Dear Markus Weiler,

Please find attached our responses and the revised manuscript of our paper.

At first, we included the additional request of you regarding the uncertainty, or the significant differences between the evaporation water lines at the end of the section 3.2. We used a two sample t-test and summarized the p-values in the new table 1.

Further, we followed the suggestions of the reviewers relatively close. Major changes included the revised figure 3 (adding trench flow), adding the riparian zones in figure 1 as hydric soils, adding the p-values for the regressions, and a more detailed description of the study area (especially about the depth to and the thickness of the argillic layer).

Best regards

Julian Klaus

Response to review of Markus Hrachowitz

We would like to thank Markus Hrachowitz for his helpful comments on the manuscript and the time he took for reviewing. As discussed in the interactive discussion in HESSD we think it is generally a great idea to use the proposed method over different system stages and investigate associated changes of flow paths.

Nevertheless, we have to keep in mind that the study period was mostly during drought conditions, i.e. most samples were taken during rather low flow conditions and dry catchment state. In the interactive discussion we presented the results when we split our isotopes samples in three different classes, based on the discharge at the sampling point.

The split data set, consists of a “high”, “mid”, and “low” flow conditions. These flow stages would not represent the full range of streamflow variability in the observation period, but is only based on the same number of discharge values and samples. In the following table we summarized the slopes, intercept, and coefficient of determination for the individual linear regression. A total of nine regressions are summarized, three classes for each of the three streams.

Table 1: Slope and Intercept of the EWLs of the R, B, and C stream, classified into three flow classes

Stream	Slope	Intercept	R2
R-stream low	1.3	-18.0	0.07
R-stream mid	2.2	-13.2	0.48
R-stream high	2.8	-10.0	0.60
B-stream low	2.9	-9.7	0.80
B-stream mid	2.2	-12.0	0.82
B-stream high	3.5	-6.7	0.86
C-stream low	3.0	-9.7	0.24
C-stream mid	2.3	-11.7	0.62
C-stream high	5.1	-0.4	0.70

There is a tendency that the samples during the higher flow class indeed showed the highest slope in the EWLs, indicating less enrichment, and thus stronger influence of the rainwater end-member. Nevertheless, the slopes for R and B are for every class relatively low, while the C stream showed a slope of above 5 for the highest flow class. This can indeed indicate changing contribution and relevance of sources of different wetness states (although the same pattern does not exist using antecedent precipitation), but this difference between the flow classes is only statistically significant for the C stream. Accounting for the few samples during higher discharge and the low statistical evidence, we don't think that adding this creates additional evidence, thus we decided not to include these results. Nevertheless, we think this is a great opportunity for future work, when the sufficient data is available to apply this idea.

Responses to Minor comments:

1) We changed it to "water sourcing in the hillslopes". We think that it is more clear now that it is about the contributions of these two end-member that can both enter the stream

2) Here I would disagree, the citations in the manuscript employ the use of dual isotopes (i.e. MWL and EWLs to infer water cycle processes), while the recommended paper uses both isotopes individually or combined for hydrograph separation. So it is, in my opinion, not related topic wise. To make this clearer we added "water lines based on dual isotopes" in the article.

3) Yes, average annual.

4) Corrected

5) It was done based on Priestley-Taylor (Rebel, 2004). Added to the revised manuscript.

6) The term "potential transpiration" was used in literature about the site and it is used for model considerations (see Rebel, 2004). The term is reflecting energy distribution between evaporation and transpiration. A quick search in Google scholar showed plenty of paper using the term. We left the manuscript unchanged at this point.

7) Thank you, it seemed that we forgot a part of this sentence. It should be "10% and 16% of open precipitation". We rephrased the sentences: "On six experimental plots throughfall was reduced by 10.1 to 16.4% compared to open precipitation". Yes, in this case interception is interception evaporation. We think the new formulation is clearer.

8) Sampling is ongoing. Precipitation was sampled from Feb 2007 until May 2012, R stream from April 2007, B and C from March 2010. The streams felt dry during May 2011. We revised the manuscript in section 2.2 to add this information and make it more clear why and how we limited the data unitl May 2012.

9) Six events in total, added to the revised manuscript

10) We did not take any measures like adding oil or Styrofoam particles in the collector. We used the suggestion from the comment of Lysette Munoz Villers and removed data points with very low deuterium excess from the data set (related to very low precipitation depths).

11) Yes, corrected

12) The wells were all located in the same strata. Groundwater depths was depended on surface elevation with about 10 m depth at higher surface elevations and as low as 1m during wet conditions in the C watershed. Isotopic composition was similar for most of the wells, for every well but one the average δ^{18}O-value lies less within one standard deviation to the mean. We simply added "all located in the same strata" to the description of the wells. Nevertheless, the one well that is outside the one standard deviation might already have some influence of an additional (deeper) groundwater system, since the screening is the deepest (25 m) of the sampled wells. But this remains still speculative, and was not added not the work.

13) We respectfully disagree at this point. Using the same range for the y axis is challenging since precipitation values in Deuterium stretch over 100 per mil, while the variations in the other compartments are clearly smaller. Using the same range will lead to barely visible variations of Deuterium in streamflow, trenchflow, and riparian groundwater and groundwater. Thus we did not change Figure 4.

14) I am not sure about this point. We never refer to the regression equations for any calculations, so I don't think the equations would need to be numbered. Thus we did not number the regression equations.

15) We agree that the p-value needs to be reported. We added the p-values to the revised manuscript.

16) Thanks for your thoughts on this. We agree that our data does not support our statement. We deleted the sentence, and just left the part with direct precipitation in this section.

17) Here we cannot follow the concern of the reviewer. This content is not related to the previous comments (16) of the reviewer which we fully support. Here we discuss alternative mechanism besides fractionation that could generate such shallow slopes to be sure that the observed EWLs are a consequence of evaporation. We slightly revised the relevant section to make it clearer.

18) Rain-fed: The wetland has no direct connection to groundwater and no stream inflow. The water is supplied by precipitation.

Response to Comments of L. Munoz Villers

The Klaus et al. manuscript presents the isotopic signatures of different water sources affected by evaporation, which are further used as end-members to identify runoff generation mechanisms in gentle slopes catchments in South Carolina, USA. I found the manuscript interesting because it provides new information about the water cycle o forests located in a low relief topographic setting. I have some comments that I would like the authors to address, which may help to improve the contents and potential impact of the paper.

Response: We would like to thank Lysette Munoz Villers for her helpful comments.

Methods. Section 2.2. I will second Dr. Markus Hrachowitz comment with regard to the measures taken to prevent evaporation/fractionation of the precipitation and throughfall samples (see also my comments below). Please provide this information in the manuscript.

Response: Regarding the measures to prevent evaporation and fractionation we attempted to go to the field directly the day after rain events to take the sample, often shortening the weekly sample interval. Nevertheless this was not always possible. We did not add oil or Styrofoam particles to prevent evaporation. Nevertheless, our observed LMWL is in agreement with observations throughout the region [slope of 7.04 and y-intercept of 8.11 ($R2=0.847$) for the period 1997 to 2009 (C. Romeneck, 2012, personal communication)]. This is suggesting little or no influence of fractionation on the sampling. Following a later comment, we removed 5 samples out of the precipitation data base based on their deuterium excess (excess<0 and weekly precipitation < 3 mm).

Section 2.2, Page. 2619. Please also mention the sampling period over which the stream water, riparian groundwater, throughfall, and lateral flow were collected.

Response: We added this to the revised manuscript.

Results. Section 3.1, Page 2620. The high positive O18 and 2H values reported for precipitation surprise me somewhat. In particular, there is one very enriched value that plots below the LMWL (Figure 4, panel a), which looks suspicious to me and makes me wonder if that sample could have been affected by evaporation. I would then suggest calculating the d-excess values (see my comment below) of all samples as a way to identify evaporatively impacted samples. In addition, please check the expected range for 18O and 2H isotope ratios at your study site or a nearby place. If the samples turn out to have been affected by evaporation, I suggest you to simply remove them from the data set.

Response: We completely agree with the reviewer that this is a suspicious sample with very low and unrealistic deuterium excess (that is also associated with low precipitation depth) is likely influenced by fractionation and should thus be omitted in the calculation of the LMWL. We used a simple quality check for the precipitation samples with a very low deuterium excess (Excess<0 and weekly precipitation <3mm). Samples falling in this range were removed from the analysis.

Section 3.2. Page 2620. I think it is very important to include in this paper a figure where the GMWL is plotted together with the LMWL; given the climatic conditions at your study site, the latter likely has a lower slope and intercept as compared to the GMWL. This would also be very useful for future comparisons across sites with similar climatic conditions.

Response: This is a good point. We added a figure including the GMWL and LMWL in the revised version to allow future comparison across sites with similar climatic conditions.

Further, I think the authors have a great opportunity to make use here of the Deuterium excess (d-excess) parameter as a measure of the degree of evaporation enrichment. D-excess is a measure of the relative proportions of O18 and 2H contained in water, and can be visually depicted as an index of deviation from the global meteoric water line (GMWL: d-excess =10) in 18O versus 2H space (Dansgaard, 1964). Hence, I encourage the authors to present in this section the d-excess values of the different water cycle components (stream, throughfall, etc.), and also to refer to them in the Discussion (Section 4.1).

Response: In general we agree that the Savannah River Site, or the Atlantic Coastal plain of the US, seems to be a good site to make use of deuterium excess. Nevertheless, we think that adding results and discussing on the impact of evaporation in detail via the use of deuterium excess would distract from the scope of this paper. The objective of the paper is the use of the dual isotope approach to constrain a conceptual model of runoff generation mechanisms and understand the runoff generation in the lower coastal plain. Thus we did not add additional work about deuterium excess.

Figures. Figure 3. Streamflow is plotted in l/s in log scale (left y-axis) in panels b, c and d; however, for consistency and to facilitate comparison with rainfall (panel a), I suggest to plot the streamflow in mm/d. I observed that panels c and d have the same scale in the right y-axis, but I am not sure if panel b does. If not, please correct.

Response: These are good points, and checked the axis. To be able to compare the trench response with the catchment response we left the unit in l/s.

Response to review of Kevin Devito

The authors take a novel approach to estimate sources of water to stream flow in low relief sub-humid (P<ET) catchments using established dual isotope methods. This study makes a significant contribution both in terms of the techniques to interpret and the conceptual understanding of runoff (or lack there of) from low gradient catchment in drier regions, which are, arguably, under represented in the published literature. I believe this paper should be published in HESSD. I propose some changes, mostly to provide more information on physiographic setting and some suggestions in aid of interpretation of the conceptual model. The paper is well written. The main assumptions of the techniques are addressed, and with respect to the conceptual model the distinction between speculation and actual data is reasonably clear. However, in using isotopic techniques equifinality (Buttle 1994) can make it difficult to make inferences about sources and especially flow path of water in complex systems, were complementary sources and process may contribute water. The paper also compares isotopes of N, and in conjunction with the dual isotopes provides a convincing argument. However, the interpretation and conceptual model also relies heavily on hydrometric data that was largely not presented and currently not accessible to the reader (Du et al in review). I echo the editor's comments about needing a little more quantification. At times, I am left asking how much water and when does it move.

Response: We want to thank Kevin for his comprehensive review that clearly helped identifying some short comings of the manuscript. We hope that our revised manuscript addresses these issues and especially give a clearer picture of the physiographic conditions.

1) There are potentially several conceptual models that could be applied to direct field sampling, interpret isotopic data and water cycling and sources to stream flow. Conceptualizing the x-section slope to the stream is often applied to steep systems with a confining layer where the contribution from hill slopes is contiguous along the stream reach. The x-section provided in Fig7 for the conceptual model seems reasonable, given the location of the depth of the argilic horizon presented in Figure 7. However, fig 7 is the only place in the paper were the reader receives information or can conceptualize the potential layering and depth of soils in the catchments. Only physical properties, not depth, are provided in study sites description. Providing general information on distribution of the thickness of the surface sandy layer (and depth to argillic horizon) in the hillslopes and draws, and a relative scale on the conceptual diagram would help the reader conceive and infer how much storage there is between the hilsllope and the stream, and how often this storage is potential filled and thus makes it to the stream? Also, providing the timing of flow from the plot study relative to the streams would provide some information on the time lags or threshold in storage.

Response: This is a very important point. In the work of Du et a.l, the depth to clay was measured in the hillslope of the R watershed on a 2*1 m grid and showed an average depth to clay of 0.76 m. Further the depth was measured at the further spots in R, B, C. We added this information to the manuscript, without going to much in detail. Regarding the connectivity/disconnectivity between landscape elements we included the hillslope runoff of the R watershed in figure 3.

2) The authors present two alternative models for flow in the catchment: the conceptual model in fig7 and that of saturation excess flow from the valleys. It appears that contributions from saturation excess surface flow are not a major mechanism, at least during the study period. In drier system where the hillslope is disconnected, there may be other alternate conceptualizations of water flow to exclude. The interpretation of the stream isotope data seems to be focused on the x-section right behind the weir. In fact it appears that the weir appears to be illustrated in the conceptual model (Fig 7). Understanding the distribution of the riparian zone and the stream-riparian-hillslope interaction is integral to conceptualizing the water cycling and interpreting the data. In low gradient systems with wide valleys, the riparian areas are not often narrow strips near a stream channel. Currently no information on the riparian zone, i.e the distribution or soil characteristics, has bee provided. Providing these may allow the reader to better visualize the potential catchment scale process presented and would address many of the questions presented above. A formal definition of what constitutes a riparian zone would help in extrapolating the conceptual model. Is the riparian zone defined to be adjacent to a well-developed stream channel eroded into the forest floor and mineral soils, or does it extend up into the ephemeral streams with no stream channel? Do the ephemeral streams have a defined channel? Do the riparian zones include the extensive valley wetlands, or are they defined as between the valley wetlands and the hillslope? Clearly define the wetlands (over and above CB) and what the riparian zone is in fig 1. In the study site description there appears to be distinct forest wetland vegetation that could be used to map out or infer the riparian area. Is there major changes in soil organic depth or other characteristics (ie holding capacity) or is it mostly slope that defines the riparian zone?

Response: The riparian zones/wetlands are wide, flat river valleys along the stream channels described in the study area. We added that we interpret them as riparian zone, we further added that hydric soils are in the stream valley and present them in the updated figure 1. This can give an idea on the extend of the riparian zone.

5) A glance at the hydro-geology literature indicates that the long-term vertical recharge through the argillic horizon could be provided for this site. This helps in visualizing the water balance. Also, in these low relief systems, there can be interactions of different scales of groundwater flow. From the "deeper well" data (locations on fig 1) presumably the general location and gradient of the water table relative to the three catchments can be presented (at least in the text). Do the valley bottoms of any of the catchments intersect the WT of the larger groundwater systems, and are the hill slopes perched when delivering flow? One or two lines can be inserted so the reader can make some assessment of the potential interactions and exclude a number of potential conceptual models of catchment-stream interaction.

Response: The reviewer is right in that vertical percolation through the argilic layer can occur. The layer shows anomalies with increased hydraulic conductivities. We added this to the study site description. Further, the deeper groundwater system is often 10 or more meters below the soil surface, nevertheless, one well in the C watershed and one downstream of B are located close to the riparian zone, and show water tables that are close to the surface during wet periods. We would argue that there is some interaction between the deep and the riparian groundwater. We added "The deeper groundwater table is often 10 and more meters below the surface, but can approach the soil surface (<1 m) in the riparian zone of the C" to section 3.1. In addition we added "Further, the deeper groundwater system can interact with the groundwater of the riparian zone during wet conditions" to the discussion section. This was generally our intention since we think from the isotope data, that deep GW is a major contributor to the riparian zone groundwater. We hope this can clarify the reviewers request. We also extended the conceptual figure by indicating the potential/likely influence of deeper groundwater on the riparian zone.

6) The wells are located on figure 1. The location of the riparian piezometers (relative to the distribution of the wetland/riparian area) should be indicated on the map (Fig 1). Also, the completion depth, length of screen, ad the soil layer they sample from should be presented.

Response: We added the riparian wells to figure 1. There is no detailed information available about the riparian piezometer. They are PVC pipes that reach 1-2 m in the soil. We left their description as shallow, but added that they sampled from the hydric soils.

7) page 9, ln 19. The EWLs for groundwater include 14 wells. It is not clear if these include riparian or stream areas, or they are all exclusively from the confined aquifer. Listing completion depths would clarify this.

Response: This is a good point, that also Markus Hrachowitz brought up in his review. See response on his minor comment 12. We added that the wells are located in the same strata.

8) page 10, ln20-25. The authors indicate that hillslopes can generate considerably more peak flow than the catchment outlet. It is implied that the water is evaporated/transpired. Could this go into deeper storage, and eventually recharged to groundwater?

Response: We absolutely agree. Evaporation/transpiration cannot fully account for the discrepancies. We extended (flowing and comment 1) the study stie description and added the anomalies of the argilic horizon to the description. These anomalies can lead to recharge through the argilic layer. We performed a hillslope irrigation experiment in the R watershed that also measured the leaching through the argilic horizon, but this data is not fully prepared yet and the manuscript not written. Nevertheless there was clear leaching through the argilic layer.

11) p11, ln 14-16. Could the valley wetlands above the weir also provide a mixed source of water?

Response: We think that the catchment morphology allows the simplified x-direction model. Nevertheless, this process is of course not limited to only the area around the stream gauge and can extent upstream in cases.

12) Citations not in the reference list: Page 3 ln 3: Sidle et al. 2000 – is not in the references, p3 ln5: Jensco et al. 2009 –is not in the references

Response: Thank you, we corrected this.

13) Figure 1; Note. Map out the riparian areas, location of piezometers. It could be my printer, but it seems that the contours and the intervals would not reproduce very well. Also, I have trouble distinguishing between the perennial and ephemeral streams.

Response: We updated figure 1.

14) Figure 3. There are periods of zero flow and periods with no data. It is difficult to tell which is which. If not already done, could zero flows be denoted with a symbol, and blank for no flow (some manipulation because of the log scale). Presenting the flow from the trench data and comparing with stream flow would provide information on the timing of potential storage and connectivity.

Response: We included the trench flow in figure 3, and updated figure 3 following the suggestions.