grasslands to sustain more severe droughts compared to pure stands. The choice of species is relevant for all Europe and more. The issue is also about methodology for studying resource sharing, and especially water, between species of a community. The choice of natural isotopic abundance of ^{18}O is relevant and the experimental design is sound, using a comparison of control plots (i.e. rainfed) and plots protected by permanent rain out shelters. The duration of the drought studied is long enough to mimic a significant water deficit. The measurements made on biomass produced during the drought period itself are relevant for at least one important issue in drought resistance studies. The replicates number each year and the two site-year experiment provide a significant number of data for sound conclusions. The text is very clear, figures are mostly clear too. All of them if not more are necessary in the main text. Some conclusions are new and important.

I have however two serious concerns with the data itself on the one hand and with the treatment of the data on the other hand. Given the importance of the topic, the quality of the data and the novelty of the science, I really hope that the authors have the resources to work on these points and I therefore suggest that the paper should go through major modifications.

Response:

We would like to thank the reviewer for the positive feedback on the manuscript. We have now responded to all the general and specific comments below, and have made changes to the manuscript as indicated.

Firstable, the soil water isotopic composition is not clear. Fig B1 should be included as figure in main paper, not as appendix. The difference in soil profiles between the rain fed and rain out shelter is puzzling. No clear explanation is given for a difference as large as 2 o/oo at Tänikon (Fig B1). If the regional waters are close to -8 o/oo, how is it possible that we have -11 o/oo at the end of the drought period in Tänikon ? Are there any measurements of rainfall isotopic signature ? This would be a very useful measurement here. Futhermore, the gradient results from the soil surface evaporation and from the net subsequent diffusion of heavy isotopes downward. Soil evaporation could have been higher under the rainout shelters due to higher temperature but indeed, the first and main impact of such superstructure is a reduction of incident radiation. As a consequence and given the small difference in air temperature, ET_c could have been likely 10- 20 % less under rain out shelters. Were there any estimate of such reduction in incident radiation and at least, could the energy interception by the shelter be measured? This is critical to discuss several aspects of the responses (biomass production, water consumption, depth of water extraction, which depends on transpiration (see Boujamlaoui et al 2005)

Finally, the soil water profiles clearly indicate a quite important water consumption below 40 cm which is not much addressed in the paper.

Response:

- The difference in soil water d^{18}O composition can clearly be seen in the current Fig. 1, and therefore we do not think including Fig. B1 in the main paper would be beneficial.
- We now have included the monthly rainfall d^{18}O isotopic composition which is available from the Swiss National Network for the Observation of Isotopes in the Water Cycle (ISOT) (Schürch et al., 2003). These show that the d^{18}O of rainwater was less negative during the drought period compared to the preceding months (difference of 2.1 and 2.9 during 2011 and 2012,

respectively). Rainfall isotopic composition ranged from -15 (Feb) to -6 (August). These data have now been included in the supplementary material and in the discussion.

- We did measure the photosynthetically active radiation (PAR) and have now included the background readings (taken above the crop canopy) from under the drought shelters compared to the control plots in Table 1. This indeed shows that the incoming PAR underneath the shelters was 11 to 28% lower compared to control plots in 2011 and 2012, respectively. This would have resulted in a decrease in evapo-transpiration, which would contribute to the more negative $d^{18}O$ signal under drought compared to control conditions. We have now addressed this in the discussion.
- We have re-run the IsoSource model with estimated values for the $d^{18}O$ composition of water from 40-50 cm soil depth to get an idea of the effect of including deeper soil layers, and have included this in our methodology discussion (for more detail see below).

Secondly, I strongly recommend to drop all reference to the first direct inference of water extraction from the comparison between the soil delta gradient and the so called “xylem water” signature. (Incidentally, only a small fraction of the water extracted from the plant samples is truly xylem water.) It has been shown that such use of comparison is wrong (Durand et al. 2007). Furthermore, it is useless here and therefore unnecessarily weakens the paper a lot. If the question was about the ranking between treatments and species, then there was no need to infer any actual depth from the delta ^{18}O data. The second estimate is clearly much more rigorous, providing an estimate of the average depth. By the way, why was direct comparison of the average depth of water extraction using the Philipps and Gregg methodology with direct inference not made? This would be much more convincing than a simple correlation. But again, even with the IsoSource computation, such statistical approach is not real evidence for actual depth. IsoSource is certainly an important step forward and provides very interesting insights for interpreting the delta and I find that the use made of that software here is really relevant.

Response:

We consider that the comparison of the two methods is a valuable addition to the paper, 1) because it shows there is a strong connection between the two methods, and 2) because it emphasises the value of the “new” approach using the IsoSource method. Here, we also refer to the review of J. Nippert, who actually expressed his appreciation of presenting and comparing both methods.

What is worrying is that for the red clover however, there seems to be a contradiction between the conclusions obtained comparing the soil water profiles (drought and control) and the comparisons using the PCWU0-10. This raises the question of the accuracy of the methodology used when there are more sources than markers in this case at least.

Response:

Comparison of the direct inference method and the IsoSource method shows that the trends in response to drought are similar, and therefore there is no clear evidence that there was a problem in relation to the number of sources relative to markers resulting in “wrong” estimates. However, the IsoSource model did result in a rather large range in the 1st and 99th percentile of the frequency distribution (See Fig. 2, related to the relatively low gradient in the soil water isotope composition in the top 20 cm) indicating that some care is needed with the interpretation of the results. The fact that the same effect occurred at both sites, strengthens the results.

Additionally, some more detailed remarks on the manuscript.

1. The species are given relative depth of rooting and water extraction a priori. However, it is very much related to the conditions of the experiments. For instance, Durand et al (1997, 2009) demonstrated that *L. perenne* could extract water from very similar depth as *F. arundinacea*, a well-known potentially deep rooted species. The qualification of shallow rooted for *L. Perenne* is

therefore questionable (a mighty reason for doing this experiment indeed). Ascribing any depth of water extraction in the introduction or as a reputation should be made more reluctantly.

Response:

The grouping of the species into deep- and shallow-rooting species was part of the experimental design, and informed our *a priori* hypotheses and we therefore cannot ignore it. There is substantial evidence to justify the selection of species based on their root morphology (I have added some references to the description of species selection in section 1.2). It is an important part of this study to test whether this morphological distinction between deep- and shallow-rooting species can be translated in terms of depth of root activity (hypothesis 2), and as such we need this a-priori classification. Indeed, we show in our own study that the effective rooting depth of species may very much depend on the conditions of the experiment. So a deep-rooting species (*T pratense*) may under certain conditions take up the bulk of its water from a very shallow depth. Similarly, as the reviewer indicates above, shallow-rooting species such as *L perenne* may under certain conditions take up water from deep soil layers. We have added a paragraph at the end of section 3.2 discussing the limitation of grouping species according to rooting depth.

“This research shows that classification of species according to rooting depth may be of limited value, as the “effective” rooting depth depends on the specific conditions. Similarly, Durand et al (1997) demonstrated that *L perenne* could extract water from very similar depth as *F arundinacea*, a renowned deep rooted species.”

2. That local water extraction depends on local root density is very well established under well watered conditions (both in trees and crops) and is difficult to be introduced as a question.

Response:

We assume that this remark relates to the last sentence of section 3.2 and have deleted this sentence from the discussion.

3. Similarly, that water is extracted from deeper & wetter horizons when water is scarce near the surface is all but surprising. The water potential distribution in the plant–soil system inevitably leads to that and this has been documented (see literature cited like Sainclair , Garwood, but more recently modeled by Jarrige , Doussan or measured under various conditions by Gonzalez Dugo et al: : :)

Response: Even though this may seem “inevitable”, there is quite a large body of evidence indicating that for specific species or under specific conditions, there is no shift in water uptake to deeper soil layers (Prechsl, 2013; Asbjornsen et al., 2008; Nippert and Knapp, 2007a; Nippert and Knapp, 2007b), and in our experiment we found a similar result for *T pratense* (see also section 3.1 in the paper). Also, there is not always a gradient in water availability in the rooted soil profile (see paragraph 3.1, and e.g. Kulmatiski and Beard (2013)). Therefore we consider that this hypothesis is not redundant.

4. The radiation below the shelters was not measured, which is an issue. The irradiative energy balance is likely more important for potential evapotranspiration_ than the air temperature or humidity in that situation. How much could have been ET_ been modified in these conditions ?

Response: As indicated above, we measured the incoming photosynthetically active radiation under the shelters and control plots, and have now included this information in Table 1). The effect of the shelter on ET is likely to have reduced the impact of the drought that could have been expected from rainfall exclusion alone, and we have now added this to the discussion (section 3.6). However, our soil moisture contents were much lower under drought compared to control conditions, resulting in a significant biomass reduction.

5. The apoplastic water in tiller’s base may well not be more than 40 %, out of which, xylem water is even much less.

Response: Barnard et al. (2006) found no significant difference in the $\delta^{18}\text{O}$ isotopic signal extracted from the root crown and the stem base of grassland species (including *T pratense* and *L perenne*). Therefore, we consider that using stem bases (+ root crowns) rather than root crowns alone, is unlikely to have had an impact on the results.

6. The outer sheath of grass tillers may transpire and therefore enrich the tissues water in heavy isotopes of water. This is not so much related to photosynthesis.

Response: We have rephrased this as: For *L perenne*, the outer sheath, which may be subject to transpiration and therefore have an altered $\delta^{18}\text{O}$ signal (Durand et al., 2007)

7. The direct inference of depth of water extraction using the soil profile delta 18O is flawed and misleading. The first paragraph of data analysis in material and methods and all paragraphs later on referring to it should be dropped (with absolutely no harm to the strength of the paper in the contrary!).

Response: We are well aware of the caveats relating to the use of the direct inference method (as discussed in section 3.6). However, we feel that the inclusion of the data in this paper is valuable for reasons outlined above.

8. The use of water from deeper than 40 cm is not discussed. Could that have had some impact on computations of the PCWU0-10 ? It should be discussed somewhere anyway because we have no data on the delta 18O below 40cm.

Response:

We reran the IsoSource model with an added 40-50 cm soil interval, estimated as $30-40 \text{ cm} + (30-40 \text{ cm} - 20-30 \text{ cm}) / 2$, assuming that the decline in $\delta^{18}\text{O}$ with increasing soil depth would start to “level out” at this depth (see also Fig 2). The corresponding estimates for PCWU0-10 were highly correlated to the original estimates ($r^2 = 0.99$) and were marginally higher (0.51 instead of 0.49 on average) particularly at low levels of PCWU. As a result this had no material effect on the observed trends in response to drought and diversity.

We have added the following paragraph in the discussion:

“For practical reasons, the $\delta^{18}\text{O}$ sampling depth was limited to 40 cm soil depth. However, it is not unlikely that water uptake from below this depth occurred (Skinner, 2008;Pirhofer-Walzl et al., 2013;Garwood and Sinclair, 1979). This would not have affected the mean inferred depth of water uptake, as these values were all well above 40 cm (Fig 2a-e). In order to get an idea of the potential effect of limiting the soil sampling depth to 40 cm on the output of the IsoSource model, we re-ran the model with estimated $\delta^{18}\text{O}$ values for the 40-50 cm soil depth interval. We assumed that the decline in $\delta^{18}\text{O}$ with increasing soil depth would start to “level out” at this depth (see also Fig. 2), and estimated the $\delta^{18}\text{O}$ value for the 40-50 cm soil depth interval as the $\delta^{18}\text{O}$ value for $30-40 \text{ cm} + (30-40 \text{ cm} - 20-30 \text{ cm}) / 2$. The resulting estimates for PCWU₀₋₁₀ were highly correlated to the original estimates ($r^2 = 0.99$) and were marginally higher (0.51 instead of 0.49), as there was now more support for the relative reliance on shallow soil depths. As a result, adding an extra (estimated) depth to the IsoSource model input had no effect on the observed trends in response to drought and diversity.”

9. The differences observed between the delta 18O profiles in the deep horizons at the same place are difficult to understand. What could have caused this ? Were the soil sampling conditions similar?

Response: Soils were sampled in two consecutive days under similar sampling conditions and we do not have any other explanation for this.

10. P12 L 337: no agreement between the two estimates is presented but a correlation.

Response: “agreement” was replaced by “correlation”

11. Why no estimate of the average depth of water extraction using IsoSource is shown?

Response: The output of the IsoSource model is a proportional contribution of the different source (soil depth intervals) to plant water uptake, and to our knowledge does not allow calculation of the average depth of water uptake.

12. Drought resistance should be defined here. In this case, drought resistance is defined as production during dry condition relatively to control conditions. The control is always an issue in drought response analysis. All that is needed here is a clear definition.

Response: We have now included this definition in the abstract and the main text

We wish to thank the reviewer for their detailed reading of the manuscript, and for helping to improve the manuscript.

On behalf of all authors,

Nyncke Hoekstra

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M. Zeppel

melanie.zeppel@mq.edu.au

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This paper provides very useful and insightful results into the impact of drought on water uptake, from deep- and shallow-rooting species.

Response: We would like to thank you for your interest in the paper and positive feedback and suggestions. Please find below our response to your comments and corresponding changes to the manuscript:

It would be useful if the authors provide details based on the latest IPCC projections of which regions will be likely to experience more drought, more severe drought. Although it is commonly argued that drought is occurring in many locations, it is clear that rainfall will increase in some regions, and at some times of year.

Response:

Climate models predict that the climate in Central Europe will be characterised by increasing temperatures and changing precipitation patterns and more frequent occurrence of drought. Projections for Switzerland indicate that in 2070, the mean decrease of summer precipitation ranges from 20 to 40 % compared to 1990 levels, resulting in severe drought stress.

However, the aim of this study was not to try and mimic future climate change conditions for this particular location, but instead was designed as a model system to investigate the potential effect of moderate drought stress.

We have added the following lines and references to our introduction:

“ Climate models predict that the climate in Central Europe will be characterised by increasing temperatures, reduced summer precipitation and increased frequency of extreme events (Christensen, 2003;Schär, 2004). These discrete events include droughts, heat-waves and storms, and can have a large impact on a variety of ecosystem functions and services (Lehner et al., 2006).”

It would be curious to consider whether this effect of altered depth in water uptake also occurs in trees, which have deeper roots than grasses. Zeppel et al 2008 report this:

"(b) water uptake must have occurred from depths of up to 3 m; (c) sap flow was independent of the water content of the top 80 cm of the soil profile; "

Zeppel, M.J.B. et al., 2008. An analysis of the sensitivity of sap flux to soil and plant variables assessed for an Australian woodland using a soil-plant-atmosphere model.

Functional Plant Biology, 35(6): 509-520

Response: We are aware of a large body of research depth of water uptake in trees and shrubs and other non-grassland species. Responses in such systems are likely to diverge from the responses we found. For example, as stated above, trees have been shown to take up water from depths of up to 3m, and this will have a large effect on the response to drought and plant diversity. Therefore, in order to keep the discussion focussed, we decided to only discuss our results in relation to (comparable) grassland systems (which includes a number of studies including trees and shrubs, e.g. Kulmatisky and Beard, 2013)).

The paper would also benefit if the authors focus more on the soil water content of each layer, and highlight more clearly their soil water content results.

Response:

We have now extended the results section in a way that addresses this point, as follows

“Under control conditions, the soil moisture content ranged from 0.33 and 0.20 g water per g dry soil in the 0-10 cm soil depth interval to 0.28 and 0.18 g water per g dry soil in the 30-40 cm soil depth interval for Tännikon 2011 and Reckenholz 2012, respectively. In both experiments, soil moisture content was significantly lower under drought compared to control conditions ($p <$

0.001). The difference in soil moisture content between control and drought was on average 0.15 and 0.11 g water per g dry soil in the 0-10 cm soil depth interval, but was only 0.03 and 0.06 g water per g dry soil in the 30-40 cm soil depth interval for Tännikon 2011 and Reckenholz 2012, respectively, resulting in a significant water supply × depth interaction ($p < 0.001$, Fig. 1a, b and Table A1).”

Also, given future climates are likely to experience more extreme precipitation, it would be useful if the authors consider the framework presented by Knapp et al. 2008 - where changed precipitation leads to water logging in some instances, and drought in others, depending on rooting depth of the species.

See Knapp, A.K. et al., 2008. Consequences of more extreme precipitation regimes for terrestrial ecosystems. *BioScience*, 58(9): 811-821.

Response:

As indicated above, our study is based on a model system simulating drought, and in order to keep the paper focussed and within word limits, we do not discuss on the wider implications of climate change for different ecosystems and climatic regions (an important topic that is discussed in considerable detail by other authors).

On behalf of all authors,

Nyncke Hoekstra