Reply to Interactive comment on "Relating stable isotope and geochemical data to conclude on water residence times in four small alpine headwater catchments with differing vegetation cover"
by M. H. Mueller et al.
Anonymous Referee #2

Reply:
We thank the anonymous referee #2 for his comments and discussion, which will help to improve our manuscript. Please see below for our detailed answers and suggested revisions (in red).

General Comments:

Referee comment:
This paper aims to add to the ongoing discussion on catchment response controls, specifically on the influence of vegetation cover on mean transit times. This is an important aspect that has not been discussed very much in detail before. Still, after reading the manuscript, it appears that the paper deals mostly with other topics and the vegetation effect is only a side note.

Reply:
We will rethink our title, however the effect of vegetation cover was the main hypothesis behind the project. However, the effect of vegetation cover is quickly discussed, but other factor play a crucial role. We thus think it is important to address these factors.

Referee comment:
In a similar manner the geochemical data is presented but not used sufficiently to support the arguments on transit times.

Reply:
The geochemical data gives evidence that the water cycles through the deeper bedrock which contains gypsiferous or calcareous layers. Thus, it is another evidence to support our mean transient times. We are not sure how we should have used the data more so that you would judge it "sufficient"?

Referee comment:
Overall, there are too many topics that the authors try to address but mostly in a superficial way leaving out important information in too many places and not integrating the pieces into a consistent story.

Reply:
We are sorry you feel that we leave out important information and that we address the topics in a superficial way. It would be helpful to know which information you are missing?

Referee comment:
Some of the modeling concepts are not clear either. For example the use of base flow samples. I'm not sure whether the term 'base flow' in this paper refers to streamflow, because there is no explanation on how base flow was sampled or distinguished from storm flow.

Reply:
Yes, base flow refers to streamflow (as stated on p. 11010 l. 9) during base flow conditions. The latter were separated from storm flow by judging the respective hydrograph (runoff peaks before, during and after precipitation events) We will add a sentence explaining this in the revised manuscript. For more details see below.

Referee comment:
Also, I was trying to make sense of the synthetic experiment but could not see the value in reducing the summer precipitation input and adjusting the transfer function in the convolution model to match the output isotopic signal that was produced by different input fluxes.

Reply:
See below in the section “specific comments”.

Referee comment:
To sum it up, I think the paper needs some streamlining and a consistent story. The authors should concentrate more on explaining important details of the methods they use, clearly stating assumptions and hypotheses. In order to do this they need to consider more of the newer literature that has been published on transit time modeling (in the last 3 years).

General comment by the authors:
We thank you for your comments and suggestions. Your specific comments will be discussed in detail below. The aim of our investigation was to compare four different small mountainous catchments in terms of their hydrological behavior taking various aspects like vegetation, but also hydrogeology/geochemistry into account. In order to address your comments/questions we will add more detailed information and condense the different topics to produce a more consistent manuscript. Nevertheless we think the different data we measured and calculated give us an integrated picture of the catchments we looked at.

Specific Comments:
Referee comment:
p. 11006, l. 19: ‘the geological and topographical situation’: You have not mentioned this before. Could you be a little more specific here?

Reply:
Here we refer to the underlying geological material and its characteristics (briefly described on p. 11006, l. 15) which we think are an important factor controlling the mean water transit time. We will rewrite this sentence to specify the statement and insert additional information in the abstract.

Referee comment:

p. 11006, l. 24: Residence time and transit time are not the same! The first one is describing the age distribution of all the water in a catchment, the second one is characterizing a specific precipitation event and the time that the water from this specific event needs to transit through a catchment. Please refer to McDonnell et al. 2010 (in your references already) or Hrachowitz et al. (currently under review in HESS) and change your terminology.

Reply:
Unfortunately there is a wide spread use of different terminology throughout the literature and different definitions are used sometimes by the same authors as already mentioned by McGuire and McDonnell (2006). As you also mentioned in your later comment concerning p. 11012, l. 1 (see below) we are modeling with the exponential model which 'is mathematically equivalent to the response function of a well-mixed reservoir' (Maloszewski and Zuber, 2002). Moreover we assumed steady state conditions. Therefore, as you mention in your later comment, mean residence time (= mean age in (McDonnell et al., 2010)) = mean transit time. In order to adapt our terminology to our purposes, we will rephrase this statement and adjust the terminology we used throughout the manuscript (p. 11006, l. 24).

Referee comment:
p. 11007, l. 2: …calculated via 'time series of' stable isotopes…..

Reply:
…'time series of'… will be added.

Referee comment:
p. 11007, l. 3: The variation of the isotopic signature is not solely dependent on varying temperatures (can be caused by varying storm tracks, precipitation event volumes, etc.).

Reply:
Yes, we agree. Our statement does not exclude the influence of storm tracks or precipitation volumes. Here we wanted to focus on the regularly (seasonally) variations which can be used to 'track' the water flow, especially the time elapsed between recharge and discharge.

Referee comment:
p. 11007, l. 20: You mean that more freely draining soils cause more mixing of new incoming water with older groundwater (instead of soils that create more subsurface, macropore and overland flow).

Reply:
Yes. Soulsby and Tetzlaff (2008) stated that more freely draining (which facilitate groundwater recharge) soils could enhance mixing of 'new' and 'old' waters which consequently results in longer mean transit times estimates. We will rephrase this sentence to be more specific.

Referee comment:
p. 11008, l. 1: Runoff generation processes can be altered 'in what way'? Please give more details.

Reply:
Wenjie et al. (2009) (we have to correct the year of this reference) found that the land management in their study area lead to soil compaction which resulted in infiltration-excess overland flow. This in turn lead to higher fractions of event water in runoff compared to their 'control site'. We will include additional information in the revised manuscript.

Referee comment:
p. 11008, l. 6: This sentence is unclear.

Reply:
By this we refer for example to the fact that the stable isotope value of shallow groundwater can deviate from the volume weighted stable isotope signal of precipitation when there are seasonal biases in recharge (Clark and Fritz, 1997). Recharge in turn can also be influenced by land use as subsequently stated by Clark and Fritz (1997). As Clark and Fritz (1997) themselves refer to the study of Darling and Bath (1988) we will rephrase this section to clarify your questions.

Referee comment:
p. 11008, l. 9: 'They' refers to Clark and Fritz or Darling and Bath?

Reply:
See above. Clark and Fritz (1997) themselves cited Darling and Bath (1988). Therefore we will rewrite this sentence.

Referee comment:
p. 11008, l. 9: So Darling and Bath did not find this effect in their study? They just hypothesized about it?

Reply:
Darling and Bath (1988) actually measured isotopically depleted soil water in drillcores from the unsaturated zone beneath permanent grassland sites compared to arable sites. Here we added the information that they additionally collected water at the bottom of a lysimeter. The stable isotope composition of this water corresponded to the stable isotope composition of the groundwater, a result which was in contradiction to the results from the drillcores. They concluded that land use can have an influence on the stable isotope composition of soil water, but this effect can be balanced in the groundwater depending on the relative area of grassland and arable land (in their study) leading to different flow patterns in the soils.
We will reformulate and specify these sentences.

Referee comment:
p. 11008, l. 13: Again, you just state that there were changes, but you don't tell what they changes were. You need to summarize these studies in your introduction better, also including their results as they relate to your study.

Reply:
In their study Stumpp et al. (2009) compared a lysimeter cultivated with a maize monoculture and a lysimeter where crop rotation was applied. They obtained differences in water flow velocities between the two lysimeters during different periods. They

concluded that different crop growth lead to different hydraulic systems over time. In their study from 2012 the authors conducted a similar experiment but with different lysimeters and different treatments. We will explain this in more detail in the revised manuscript.

Referee comment:
p. 11008, l. 16: You hypothesize that there will be changes but you don't tell which changes in what direction.

Reply:
We think that the shrub encroachment could lead to higher infiltration rates of water into the soils (as stated on p. 11008 l. 18) and therefore lead to shorter transit times of soil water (similar to Asano et al., 2002) – but soil water is not the focus of our study. According to Soulsby and Tetzlaff (2008) more freely draining soils could lead to longer estimates of mean water transit times of stream flow (see also your comment above). We therefore hypothesize that higher infiltration rates of water into the soils (due to the shrub encroachment) and subsequent recharge to groundwater in the bedrock will result in longer mean transit times. We will extend our hypothesis in the revised manuscript to be more specific.

Referee comment:
p. 11008, l. 21: What do you mean by 'scale dependency'? Which scale?

Reply:
Here we refer to the scale of catchment area. We would like to point out that we primarily focus on four small catchments (< 1 km^2) which we compare among each other. But we also measured a (shorter) time series of stable isotopes for a larger catchment (132 km^2) which gives us a qualitative estimate about the mean transit time of this larger catchment.

Referee comment:
p. 11010, l. 19: How did you sample only stream base flow if you had a fixed sampling every 14 days? How did you distinguish base flow conditions from event conditions?

Reply:
The distinction of base flow and storm flow before sampling was done qualitatively. This was done by expert knowledge in the field before the sampling. We did not apply a quantitative definition of base flow since it can vary seasonally within a certain range (higher base flow in spring and early summer). For data interpretation, base flow was defined as the 'baseline' in the hydrograph subtracting storm flow peaks. These runoff peaks could be clearly associated to precipitation events. From discharge and precipitation measurements we knew that runoff very quickly responds to incoming precipitation within 20-30 to 60 minutes (depending on precipitation intensity). Moreover, storm flow very quickly decreased back to pre-event (base flow) conditions within approximately 12 hours after the runoff peak from a precipitation event. In the case of expected non-base flow conditions at the chosen sampling day we adapted the sampling strategy and sampled one day in advance or after the originally fixed day.
We controlled our judgment later in evaluating the hydrograph and applied the criteria of Wittenberg (1999) to calculate the contribution of stream base flow to total discharge. During our 2 year sampling campaign only a few planned sampling days coincided with event flow conditions. We think that our sampling strategy is appropriate since we aim to

estimate a mean water transit time and difference of one or even two days in sampling seemed to be acceptable to us.

Referee comment:
p. 11010, l. 23: Did you melt the whole snow column to measure water isotopes? Did you also measure water isotopes from naturally occurring snow melt (as input to the flow system)? The two methods could potentially yield significantly different isotope values…

Reply:
During snow sampling in the field we directly transferred the snow into 2-L-bottles which were closed tightly. Afterwards these bottles were transported to the lab and we waited until snow was melted to take a subsample for stable isotope analysis. In spring 2012 we sampled the (bulk) melt water of snow at one location and there were only a slight difference between the bulk snow sample and the melt water at this site. Nevertheless, we are aware of the uncertainty we introduce by taking the bulk snow sample as our input signal for the mean transit time modeling and we discussed this in section 3.2.2 'Evaporation of snow…'. Please see also furthers comments on snow melt inputs below.

Referee comment:
p. 11012, l. 1: I recommend calling this transit time modeling. I understand that you are assuming the system to be in steady-state and hence transit time = residence time; still the term transit time would better reflect what you want to express in the paper.

Reply:
Yes, thank you for this comment. We will rename the section and adapt our terminology in the manuscript (see also earlier comment for further details).

Referee comment:
p. 11018, l. 12: This is the first time you mention the Reuss river and later the wetland site. You should introduce them in the study site description.

Reply:
We will add information on the Reuss river and the wetland site in the study site section.

Referee comment:
p. 11018, l. 26: Enriched in what? In the heavy or in the light isotope?

Reply:
We will add this information in the revised manuscript. The samples from the wetland site have more positive $\delta^{18}O$ values than the respective stream water samples. The wetland samples therefore show a stronger influence of precipitation water.

Referee comment:
p. 11018, l. 27: I don't understand this. Almost all water in the water cycle is of meteoric origin. Maybe you want to say that the subsurface/overland flow derives mainly from precipitation that has not traveled via deeper flow paths?

Reply:

Yes, we conclude that the subsurface/overland flow mainly is formed by quickly ‘discharging’ precipitation and does not originate from deeper zones. We will rewrite this sentence to be more precise.

Referee comment:
p. 11019, l. 8: Do you mean ‘less negative’ values? Please be more precise with the isotope nomenclature.

Reply:
We understand that a ‘higher’ $\delta^{18}O$ value of a sample corresponds to ‘less negative’ $\delta^{18}O$ value compared to another sample. We will adapt our nomenclature so that we are consistent throughout the manuscript.

Referee comment:
p. 11019, l. 9: Why was it more pronounced in the modeled data?

Reply:
For the Wallenboden, Bonegg and Laubgädem micro catchments the mentioned effect was more pronounced in the modelled data, whereas this was not the case for the Chämleten micro catchment. Model tests revealed that the influence of snow inputs could still be underestimated by our approach which in turn means that the snow component is a crucial component in our system as we stated on p. 11029 l.6. Since the assessment of the snow melt input is not trivial, it introduces uncertainty to the mean transit time modeling and consequently to the calculated stable isotope values of stream water. (for more details on the stable isotope values of snow as input into the model data, see comments below).

Referee comment:
p. 11019, l. 16: Better write: ‘The model estimates the same mean water…’, because it is very apparent that the real mean has to be longer just from looking at the data.

Reply:
Our intention was to refer to the model estimate by writing ‘calculated mean water….’. But we will rewrite this sentence in order to be more specific at that point.

Referee comment:
p. 11019, l. 21: The real problem is that you assume that the mean transit time is time-invariant although it has been shown that it varies from season to season and from event to event. You should acknowledge that and refer to papers of (Botter et al. 2011, van der Velde et al. 2010, Heidbüchel et al. 2012, Hrachowitz et al. 2010).

Reply:
We are aware of the problems in assuming the mean transit time to be time-invariant. Nevertheless we think that our data and the modeling can give us useful information on the behavior of these four micro catchments. We also think our approach is justifiable since we inter-compare four micro catchments in the same valley under the same boundary conditions (e.g. same time span, climate and geology). Nevertheless we will add a discussion on the assumptions we made by using the time-invariant approach and the implications of time-variant transit times.

Referee comment:
p. 11020, l. 3: This is no justification for including snow melt samples if you are looking for base flow mean transit times.

Reply:
Since we are dealing with a nival runoff regime and snow melt inputs and its contribution to groundwater recharge, which subsequently is discharged in our catchments, can be important throughout the months of June, July and August, we consider the snow component as an important part in the water (stable isotope) balance. Your earlier comment on the distinction between base flow and storm flow already pointed in a similar direction. We are aware that especially in this mountainous region 'base flow' has a different level in different seasons. In our micro catchments the snow melt itself takes place during approximately six weeks until 'all' the snow in the micro catchments is gone. On the other hand the 'extreme' negative peak of stable isotopes in stream water, which is produced by fast snow melt inputs, only lasts about 7 days and only in Laubgädem we detected a very extreme snow melt peak.
However, the applied software calculates the stable isotope values of stream water by a chosen transfer function (flow model) and a mean transit time. Calibration of the model is carried out by a trial-and-error procedure by comparing the modelled stable isotope values of stream water with the measured ones via the sigma value (measure for the goodness-of-fit, p. 11023 l. 10). Leaving out the 'extreme' snow melt peaks of stream water would reduce the sigma value but it would not improve the matching of the modelled and the measured data, which can be judged visually.

Referee comment:
p. 11020, l. 24: The reasoning is unclear.

Reply:
We refer to the fact that the volume weighted stable isotope signal of the precipitation input equals the mean stable isotope signal of the stream water output (p. 11020 l. 21). Our hypothesis is that a stronger influence of summer evapotranspiration should result in a difference between the above mentioned mean stable isotope values. We agree that a possible evaporation from snow cover and fractionation processes could average out the effect of evapotranspiration on the mean of stream water stable isotopes. Nevertheless evaporation during the winter period is estimated to be very low (Baumgartner et al., 1983) – especially from these north facing slopes.

Referee comment:
p. 11021, l. 23: Enrichment of what?

Reply:
'….enrichment of the heavier stable isotopes….' We will add this information.

Referee comment:
p. 11021, l. 27: But the input to the system is the snow that melts from the snow pack. That snow melt water is generally much enriched in the lighter isotope and only later during the snowmelt becomes more enriched in the heavier isotope. So your reasoning,

that there is only little change in the snow pack isotopic signature does not address the real problem of input characterization.

Reply:
We are aware of the drawback not having data from snow melt lysimeters and the limitations of using the bulk snow samples as our input data. However, we are not describing the input signals in high frequency resolution but medium term values over two or more weeks. Water balance estimates suggest that only a minor fraction of the total accumulated snow during melt is lost via runoff and the bigger fraction recharges to the groundwater. Assuming that evaporation from snow during snow melt at our sites (mainly March to April, (Baumgartner et al., 1983)) is minor, the input signal from the snow melt should be reflected by the mean isotope signal of the accumulated snow recharging the aquifer.
However we will perform additional mean transit time modeling with different input parameters during/shortly after snow melt and report the data in order to give a range of the uncertainty introduced by the snow component.

Referee comment:
p. 11022, l. 13: This cannot work because isotope values are not only related to air temperatures. You should rather repeat the measured time series a couple of times.

Reply:
We are aware that stable isotope values are not solely dependent on air temperature and other factors can also play an important role. Nevertheless, in our study we obtained a very good linear correlation between stable isotope values of precipitation and air temperature for the Ursern valley: $\delta^{18}O = 0.73 * T$ (°C) - 16.89, $r^2 = 0.84$, $p<0.0001$, $n = 145$. Moreover, extending the stable isotope time series by this technique was already successfully used by other authors and proved to be a valuable tool to calculate stable isotope data (Burns and McDonnell, 1998;Uhlenbrook et al., 2002).

Referee comment:
p. 11022, l. 16: How did you use that measured record to reduce input uncertainties?

Reply:
As described on p. 11022 l. 14, we used the extended stable isotope series (to about 5.5 years) as input for the model and calculated the stable isotope values of the stream water with the exponential flow model for different mean transit times. We then compared this calculated series of stable isotope values of stream water with the measured stable isotope values of the stream water in our measured 2 year series. From this comparison we could conclude that in our case the extended time series did not help to improve the fit of the modelled data to the measured data.

Referee comment:
p. 11023, l. 7: Unambiguously, really? There is always uncertainty and Figure 5 shows that there is little difference between 50 and 80 weeks for three of the catchments although there is only one minimum value.

Reply:
By ‘unambiguously’ we refer to the minimum value for each catchment, which you also mention in your comment. Nevertheless we agree with you that there is an overlap for the three micro catchments Chämleten, Wallenboden and Bonegg. We will introduce

additional comments on the uncertainty of our approach and give ranges for the estimated mean water transit times. We are totally aware that these numbers should not be taken as absolute numbers rather than to compare our micro catchments.

Referee comment:
p. 11023, l. 10: Is this a common goodness-of-fit measure? Why not use the Nash-Sutcliffe Efficiency, it would also provide information on whether your modeling is better than using an average value.

Reply:
The FlowPC software also provides the Nash-Sutcliffe efficiency (Maloszewski and Zuber, 2002). We compared the 'best fits' as defined by the 'sigma value' and the Nash-Sutcliffe efficiency for different mean transit times. The best fit as defined by the Nash-Sutcliffe efficiency turned out to strongly overestimate the measured stable isotope values of stream water in summer (more positive values) and strongly underestimate them in winter (more negative values). In our case the stream water stable isotopes were strongly dampened and the differences between the measured stable isotope values and the respective mean of stable isotope of stream water are very low. We therefore considered the Nash-Sutcliffe efficiency not to be the best goodness-of-fit measure in our case. Instead we used these 'sigma-values' implemented in the used software. They can be used to evaluate the goodness-of-fit for each catchment individually.

Referee comment:
p. 11023, l. 21: I don't understand this sensitivity analysis. You reduced the precipitation input and assumed that the output signal remains unchanged. Then you adjust the transfer function so that it again produces the observed output (that was created by different input). What information do you gain by doing this? I would expect that the output signal would change too, if the input signal was changed.

Reply:
The aim of this sensitivity analysis was to gain information of the vegetation cover on the mean transit or rather the stable isotope values of stream water under the same boundary conditions (except the vegetation cover). We calculated the stable isotope values of stream water by using the same mean transit time and flow model as determined in section 3.2.1. under the assumption that during summer there had been less infiltration of precipitation into the system than we used in our original model input data (in section 3.2.1). So this experiment gives information what the stable isotope signal in the stream would look like if there was more evapotranspiration through invading shrub (leading to less input of summer precipitation into the system).

Referee comment:
p. 11025, l. 1 - 22: You could remove the whole geochemistry section. It is a little confuse and doesn't add to the story.

Reply:
We think the geochemical data adds valuable information to our stable isotope data and helps to put the results in a wider context (please see above). It supports our conclusions we draw from the stable isotope modeling and gives information of possible flow paths in the underground. Together with the section on hydrogeological aspects we think it helps to deal with the whole hydro(geo)logical systems from different

perspectives. In order to streamline the manuscript and to be more specific on that point we will rewrite the section.

Referee comment:
p. 11026, l. 26: What do you mean by 'If we compare these results with the time series of the Reuss river…'. Compare them in which way, please give more details.

Reply:
By 'compare' we mean a visual qualitative comparison of the different time series and their temporal variations. This is not intended in a quantitative way as we mentioned on p. 11018 l. 16.

Referee comment:
p. 11027, l. 24: If you use mean water transit time and mean discharge you will only get an average mobile catchment storage. However, the storage is likely variable and at certain times potentially much larger.

Reply:
We agree with you that we calculate a mean mobile catchment storage and that it can be variable in time – as the mean transit time. Our aim is to estimate hydro(geo)logical parameters of our systems and to integrate additional information in order to get a broader picture of the catchments' hydrological behavior. We think that these additional information (including also the geochemistry) can also serve to check our different data sets for plausibility.

Referee comment:
p. 11027, l. 26: How exactly did you estimate the volume of rocks? Please give more details.

Reply:
Originally we estimated the volume of rocks by measuring the approximate extend of the catchments (length * mean width * height/2). Height was calculated as 'altitude of highest point minus altitude of the catchment outlet sampling point'. We are aware that this is a simple estimate which involves a certain degree of uncertainty. However, it gave us a rough estimate of the volume of rocks in order to calculate a mean porosity of the rocks. We now validated our rough estimation with the ArcGIS Hydro Tool (Terrain Morphology) and resulting rock volumes (and subsequently porosity and hydraulic conductivity) are in the same order of magnitude (see also your comment on uncertainty below). We will give the ArcGIS numbers in the new version of the manuscript.
However, we are also aware that the subsurface catchment area does not necessarily correspond to the surface area (see also p. 11028 l. 14), which could introduce uncertainty to the above mentioned numbers.

Referee comment:
p. 11027, l. 27: An equation that shows how you used Darcy's law to estimate porosity would be helpful.

Reply:

We used the mean mobile catchment storage and the estimated volume of rocks to calculate a 'mean' porosity n for the whole rocks: $n = \frac{V_{H_2O}}{V_{rocks}}$. From the topographic data we also estimated the flow path length x to calculate the tracer velocity $v = \frac{x}{MTT} \cdot n$.

These data were subsequently used to calculate the hydraulic conductivity with Darcy's law:

$K = \frac{v}{\frac{\Delta H}{\Delta x}}$; ($\frac{\Delta H}{\Delta x}$ is the hydraulic gradient). (e.g. Zuber, 1986)

Referee comment:
p. 11028, l. 2: The values you are giving (3.46x10-4 to 4.09x10-2) are not within the range of values given by Frick and Himmelsbach (7.4x10-3 and 1.3x10-3). It's the other way around.

Reply:
Sorry, we agree that our formulation is a little bit misleading. We wanted to say that our values, which we estimated from hydro(geo)logical data, are in the same order of magnitude as the measured ones by the cited authors. This again is a good check for plausibility of our data.

Referee comment:
p. 11028, l. 26: This is the first time you mention that you expect karst formation at you test sites.

Reply:
Initially karst formation was not expected at our test site and the area is also not particularly known as a karstic environment/landscape since outcropping geological material of the north facing slopes mainly consists of gneiss and/or granites. Typical karst phenomena can not be observed at the surface, but the stream geochemistry suggests some influences of dolomitic/calcareous/gypsiferous rocks. Geologists have confirmed the existence of these types of rocks in this region (p. 11009 l. 12). We will introduce our conclusions in previous sections.

Referee comment:
Table 1: The correct English term for the German word 'Exposition' is 'aspect'.

Reply:
Thanks, we will change this and use the correct expression.

Referee comment:
Table 3: The values you are presenting here are probably very uncertain, so don't add two decimal places.

Reply:
We agree that these values must be considered as estimates and we will therefore adapt the representation of the given numbers.

Referee comment:
Figure 1: Is it Ursern or Urseren Valley?

Reply:
In fact unfortunately both names can be found in the literature. We will stick to 'Ursern' to be consistent throughout the manuscript.

Referee comment:
Figure 7: You can remove this figure. Since there are no significant correlations anywhere, you can simply state that in the text.

Reply:
Despite the lack of significant correlations we decided to include this figure into the manuscript in order to provide the actual numbers of the different parameters to the reader. We think this is useful when data from other studies will be compared with our study.

Referee comment:
Figure 8: The same applies for this figure.

Reply:
We agree in the case of Figure No. 8. We consider removing it and we will mention the lack of correlation in the text.

Technical Corrections:

Referee comment:
p. 11006, l. 14: either "catchments' outlets" or "catchment outlets"
Reply:
We refer to the catchments' outlets.

Referee comment:
p. 11008, l. 12: Better write '…Stumpp et al. (2009b and 2012)…'
Reply:
We will rewrite this sentence.

Referee comment:
p. 11008, l. 20: Stable isotope(s) values.
Reply:
We will correct the expression.

Referee comment:
p. 11014, l. 15: 'melted' is the past participle of 'to melt'.
Reply:
We will correct the expression.

Referee comment:
p. 11016, l. 12: represent's'
Reply:
We will correct the expression.

Referee comment:
p. 11018, l. 2: …values 'are' reflecting…
Reply:
We will correct the expression.

Referee comment:
p. 11018, l. 22: …stored water(s)…
Reply:
We will correct the expression.

Referee comment:
p. 11018, l. 28: …these waters represent(s)…
Reply:
We will correct the expression.

Referee comment:
p. 11019, l. 27: Months are spelled with capital letters.
Reply:
We will correct the expression.

Referee comment:
p. 11024, l. 10: extenT
Reply:
We will correct the expression.

Referee comment:
p. 11024, l. 19: …higher than can BE expected…'
Reply:
We will correct the expression.

Referee comment:
p. 11029, l. 14: A 'time' is missing.
Reply:
We will add 'time'.

References added by the referee:

Botter G., E. Bertuzzo, and A. Rinaldo (2011), Catchment residence and travel time distributions: The master equation. Geophys. Res. Lett., 38, L11403, doi:10.1029/2011GL047666.

Heidbüchel, I., P. A. Troch, S. W. Lyon, and M. Weiler (2012), The master transit time distribution of variable flow systems, Water Resour. Res., 48, W06520, doi:10.1029/2011WR011293.

Hrachowitz, M., H. Savenije, T. A. Bogaard, D. Tetzlaff, and C. Soulsby (2012), What can flux tracking teach us about water age distributions and their temporal dynamics?, Hydrol. Earth Syst. Sci. Discuss., 9, 11363-11435, doi:10.5194/hessd-9-11363-2012.

Hrachowitz M., C. Soulsby, D. Tetzlaff, I.A. Malcolm, and G. Schoups (2010), Gamma distribution models for transit time estimation in catchments: Physical interpretation of parameters and implications for time-variant transit time assessment. Water Resour. Res., 46, W10536, doi:10.1029/2010WR009148.

van der Velde Y., G.H. de Rooij, J.C. Rozemeijer, F.C. van Geer, and H.P. Broers (2010), Nitrate response of a lowland catchment: On the relation between stream concentration and travel time distribution dynamics. Water Resour. Res., 46, W11534, doi:10.1029/2010WR009105.

References cited by the authors in this reply:

Asano, Y., Uchida, T., and Ohte, N.: Residence times and flow paths of water in steep unchannelled catchments, Tanakami, Japan, Journal of Hydrology, 261, 173-192, 10.1016/s0022-1694(02)00005-7, 2002.

Baumgartner, A., Reichel, E., and Weber, G.: Der Wasserhaushalt der Alpen: Niederschlag, Verdunstung, Abfluß u. Gletscherspende im Gesamtgebiet d. Alpen im Jahresdurchschnitt für d. Normalperiode 1931-1960; mit 68 Tab, Bd. 1, Oldenbourg, 1983.

Burns, D. A., and McDonnell, J. J.: Effects of a beaver pond on runoff processes: comparison of two headwater catchments, Journal of Hydrology, 205, 248-264, 10.1016/s0022-1694(98)00081-x, 1998.

Clark, I. D., and Fritz, P.: Environmental Isotopes in Hydrogeology, CRC Press/Lewis Publishers, 352 pp., 1997.

Darling, W. G., and Bath, A. H.: A stable isotope study of recharge processes in the english chalk Journal of Hydrology, 101, 31-46, 10.1016/0022-1694(88)90026-1, 1988.

Maloszewski, P., and Zuber, A.: Manual on lumped-parameter models used for the interpretation of environmental tracer data in groundwaters, in: Use of Isotopes for Analyses of Flow and Transport Dynamics in Groundwater Systems, edited by: Y, Y., Vienna, 1–50, 2002.

McDonnell, J. J., McGuire, K., Aggarwal, P., Beven, K. J., Biondi, D., Destouni, G., Dunn, S., James, A., Kirchner, J., Kraft, P., Lyon, S., Maloszewski, P., Newman, B., Pfister, L., Rinaldo, A., Rodhe, A., Sayama, T., Seibert, J., Solomon, K., Soulsby, C., Stewart, M., Tetzlaff, D., Tobin, C., Troch, P., Weiler, M., Western, A., Wörman, A., and Wrede, S.: How old is streamwater? Open questions in catchment transit time conceptualization, modelling and analysis, Hydrological Processes, 24, 1745-1754, 10.1002/hyp.7796, 2010.

McGuire, K. J., and McDonnell, J. J.: A review and evaluation of catchment transit time modeling, Journal of Hydrology, 330, 543-563, 10.1016/j.jhydrol.2006.04.020, 2006.

Soulsby, C., and Tetzlaff, D.: Towards simple approaches for mean residence time estimation in ungauged basins using tracers and soil distributions, Journal of Hydrology, 363, 60-74, 10.1016/j.jhydrol.2008.10.001, 2008.

Stumpp, C., Maloszewski, P., Stichler, W., and Fank, J.: Environmental isotope (δ18O) and hydrological data to assess water flow in unsaturated soils planted with different crops: Case study lysimeter station “Wagna” (Austria), Journal of Hydrology, 369, 198-208, 10.1016/j.jhydrol.2009.02.047, 2009.

Uhlenbrook, S., Frey, M., Leibundgut, C., and Maloszewski, P.: Hydrograph separations in a mesoscale mountainous basin at event and seasonal timescales, Water Resources Research, 38, 10.1029/2001wr000938, 2002.

Wenjie, L., Wenyao, L., Hongjian, L., Wenping, D., and Hongmei, L.: Runoff generation in small catchments under a native rain forest and a rubber plantation in Xishuangbanna, southwestern China, Water and Environment Journal, 25, 138-147, 10.1111/j.1747-6593.2009.00211.x, 2009.

Wittenberg, H.: Baseflow recession and recharge as nonlinear storage processes, Hydrological Processes, 13, 715-726, 10.1002/(sici)1099-1085(19990415)13:5<715::aid-hyp775>3.0.co;2-n, 1999.

Zuber, A.: Review of existing mathematical models for interpretation of tracer data in hydrology, Report 1270/AP of the Institute of Nuclear Physics, Cracow, in: Mathematical models for the interpretation of tracer data in groundwater hydrology, edited by: IAEA, 69-116, 1986.