Reply to reviewer 1 Matthias Beyer

In what follows, we respond to the individual comments and recommendations of reviewer 1, Matthias Beyer, MB. These responses are keyed to the specific comment by numbering, and are given in blue print, followed by indications of the changes made in the manuscript (in italics), and referring to the position in the original manuscript. Also, we revised again the entire manuscript for clarity, paying close attention to all of the reviewers' comments.

MB 1

Thank you for letting me review the manuscript 'The 18O ecohydrology of a grassland ecosystem – predictions and observations'. I enjoyed reading. In their work, the authors apply an 18O-enabled soil-plant-atmosphere transfer model in order to predict the dynamics of δ^{18}O in soil water, the depth of water uptake, and the effects of soil and atmospheric moisture on 18O-enrichment of leaf water in a grassland in southern Germany. In particular, they investigate the propagation of the δ^{18}O signal of rainwater through soil water pools, root water uptake and 18O enrichment of leaf water by tracing, predicting and validating δ^{18}Osoil, δ^{18}Ostem and Δ^{18}Oleaf. Finally, the authors test two models for describing Δ^{18}Oleaf at the canopy scale (the two-pool model or the Péclet model) and evaluate their performance.

We thank Matthias Beyer for the thorough and encouraging review and the detailed comments and recommendations that helped us much to improve the presentation of our work.

MB 2

Without doubt, this manuscript is well-prepared and written. The structure is clear, research questions are stated concisely, and the introduction provides a thorough overview on the topic. The graphics are suitable and well illustrated. I also agree to the authors that the model results are promising. The applied model MuSICA definitely seems capable of simulating ecohydrological processes including water isotopes. In my opinion, the hydrological and ecological community definitely needs a more integrated approach in modeling and investigating, and MuSICA seems a promising approach to that. I do not have major criticism on the manuscript, but a number of questions and comments that should be addressed in a revised version.

In summary those are: In general, I find that the discussion of the results needs to be more critical.

We revised the discussion thoroughly, considering all points raised by the reviewer (see responses to individual comments, below).

MB 3

Yes, the results are good for an uncalibrated model. BUT: Grass is (sorry for saying that) probably the simplest plant to model (homogeneous and short roots).

We are uncertain if modelling grass is inherently much simpler than modelling a non-grass species. For instance, the potential range of rooting depths of perennial grasses (and other grassland plants) can be very large (up to 6 m depth; cf. Schenk and Jackson, 2002), and grazing pressure (or defoliation frequency) can affect rooting depth very strongly (e.g. Klapp, 1971, Figure 43, page 81), providing scope for a large variability in rooting depth and depth of water uptake in different grassland systems.

In the revision we added a paragraph in the discussion pointing to this factor (see MB 9, below).

MB 4

Looking at the isotope results, the 20cm depth and also under dry circumstances does not really fit well – see R2. Hence, I would appreciate a more critical discussion, you have to highlight also the weaknesses that certainly still exist.

We believe that there is some misunderstanding here, and revised the text to eliminate any opportunity for such misunderstanding (again, see responses to individual comments, below).
In fact, the model performance for predicting $\delta^{18}O_{soil}$ at 20 cm depth was really good, as was indicated by the close relationship of modelled and observed data ($R^2 = 0.79$) and the very small bias (MBE = 0.5‰; Table 2). Also, the observations and the model agreed rather well with respect to the relationship between $\delta^{18}O_{stem}$ and $\delta^{18}O_{soil}$ (Figure 3): that relation was close in both the observed ($R^2 = 0.69$) and predicted data sets (0.65) and virtually unbiased at a depth of 7 cm, independently of soil water contents. Further, the predictions and observations agreed in that both indicated a poor relationship between $\delta^{18}O_{stem}$ and $\delta^{18}O_{soil}$ at 20 cm, both in terms of scatter ($R^2 = 0.34$ for the observed and 0.17 for the model predicted relationships) and bias. On average, $\delta^{18}O_{stem}$ was ca 2‰ higher than $\delta^{18}O_{soil\ 20}$, meaning that $\delta^{18}O_{soil\ 20}$ did not agree with $\delta^{18}O_{stem}$. Thus, both the observations and the modelling independently indicated that water uptake must have occurred mainly from shallow depths (<20 cm).

In the revision, we worked through the text and relevant Table captions and Figure legends very carefully to enhance clarity and eliminate any ambiguity on model performance (see also response to MB 7, below).

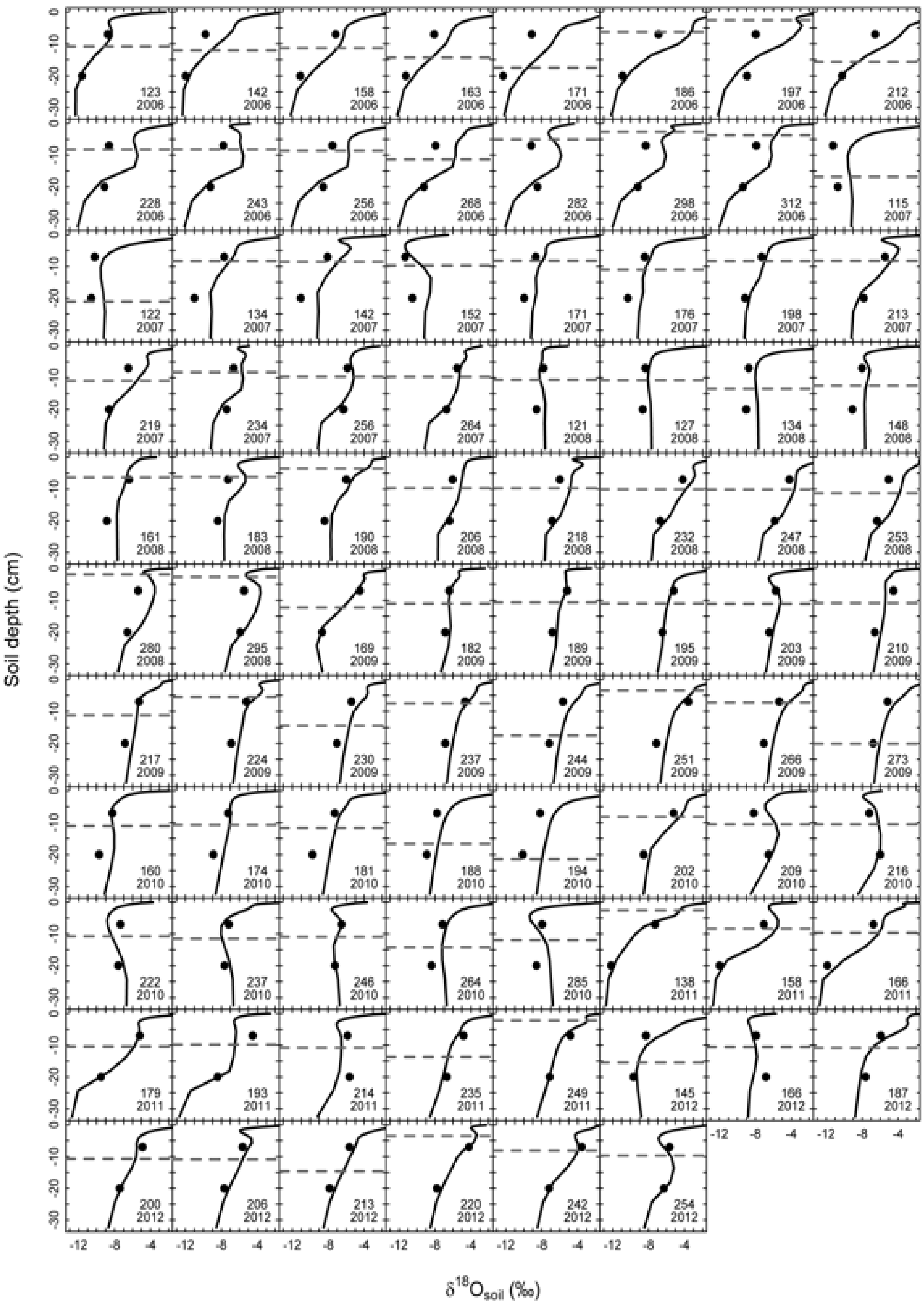
The following main changes were made:
Abstract (P1 L18ff): "The model accurately predicted the $\delta^{18}O$ dynamics of the different ecosystem water pools, *suggesting that the model generated realistic predictions of the vertical distribution of soil water and root water uptake dynamics. Observations and model predictions indicated that water uptake occurred predominantly from shallow (<20 cm) soil …*"

P11 L14ff: "*Conversely, the relationship between $\delta^{18}O_{stem}$ and $\delta^{18}O_{soil}$ at 20 cm was generally weak, exhibiting large scatter and a significant offset between $\delta^{18}O_{stem}$ and $\delta^{18}O_{soil}$ at 20 cm for most of the data (Fig. 3c).*"

P11 L22ff: "MuSICA simulations were based on this assumption and reproduced very similar relationships between $\delta^{18}O_{stem}$ and $\delta^{18}O_{soil}$ as those observed at both depths, with *similar R^2, MBE and MAE* (Figs. 2-3), *thus showing a close agreement between observed and predicted data.*

P14 L25ff: The comparison of observed $\delta^{18}O_{stem}$ and $\delta^{18}O_{soil}$ (Fig. 3a) strongly suggested that root water uptake occurred *mainly* at shallow depths *(<20 cm)* throughout the vegetation periods, largely independently of changes in SWC. *That interpretation of observed data was based on comparison of $\delta^{18}O_{stem}$ and $\delta^{18}O_{soil}$ at two depths (7 and 20 cm) only, which provides limited spatial resolution and cannot inform precisely on the depth of root water, if $\delta^{18}O_{soil}$ does not change monotonously with soil depth (Rothfuss and Javaux, 2017; Brinkmann et al., 2018). Such information can be improved by a locally-parameterized, physically-based, ^{18}O-enabled ecohydrological model, as shown here. For instance, the standard MuSICA runs (Fig. 3b) indicated near-monotonous increases of $\delta^{18}O_{soil}$ between 20 and 7 cm depth, matching well the observations in the majority of sampling dates (Fig. S13). Further, the simulations predicted a mean (uptake-weighted) depth of root water uptake at <15 cm, in 90% of all sampling dates, independently of SWC and observations of $\delta^{18}O_{soil}$. Support came also from the MuSICA sensitivity analysis (Fig. 6h) in showing that $\delta^{18}O_{stem}$ was well predicted by the model only when root length density was maximum at shallow soil depth. The potential range of rooting depths is large in grassland, depending on site, species, climatic and management effects (Schenk and Jackson, 2002; Klapp, 1971). So, why was root water uptake constrained to shallow depths in this drought-prone permanent grassland system? Several factors likely contributed: (1) the shallow top-soil overlying calcareous gravel (Schnyder et al., 2006), (2) the rapid shoot and root biomass turnover, that is associated with high phytomer dynamics leading to short leaf and root lifespan in intensively managed grassland (Schleip et al., 2013; Yang et al., 1998; Auerswald and Schnyder, 2009; Robin et al., 2010), (3) the high rates of shoot tissue (mainly leaves) losses that elicit a priority for assimilate (including reserve) allocation to shoot regeneration at the expense of the root system (e.g. Bazot et al., 2005), and (4) predominant placement of the root system near the soil surface dictated by the high need for nutrient interception and uptake (e.g. from excreta deposits), to compensate the high rates of nutrient losses due to grazing (Lemaire et al., 2000). Importantly, (5) in* *a relatively high number of cases, the model predicted situations in which rainfall recharged mainly the top soil, while SWC at depths >20 cm remained low (e.g. June-end of year 2006, April-October 2007, or May-end of year 2008; Fig. S12; see also below). Principally, however, factors (2)-(4) alone can explain why shallow rooting depth is a typical feature of intensively grazed grasslands (Troughton, 1957; Klapp, 1971). Also, Prechsl …"*

Further, we added a supplemental figure (Figure S13), showing *$\delta^{18}O_{soil}$ with soil depth as predicted by MuSICA (continuous lines) and mean uptake-weighted depth of root water uptake (dashed horizontal lines) on the different sampling dates. Closed circles: observations of $\delta^{18}O_{soil}$ at 7 and 20 cm depth. Sampling date is given by DOY and year, in the lower right corner of each panel*:



Legend of Fig. 3 (P29 L5ff):

"The R^2, MBE and MAE for the relationship between $\delta^{18}O_{stem}$ and the $\delta^{18}O_{soil}$ at 7 cm depth were 0.69, 0.2‰ and 0.7‰ for the observed data (a) and 0.65, –0.2‰ and 0.7‰ for the predicted data (b). Conversely, the R^2, MBE and MAE values for the relationship between $\delta^{18}O_{stem}$ and the $\delta^{18}O_{soil}$ at 20 cm depth were 0.34, 1.9‰ and 2.1‰ for the observed data (a) and 0.17, 1.8‰ and 1.9‰ for the predicted data (b)."

MB 5

Also, a total water balance is always a good means of validation and would be nice to have.

We agree with the reviewer. Unfortunately, we could not do a total water balance. E.g. we did not measure runoff (which was probably close to nil in this non-sloping pasture) and ground water recharge. The latter would have required installation of lysimeters, which was impractical on this intensively managed pasture. However, we did validate the model with latent heat flux data that were available from an eddy covariance station at the site, and we assessed the model's performance in predicting total plant-available water in the entire top soil by comparison with plant-available soil water modelling and data for the same site presented in Schnyder et al. 2006.

In the revision, we added a paragraph (P5 L23ff) stating: "*The model was validated with latent energy flux (LE) data obtained from an eddy covariance station (EC) at the site. According to that comparison (Fig. S1), MuSICA estimates were unbiased ($LE_{MuSICA} = 0.997\ LE_{EC}$; $R^2 = 0.59$). Further, we compared MuSICA predictions of total plant-available soil water (PAW, mm) in the entire top soil with PAW modelling and data for the same site presented in Schnyder et al. (2006). For the 2007-2012 data, this yielded the relationship $PAW_{MuSICA} = 0.99\ PAW_{Schnyder\ et\ al.\ 2006} + 7.8$ (R^2 0.83).*"

MB 6

The results section contains a lot of discussion (see detailed comments)

We eliminated discussion from the Results section following closely the reviewer's suggestions (see our answers to the specific comments below).

MB 7

Why was model not calibrated?

(This question is connected with point MB 5; see response above) We agree that we did not perform a classical calibration in the sense that the different model parameter values were statistically optimised. To do that we would have needed a greater number of hydrological measurements that we did not have (e.g. the dynamics of ground water recharge and soil water contents). The only instance where we did use parameter optimization (fine tuning) was in the case of the factors controlling ^{18}O enrichment of leaf water: mesophyll water content and night-time and minimal stomatal conductance (P9 L7-9), as well as the fraction of unenriched water in bulk leaf water. All other parameter values were based on measurements at the site, or – if such measurements were unavailable – on data from literature (as we explain). In that way we did ascertain realistic parameter values in this (otherwise) purely physically-based model. The fact that the model predicted well the $\delta^{18}O_{soil}$ at two different depths (that is a depth within the zone of most active root water uptake, 7 cm, and a depth just below that zone, 20 cm) did indicate strongly that the ensemble of parameters dictating soil water dynamics (including the spatial distribution of soil water uptake) in the zone of water uptake was described well by the model. This conclusion is further substantiated by the sensitivity analysis.

In the revision, we added the following short paragraph (see also response to MB 5) in P14 L15ff: "*The ability of the model to generate realistic predictions of the $\delta^{18}O$ dynamics at different depths in the soil (within the zone of most active root water uptake and just below that zone) suggests strongly that the ensemble of parameters dictating the spatio-temporal dynamics of soil water contents (including emptying and refilling dynamics) was described well in the model. That interpretation was also supported by the sensitivity analysis.*"

MB 8

Why was 2H not used? How was fractionation evaluated without 2H - did the authors simply use the offset of 18O from the LMWL? Is the model capable of modeling 2H as well? The dual-isotope space enables a more comprehensive understanding of processes. Also, it is more sensitive compared to 18O and since the authors did a sensitivity study, perhaps very useful. I don't say I expect that in a revised version, but I am interested on the authors opinion on that.

Yes, the MuSICA model is capable of simulating the δ^2H of soil water, xylem and leaf water. However, we elected to not include those data in the manuscript, as (1) we are primarily interested in the processes leading up to the δ^{18}O of cellulose, (2) we had noticed discrepancies in the model-data agreement for D/H that indicated fractionation (including a surface effect on D/H of soil water at the experimental site; Chen et al., 2016) that are currently not accounted for in the model. Hence, reporting both δ^{18}O and δ^2H would have changed the focus of the paper and would have brought up additional questions (that we wish to investigate in a separate paper). Also (3), we did not want to overload the paper with extra figures and discussion.

In the revisions we added the following sentence (P5 L27ff): *Although the MuSICA model is capable of simulating δ^2H of water pools in the soil-plant system, we excluded those data in the manuscript, as (1) we are primarily interested in the processes leading up to the δ^{18}O of cellulose, (2) we had noticed discrepancies in the model-data agreement for D/H indicating fractionation (including a surface effect on D/H of soil water at the experimental site; Chen et al., 2016) that are currently not accounted for in the model, and (3) we did not want to overload the paper with extra figures and discussion. Issues of D/H fractionation of water including data from this experimental site will be addressed in a separate paper.*

MB 9
Having that said, I suggest minor revision. I am looking forward to see the manuscript published in HESS.

Detailed comments:
Abstract l.20: grazing pressure, but how about rooting depth? Grasses are shallow-rooted so any other uptake is not expected?!

As we mention above, the potential range of rooting depths of perennial grasses (and forbs) is very large and dependent on a wide range of factors including site conditions, species and management conditions (particularly grazing pressure or defoliation frequency). So, the predominance of water uptake from shallow depths is not necessarily a universal feature of grassland.

In the revision we added a phrase in the Abstract, P1 L20ff
"The model accurately predicted the δ^{18}O dynamics of the different ecosystem water pools, *suggesting that the model generated realistic predictions of the vertical distribution of soil water and root water uptake dynamics. Observations and model predictions indicated that water uptake occurred predominantly from shallow (<20 cm) soil depths …"*

See also the detailed response to MB 4, above)

MB 10
l.20: respond to atmospheric moisture….does that mean leaves take up moisture from the atmosphere? (foliar uptake???)

Yes. Leaves exhibit bidirectional exchange of water vapour with the atmosphere, with a relative magnitude of the inward flux proportional to the relative humidity of the air, as we describe in the manuscript.

In the revision we changed the respective sentence to clarify the fact that it is actually the relative moisture 'content' of the atmosphere that drives the observed relationship. The sentence now reads (P1 L20): *"$\Delta^{18}O_{leaf}$* responded to both soil and atmospheric moisture *contents..."*

MB 11
l.21: two non-mixing pools: is that realistic or justified?

We see the point. Yes, the idea of two 'non-mixing' pools is a simplification, and unrealistic in the strict sense. The idea of having two discrete water pools in a leaf is the simplest conceptual model for explaining the observation that leaf water is usually less enriched than predicted by the Craig-Gordon model. The two-pool model is based on the notion that xylem and ground tissue are composed of unenriched water, whereas mesophyll cells are filled with evaporatively enriched water, implying constant fractions of unenriched and enriched leaf water (given full hydration of the leaves).
However, the reviewer is correct in questioning the realism of the 'non-mixing pools' idea, particularly in grasses that exhibit a continuous ^{18}O-enrichment towards the tip.

So, in the revisions we replaced the term 'two non-mixing water pools' by 'two pool' model characterized by constant proportions of unenriched and evaporatively enriched water. In the Abstract, this sentence now reads (P1 L20ff): *"$\Delta^{18}O_{leaf}$ responded to both soil and atmospheric moisture contents and was best described in terms of constant proportions of unenriched and evaporatively enriched water (two-pool model)."*

MB 12
l.26: the second sentence is not well written/unconcise

The revised sentence now reads:
"Meteoric waters impart their isotopic signal ($\delta^{18}O_{rain}$) to that of soil water ($\delta^{18}O_{soil}$), changing it as a function of refilling, exchange and percolation processes throughout the soil profile."

MB 13
l.29: explain better or provide citation – explain why do leaves fractionate

The revised sentence now reads:
"The oxygen isotope composition of leaf water ($\delta^{18}O_{leaf}$) differs from that of the water taken up from the soil, as leaf water becomes ^{18}O-enriched due to evaporative effects and morpho-physiological controls (Barbour 2007)."

MB 14
p. 2 l.14: 'source water' for plants would be soil or groundwater, but not xylem water as it is plant water already

We revised the sentence accordingly:
"The isotopic composition of the water taken up by plants (henceforth termed $\delta^{18}O_{stem}$) can vary over time through changes in the depth of soil water uptake by roots or direct changes in soil water isotopic composition."

MB 15
p. 2 l.15/16: 'summer' and 'winter' should be related to the particular study area, these statements are not true for the whole earth….

We modified the sentence accordingly: *"For example, summer rains in continental Europe are usually isotopically distinct (^{18}O-enriched) relative to winter precipitation, generating intra-annual variations of $\delta^{18}O_{soil}$ ($\delta^{18}O$ of soil water) with soil depth."*

MB 16

p. 2 l.29: 'enrichment above….' I know what you mean but this is written ambiguous – stem water can also be subject to fractionation under certain conditions. It should be more clearly expressed what is meant with this sentence.

We see the point.
Here we use the term $\delta^{18}O_{stem}$ to denote the $\delta^{18}O$ of the water taken up from the soil, and we define that term on first use. In what follows, we assume that there is no (relevant) further fractionation against ^{18}O, so that the water entering the leaf has the same $\delta^{18}O$ as that taken up by the root system as a whole.

We revised the annotated sentence, specifying that point: *"The mechanisms driving the isotopic enrichment of leaf water can be studied separately from those driving changes in $\delta^{18}O_{stem}$ by expressing the isotopic composition of leaf water as enrichment above $\delta^{18}O_{stem}$, i.e., $\Delta^{18}O_{leaf} = \delta^{18}O_{leaf} - \delta^{18}O_{stem}$, if the $\delta^{18}O$ of water entering the leaf is the same as that taken up by the root system as a whole.*

MB 17
p. 2 l.31: 'many authors' – could you provide some citations, please?

We added a citation to a pertinent review: Cernusak et al. 2016.

MB 18
p. 3 ll.2-14: this is well written!

Thank you!

MB 19
p. 3 l.15: is this relevant for grasslands only?

Actually, there is no reason to believe that this is only relevant for grassland.

So, we deleted 'grassland'.

MB 20
p.4.l.5: please review this sentence and provide more information…which species, which soil depths, what exactly is meant with 'growing season'

We added the requested info.

The paragraph now reads: *"To explore these questions we compared predictions from the ^{18}O -enabled soil-plant-atmosphere model MuSICA (Ogée et al., 2003; Wingate et al., 2010; Gangi et al., 2015) with those observed in a unique, multi-annual data set (7 years) of growing season (April to November), biweekly samplings and $\delta^{18}O$ analysis of soil water (at 7 and 20 cm depth), stem and midday leaf water, atmospheric water vapour, along with rainfall amount and $\delta^{18}O_{rain}$ data. The experimental site (Schnyder et al., 2006) was an intensively grazed Lolio-Cynosuretum (Williams and Varley, 1967; Klapp, 1965) community with Lolium perenne, Poa pratensis, Dactylis glomerata, Phleum pratense, Taraxacum officinale, and Trifolium repens as the main species. Vegetation samples were taken as mixed-species samples, as described below.*

MB 21
p.5.l8: though you cite a paper on the cryogenic system you use, it would be nice to specify temperature and extraction time here

We revised the sentence as follows:

"All samples were stored in a freezer at approx. -18°C until water extraction. Water was extracted for two hours using a cryogenic vacuum distillation apparatus with sample vials placed in a water bath with a temperature set to 80°C (Liu et al., 2016)."

MB 22
p.6.ll. 1 & 2-7: These information belong together, I'd suggest to either put the first part down or the second up

We followed the recommendation and revised the paragraph as follows:
"MuSICA was forced by half-hourly values of meteorological data and $\delta^{18}O$ of water vapour ($\delta^{18}O_{vapour}$) and rainwater ($\delta^{18}O_{rain}$). Wind speed, precipitation, air temperature, relative humidity and air pressure data were obtained from the Munich airport meteorological station, located at about 3 km south of the experimental site. Radiation was calculated as the mean of two weather stations located 10 km west and 12 km east of the experimental site. CO_2 concentration was measured at the site by an open-path infrared CO_2/H_2O gas analyser (LI-7500, LI-Cor, Lincoln, USA). For $\delta^{18}O_{vapour}$ and $\delta^{18}O_{rain}$, observations at the experimental site were used whenever available. Otherwise $\delta^{18}O_{vapour}$ and $\delta^{18}O_{rain}$ estimates were obtained from globally-gridded reconstructions derived from the isotope-enabled, nudged atmospheric general circulation model IsoGSM (Yoshimura et al., 2011). The IsoGSM-predicted $\delta^{18}O_{vapour}$ and $\delta^{18}O_{rain}$ at the grid point relevant to our site were first corrected for their offset with observed data, as predictions were found to be more enriched by 2‰ and 1.3‰ on average compared to the $\delta^{18}O_{vapour}$ and $\delta^{18}O_{rain}$ measured at the site (Figs. S2–S4)."

MB 23
p.7 l. 33: based on what was the beta distribution assumed (based on previous research or citation)

The beta distribution was shown to provide a good description of the vertical distribution of root-length-densities (e.g. Sadri et al., 2018).

We added a reference to Sadri et al. (2018).

MB 24
p.10.l 2: Why does the ratio need to remain 1.6?

In their review, Medlyn *et al.* (2002) found a close relationship between the potential rate of electron transport (J_{max}) and the maximum rate of carboxylation (V_{cmax}) for a broad range of crop, broadleaf and coniferous species. The slope of that regression was 1.6. Based on that study, we assumed a constant J_{max}/V_{cmax} = 1.6 also in our work (see Supplement, Table S1).

In the revision, we added the citation to Medlyn et al. (2002) in the main text. The sentence now reads: *"V_{cmax} and J_{max} were altered in tandem to keep the ratio J_{max}/V_{cmax} at 1.6 (Medlyn et al., 2002), the same as in the standard simulation (Table S1)."*

MB 25
p.10.ll. 4-6: Perhaps that fits better to 2.4.1 isoforcing

We revised the text in section 2.5 that was misleading, to clarify that the sentence relates to the sensitivity analysis and not to the isoforcing for the standard simulation.

That sentence now reads *"In addition, we investigated the effect of using uncorrected IsoGSM-predicted $\delta^{18}O_{rain}$ and $\delta^{18}O_{vapour}$ data instead of local isotopic data (gap-filled with offset-corrected IsoGSM data; see 2.4.1) for the isoforcing of MuSICA. This served to illustrate the usefulness of having local rainwater $\delta^{18}O$ data."*

MB 26
p.10.l21: Was predicted soil water content validated somehow?

Yes, we obtained a good agreement between predictions of soil water content with MuSICA with predictions obtained using the approach described by Schnyder et al. (2006) for the same site.

See response to MB 5, above

MB 27
p.11. l 29: in the way that (word missing)

We inserted '*in the way that*'.

MB 28
p.11: paragraph 3.4 contains a lot of discussion, I suggest reviewing and removing some of the 'judging' (e.g. last sentence or l.29/30)

We revised the paragraph, accordingly.

MB 29
p.12.l.21: MLR does not appear in the methods/statistics

We added in the Statistics section: *"Simple and multiple linear regression analyses and student's t tests were performed in R, version 3.4.2 (R Core Team, 2017) and RStudio, version 1.1.383 (RStudio Team, 2016)."*

MB 30
p.12.l.23: weakly significant? I think this should be rephrased ! significant or not

The P values for the predicted and observed regressions lay between 0.05 and 0.1, i.e. close to significant. Thus, the sentence was rephrased as follows: *"The interaction effect of air relative humidity and SWC was close to significant for both observed ($P = 0.080$) and predicted ($P = 0.073$) $\Delta^{18}O_{leaf}$ (Table 4)."*

MB 31
p.12. paragraph 3.5.: the authors mix VPD and relative humidity quite a lot here, which makes this chapter hard to read. I suggest restructuring and rephrasing of this chapter (though the results completely make sense)

We agree and restructured the paragraph.

The new text now reads: *"Multiple regression analysis demonstrated significant effects of air relative humidity ($P < 0.01$) and SWC ($P < 0.05$) on both observed and predicted $\Delta^{18}O_{leaf}$ (Table 4). $\Delta^{18}O_{leaf}$ increased with decreasing air relative humidity and SWC (Figs. 4a, b and 5a, b). The interaction effect of air relative humidity and SWC was close to significant for both observed ($P = 0.080$) and predicted ($P = 0.073$) $\Delta^{18}O_{leaf}$ (Table 4). The effect of dry soil conditions on $\Delta^{18}O_{leaf}$ was most evident at low air humidity (Figs. 4a, b and 5a, b) and was connected with a decrease of canopy conductance (g_{canopy}) (Fig. 5c).*
The modelled dependence of transpiration on air VPD (the climatic driver of transpiration) was strongly modified by SWC (Fig. 4c). High air VPD drove high transpiration rates only under wet soil conditions ($SWC \geq 0.25$)."

MB 32
p.13l 4-10: Discussion
p.13. l.26-32: This sounds more like a conclusion

This paragraph is summarizing the main observations on model-data agreement. We would like to retain it, as it is.

MB 33
p.14. l.5: quite

We removed 'quite'

MB 34
p.14. l.6-7: suggest rephrasing: 'likely result from sampling effects and analytical error'

We agree and rephrased the sentence as follows: "*The greater scatter in the observed relationship between $\Delta^{18}O_{leaf}$ and relative humidity compared to predictions (Fig. 4) likely resulted partly from sampling effects and error.*"

MB 35
p.14. l.12-23: I agree, but also it should be clear that grass with a fairly uniform uptake depth right below surface is probably the easiest of plants to model. This is not a criticism but would be interesting how the model performs for different plant types.

We agree, in principle. Yes, it would be extremely interesting to also test the model for its performance with different biomes in different site conditions, exploring also especially systems that include deep-rooted species.

MB 36
4.2: I am not sure if this deserves an own chapter. I believe that it is true that the grass takes the water mainly from the upper depths but considering the characteristic shape of soil water isotope profiles at the surface (enrichment and subsequent decrease of isotope values towards a constant value), the used resolution of only 2 depths might not reveal true uptake patterns. Also see Rothfuss and Javaux, 2016.

We see the point, and the caveat. We are aware of the fact that the soil water $\delta^{18}O$ values from only two depth positions do not necessarily reflect the total range of $\delta^{18}O$ expected for the entire soil profile. Nevertheless, the model simulations generated a detailed prediction of how $\delta^{18}O$ varied along the profile. For the sampled depth, the predictions matched the observations generally well. We added a supplemental figure (Figure S13) showing the predicted soil water $\delta^{18}O$ profiles (see response to MB 4, above). The most extreme (positive) values were predicted for the uppermost 1-2 cm of the soil (Fig. S13), as a consequence of evaporative ^{18}O enrichment at the soil surface. The model predicted very little root water uptake in that zone (Fig. S12).
The $\delta^{18}O$ of soil water at 7 cm was greater (i.e. more enriched) than the $\delta^{18}O$ at 20 cm for 79 out of 86 cases, i.e. for more than 90% of the dataset. In line with that, the model mostly predicted a decrease of $\delta^{18}O$ between 7 and 20 cm, which was monotonous for a large part of the dataset (new Figure S13). Even if the decrease was not monotonous (e.g. in late summer/autumn of 2006), the highest and lowest $\delta^{18}O$ values were still found in the upper and lower profile, respectively. Hence, at least the qualitative assessment that the roots take up the water from the shallow horizon was still valid in those cases.
On 12 days, $\delta^{18}O_{soil}$ was predicted to be quite constant from approx. 5 cm to the bottom of the profile. In those specific cases, additional soil samples between 5 and 37 cm would not have had additional value with regard to inferring the depth of water uptake by comparing $\delta^{18}O_{stem}$ and $\delta^{18}O_{soil}$. On another 6 days in 2008 and 2010 (e.g. DOY 209 and 285 in 2010), the uptake depth could not be unambiguously inferred by comparing $\delta^{18}O_{stem}$ and $\delta^{18}O_{soil}$. Considerable rainfall had occurred in the two weeks preceding those sampling days (e.g. 61 litres of rain during DOY 203 to 208 of 2010), creating non-monotonous isotopic profiles (e.g. an S-shaped profile on DOY 209 of 2010). In those cases, the model predictions, which were solely based on hydraulic properties of the soil, root architecture and evaporative demand, and not on observed $\delta^{18}O_{soil}$ data, can help to deduce the root water uptake depth. For day 209 in 2010 for example, the model predictions indicated that the average mass-weighted root water uptake depth was located at 10.5 cm (dashed horizontal line in Fig. S13 for that DOY).

We revised this chapter thoroughly, paying close attention to the reviewers' concerns. See response to MB 4, above.

MB 37

p.15. l.26-27: 'online transpiration isotope method' this appears here for the first time?

Yes. These data help us in the discussion, in that they provide supporting evidence for the two-pool model also for individual grass species (that were part of the codominant species in our grassland community).

The methods and results of these supplementary experiments with *Lolium perenne* and *Dactylis glomerata* are described in the Supplement. The citation to that description (Notes S2) was missing and is now added to the revised manuscript:

"We did not know if putative between-species differences in leaf water dynamics and associated ^{18}O-enrichment, or any other morpho-physiological effects e.g. associated with leaf aging, could have led to a missing correlation between the proportional difference between measured leaf water ^{18}O-enrichment and that predicted by the Craig-Gordon model ($1 - \Delta^{18}O_{leaf}/\Delta^{18}O_e$) and transpiration rate. For these reasons, we explored this question with separate studies of L. perenne and D. glomerata, two species that also formed part of the present grazed grassland ecosystem. Again, these studies found no evidence for a Péclet effect, and supported the two-pool model, as there was no relationship between the proportional difference between measured leaf water enrichment and that predicted by the Craig-Gordon model ($1 - \Delta^{18}O_{leaf}/\Delta^{18}O_{e,ss}$) and transpiration rate in either *L. perenne* plants grown in a controlled environment at different relative humidities and water availabilities, or *D. glomerata* leaves measured using an online transpiration isotope method *(Notes S2 and Figs. S14-15)."*

MB 38

p.16 l.9-11: I like this chapter, but the last sentence does not make sense – why compare and justify grass species with a study on non-grass-species?

We do not wish to justify our data by comparison with non-grass species. However, it is interesting and important to note that the range of proportional differences between measured leaf water ^{18}O enrichment and that predicted by the Craig-Gordon model (φ) is very similar in grasses and dicots.
We revised the faulted sentence, which now reads: *"Considering a similar effect of vein removal would move our observed φ to about 0.2. Such a value of φ for grasses is very similar to the mean φ reported for a wide range of non-grass species by Cernusak et al. (2016)."*

MB 39

Conclusions: An experienced and known Professor once gave me the advice 'A good paper doesn't need a conclusion – the reader draws it him/herself.' The authors should decide themselves, but I feel emphasizing some key points in the manuscript/abstract a bit more would be sufficient without conclusion.

We deleted the Conclusions, and emphasized key points, as documented above.

MB 40

Fig. 3: As stated above, the model does not work that well for 18O. I think this needs to be discussed thoroughly

See our response to MB 4 (above).

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