<u>**Author responses to reviewer comments on the manuscript entitled**</u>

"The influence of riparian evapotranspiration on stream hydrology and nitrogen retention in a subhumid Mediterranean catchment"

By Anna Lupon, Susana Bernal, Sílvia Poblador, Eugènia Martí and Francesc Sabater

Dear reviewers and Prof. Cristian Stamm (Editor of HESS),

Many thanks for you thoughtful review comments on the paper "The influence of riparian evapotranspiration on stream hydrology and nitrogen retention in a subhumid Mediterranean catchment" we submitted for publication to HESS. We feel thankful for your positive and constructive comments such as that "*this paper is straightforward and convincing*" or that "*the authors did a commendable job*". Moreover, we think that your comments and edits on the paper have been of great help to improve the quality and clarity of it.

We have taken into consideration all the comments highlighted by you and we are working thoroughly on a new version of the manuscript to tackle most of them. According to your comments, in the new version of the manuscript, we will explain in more detail sap-flux measurements, chemical data analysis and both riparian and stream characteristics. Moreover, we will clarify the interpretation of diurnal signals of stream discharge during the dormant period. Following your advice, we will also provide some explanation for the low evapotranspiration rates, and temperature will be included as a potential driver of in-stream nitrification. We will also tone down our conclusions regarding the effect of riparian evapotranspiration on stream nitrogen loads at annual scale. Finally, we will add the graphs requested by you as supplementary materials.

Below we provide the answer to each of your comments. In case of not following completely your suggestions, we have stated why. Please, do not hesitate to contact us if you considered that further clarifications are needed.

Looking forward hearing from you soon,

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cc: Susana Bernal, Sílvia Poblador, Eugènia Martí, Francesc Sabater

Editor comments

Detailed comments:

L.20: What is about the nitrogen budget? **Answer:** Unfortunately, we could not directly calculate the influence of riparian trees on catchment N budgets because tree N uptake was not measured.

L.35: What do you mean here (and elsewhere) with "reduce": changing the oxidative status of N compounds? **Answer:** We mean “diminish” or “lowered”. Changes in the text according to this have been done.

L.39: Why is the residence time large during wet conditions? **Answer:** We have clarified that high water residence time can occur when the water table rises up during wet conditions in flat riparian areas (Ranalli and Macalady, 2010).

L.57: Why should a losing streams show decreasing N concentrations in the stream? If you only have a losing stream you diminish the load but do not change the concentration in the stream unless other fate processes are affected (e.g., fraction of water exchanging between the hyporheic zone and the stream). Please explain. This is essential also for understanding your hypothesis (L. 70). **Answer:** In previous studies, decreases in stream N concentration along losing stream reaches have been attributed to high N uptake rates at the stream-riparian interface. We have added this statement in the manuscript.

L.67: "direction of water flow. . ." **Answer:** OK.

L.72: "paramount" seems slightly overstated to me. **Answer:** OK, change in the text has been included.

L.79-80: Is there any temporal trend in N deposition over the years or are these interannual variations? **Answer:** According to Àvila and Rodà (2012), bulk nitrogen deposition did not significantly varied from 1983 to 2007. Therefore, it does not seem that there is any temporal trend in N deposition over the past decades.

L.85: How do you define the riparian zone? Is it based on vegetation (species composition), pedology or terrain? Please explain. This seems also essential for the subsequent discussion (e.g., Fig. 6). **Answer:** Riparian zone is defined based on vegetation. We do now refer to “riparian forest” in order to clarify such definition through the manuscript.

L.86: In which direction do you measure the slope here? The steepest descent or perpendicular to the river? Slopes < 10% are not necessarily almost flat. **Answer:** Slopes were measured perpendicular to the river by using a theodolite. This procedure will be clarified in the manuscript.

L.87: The increase of the basal area is not clear to me, sorry. **Answer:** We will clarify the sentence indicating that the total basal area of riparian trees (based on tree diameter at breast height) increases by 12-fold along the study stream reach.

L.117-118: Please show the scatter plots for this regression in the Supplementary Material. **Answer:** OK, we will provide this information in the supplementary material.

L.123: How frequently were samples taken to the lab and processed? Where there any measures taken to prevent nitrification or any other changes of N forms? **Answer:** Samples collected with autosamplers were carried to the lab every 10 days. Autosamplers were installed about 1 m below ground for keeping water samples in a fresh environment with small changes in temperature. We have followed the same procedure in previous studies, and despite not taken any especial measure to prevent nitrification, we have never found substantial differences between grab samples and samples kept in the auto-samplers for this period of time. To illustrate that, here we compare chloride (conservative tracer), ammonium, and nitrate concentrations between samples collected by the two methods (grab vs autosamplers) (Figure R1). The samples collected with autosamplers were taken in the same day than the manual ones, but the former were then kept in the autosampler between 1-10 days. The good match between the two types of samples as well as the low relative root-mean-square error (< 3%) suggest that biogeochemical transformation was minimal within the autosampler bottles (Figure R1).

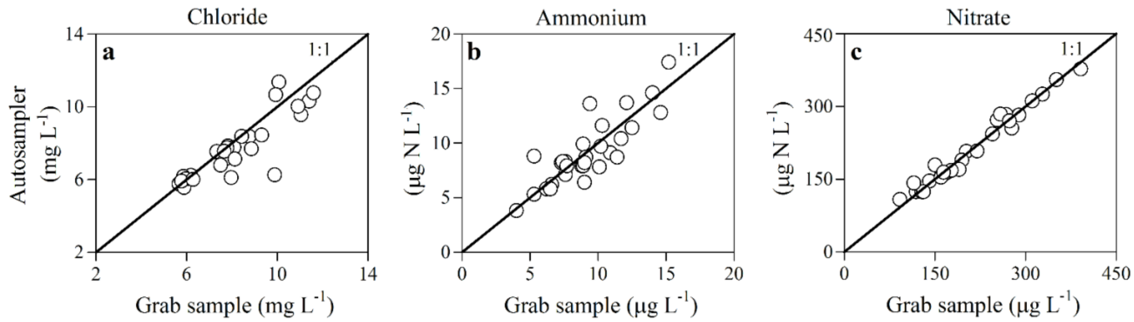

Figure R1. Comparison of stream water chemistry measured by grab samples vs autosamplers (Figure R1). The samples collected with autosamplers were taken in the same day than the manual ones, but the former were then kept in the autosampler between 1-10 days. Data is shown for (a) chloride, (b) ammonium and (c) nitrate. The line 1:1 is also shown. The relative root-mean-square error was 3.1, 2.7 and 1.1% for chloride, ammonium and nitrate concentrations, respectively.

L.126: Please indicate these four locations on the map. **Answer:** OK, locations included on the map.

L.147: How was sap flow measured? **Answer:** Sap flow was measured using constant thermal dissipation sensors (Granier, 1985). Each sensor consisted of two probes (10-20 mm long) inserted in the north-side of the trunk at breast height 10 cm apart. The upper probe was heated at constant temperature. The thermal difference between probes was scanned at 10 s intervals and recorded as 15 min average with a data-logger (CR1000, Campbell Inc.). Then, thermal differences were related to sap flux density (sap flow per unit of basal area; in dm^3 H_2O m^{-2} *BA* min^{-1}) following the original calibration of Granier (1985). Following your suggestion, we will include this brief description of sap flow measurements in the methods section.

L.154: What does n stand for? **Answer:** “n” stand for the number of months. We removed it from the manuscript to avoid confusions.

L.167: Please show actual data as scatter plots in the Supplementary Material. **Answer:** OK

L.177: I suppose the last term of the right hand side is added not subtracted. **Answer:** That’s right. Thank you for noticing.

L.182: Again, please show the actual data in the Supplementary Material. **Answer:** OK

L.188-190: Again, please show the actual data in the Supplementary Material. **Answer:** OK

L.252: A higher values implies lower water table levels, correct? **Answer:** Yes, we have clarified that.

L. 292: What are possible reasons for such low values? **Answer:** The truth is that we are not sure about it. One explanation could be because of low solar radiation (Aguilar et al. 2010). The study catchment is quite V-shaped and the riparian forest is in a topographically-shaded area. On average, solar radiation arriving to the riparian canopy is 36 ± 18 W m^{-2} d^{-1}; that is 80% less than in the open areas of the catchment (Poblador, unpublished data). This possible explanation will be included in the discussion.

L.301: What are there references used for Fig. 6? How large (percentage of catchment) are the riparian areas in the respective studies? **Answer:** References are now included as Supplementary material. Moreover, Table S2 now shows the percentage of the catchment covered by riparian areas, which ranges from 2% to 15%.

L.352 - 361 : I have problems to follow your argument: On L. 354 you state that the fluxes into and out of the valley reach during the vegetative period were similar and that nitrate export would have been about 15% higher without water lost to the riparian area (L. 357). During the dormant period, the nitrate fluxes are larger (about 18 mg N s^{-1}) without a change along the reach. Under the assumption that the dormant and vegetative period each last 6 months, this indicates that the effect size *is in the order of 5% of the annual nitrate export. Is this substantial?* **Answer**: That's right; on annual terms, the riparian zone does not have a big influence on catchment nitrate exports. We will rewrite this part of the discussion to clarify this message.

L.371: What about temperature and pH? Both are known to have an important influence on nitrification rates in streams and temperature will exhibit a pronounced seasonal pattern, I assume (e.g., Laursen & Seitzinger, 2004; Warwick, 1986). **Answer:** Following your advice, we will include in the manuscript that warm temperatures in summer could also stimulate in-stream mineralization and nitrification at the valley reach (Laursen and Seitzinger, 2004). However, we do not have evidences that stream pH changed over the year at the down-stream site (pH = 7.6 ± 0.3, n = 58; unpublished data); and therefore, we have decided not include this explanation, unless you consider it would add to the discussion.

L. 373: See comment above: strong regulation seems to strong an expression here. **Answer:** Thanks, we will tone down our conclusions in that regard.

Reviewer #1

Major concern:

The only major point is that there is no differentiation between winter and summer type diurnal signal therefore false calculation/interpretation of Q_lost in the dormant season. Q_lost estimation (used in paper) is good only for summer type signal. But in dormant season there is another so called winter type signal, which has a different shape and phase than summer type. The inducing effect of winter type signal is freezing and thawing not ET (see e.g. Lundquist and Cayan 2002, Gribovszki et al. 2010).

Answer: That's right; we completely agree on that. In temperate catchments there is usually a freezing and thawing diel signal characterized by a dawn minima and early afternoon maxima (Lunquist and Cayan, 2002). However, this type of signal does not usually occur in Mediterranean catchments, such as Font del Regàs, because there is no snow pack and soils are always > 0 ºC (except some days during particularly cold winters, which was not the case during the study period). Instead, we observed a tinny diel variation in stream discharge during winter, with maxima in early morning (3-6 am) and minima in early afternoon (2-5 pm). Similarly to the vegetative period, this type of signal is typically induced by tree evapotranspiration (Lunquist and Cayan, 2002). We agree with the reviewer that this signal cannot come from riparian evapotranspiration because riparian trees do not have leaves during winter time. Most likely, diel variations in winter result from the evapotranspiration of riparian understory vegetation (Roberts, 1983) and upland evergreen oaks (Savé et al., 1999). We will rewrite the methods and results sections to clearly explain (i) that there is no snow pack in our catchment and (ii) that winter diel signals probably result from riparian understory vegetation and hillslope evergreen oak forests.

Detailed comments:

Ln.15: subscript (lowercase). **Answer:** OK.

Ln.87: You have to define basal area here because this is the first mention in text. How can be basal area 22776 m^2/ha? Is this basal area the same as BA later defined in line 108? **Answer:** Yes, it was the same basal area. However, there was a mistake in the numbers, we apologize. We changed the sentence to define basal area and to clarify the message from these values (please, see our answer to Editor's comment, who also highlight this issue).

Ln.95. Longitudinal slope of reaches would be also informative. **Answer:** OK, we will add this information in the study site section.

Ln.100: It would be better to characterize h_{gw} not only for the valley reach, because as you mentioned in line 189 you have piezometers also along the headwater reach. Are there any differences between the water table levels or dynamic? Hydraulic conductivity values for streambed and riparian aquifer (if you have information about it) would also be informative. **Answer:**

The two reaches show similar riparian groundwater level. To illustrate that, here we compare the temporal pattern of near stream riparian groundwater (< 1.5 m from the stream channel) measured in 7 wells along each stream reach. Groundwater level was measured every 2 months from August 2010 to December 2011 as a part of a parallel study (Bernal et al. 2015). On average, groundwater level was 0.5 ± 0.1 m below the soil surface in both reaches (Figure R2). Following your advice, we will include this information in the study site section. Unfortunately, we did not measure hydraulic conductivity.

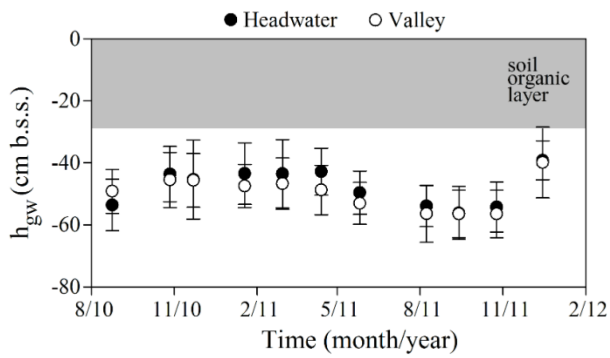

Figure R2. Temporal pattern of near stream groundwater level (h_{gw}) measured at the headwater and valley reaches during the period August 2010-December 2011. Circles are mean values for 7 wells located along each selected study reach, and whiskers denote the standard deviation. Both reaches showed similar temporal pattern (Wilcoxon paired rank sum test, $p > 0.1$, $n = 82$), being mean groundwater level 0.5 ± 1 cm below the soil surface.

Ln.107: BA needs to be defined where it is used first. See earlier comment. **Answer:** OK, we will include this definition in the text of the new version

Ln.115: Pressure transducer (instead of water sensor). **Answer:** OK.

Ln.122: Why had not you also installed another piezometer with a pressure transducer at the headwater reach (it is only a question of interest)? **Answer:** We agree that it would have been of a great help to install an additional piezometer at the headwater reach. We realized about that when the study was already ongoing. The study was initially designed to understand the influence of riparian zones on stream discharge and in particular of stream hydrological retention, and we decided to use the diel variation in stream discharge at the two reaches to estimate riparian ET. As the study evolved, we realized that it would be a great piece of information to show the close hydrological link between riparian groundwater and stream discharge by comparing the diel variation of discharge with the variation in riparian groundwater table. We then invited Sílvia Poblador to become part of the study because she was collecting high temporal resolution data of the riparian groundwater table at the valley bottom of the catchment as a part of her PhD thesis. Although we could only make these measurements for the valley reach, we think that it was an added value to show these data in the paper.

Ln.135: This Q_lost estimation is good only for summer type signal. But in dormant season there is another so called winter type signal, which has a different shape and phase than summer type. The inducing effect of winter type signal is freezing and thawing not ET (see e.g. Lundquist and Cayan 2002, Gribovszki et al. 2010). **Answer:** That's right, as we mentioned earlier in our response, these winter signal is not observed at Font del Regàs, probably because the winters are relatively mild and freezing only occurs in few limited dates. In fact, stream discharge at Font del Regàs exhibits a tinny winter signal with maxima in early morning and minima in early afternoon, which may correspond to evapotranspiration by understory and hillslope evergreen oak forests.

Ln.144: Why had not you calculated ripartian ET from diurnal signal of h_gw. There are a lot of methods available for calculation (see comparison of methods e.g. in Fahle and Dietrich 2014). **Answer:** As we mention in our earlier responses, the goal of our study was to evaluate the effect of riparian ET on stream discharge, in particular on stream hydrological retention. Variability in riparian groundwater level was not our central target. This is why we used the Cadol et al. (2012) method to calculate riparian ET from stream discharge rather than from groundwater level variations (e.g. White, 1932).

Furthermore, as we also mentioned earlier in this letter, we only had groundwater data for the valley reach, which would not allow us making direct comparison of riparian ET (measured from groundwater) between the two reaches.

Ln.197-199: I do not understand this sentence. Please clarify it. **Answer:** OK, the sentence will read as follow: "For $Q_{gw} > 0$ (net gaining stream), Obs:Pred ≠ 1 was interpreted as differences in riparian groundwater nutrient concentration between the headwater and valley reach".

Ln.228: Please take care of. It is probably a winter type signal. **Answer:** Many thanks for the advice. We have checked diel variations in discharge and, although being small, they show the shape and phase of ET induced signals. Please, see our earlier comments on this regard.

Ln.238: In dormant season (Jan-March) a winter type signal is typical, and it is not caused by T_rip (or ET_rip). **Answer:** That's true; evapotranspiration by riparian trees could not induce diel cycles of in-stream discharge in winter. Most likely, either riparian understory or evergreen oak forest is inducing such diel cycles in Font del Regàs catchment. Please, see our earlier comments on that regard.

Ln.343-351: Please, take into account that under 10ºC the nitrification is very slow. **Answer:** This is a good point. However, empirical results from a parallel study in Font del Regàs measured relatively high net nitrification rates (0.84 ± 0.23 mg N kg^{-1} day^{-1}, n = 36) in riparian soils during winter, when soil temperature ranged between 5-10 ºC (Lupon et al. 2016), suggesting that limitation of nitrification rates below 10ºC may not be so strong in the study area. These results support the idea that high rates of nitrification can promote N export from the riparian zone to the stream during the dormant season.

Reviewer #2

<u>Major concern:</u>

This paper seeks to determine the role of riparian vegetation on controlling duration and extent of stream recharge to near-stream aquifers, termed stream hydrological retention by the authors, and concomitant changes to forms of inorganic nitrogen. This is no easy task, as the processes involved are hard to link as they operate at different spatial and temporal scales. None the less, the authors have done a commendable job, providing enough correlative data to strongly suggest water table drawdowns are indeed induced by ET, which leads to increased stream hydrologic retention. That this would also be associated with increased rates of nitrification is novel. I recommend publication with hopefully minor revisions.

My most important concerns with the manuscript involve separation of the chemistry data to look at time periods that strictly align with periods of net discharge losses (unless I've misread how the data were grouped), and the presentation of the methods with respect to the ET determinations.

Answer: Many thanks for your positive and constructive comments of the paper. Regarding how data was analyzed, only discharge and solute concentrations during base flow conditions (i.e., when changes in discharge were < 10% in 24 h) were included in the analysis. The same data set was used to investigate differences between the vegetative and dormant periods (i.e. Wilcoxon rank sum test) as well as for the mass balance approach. We will work to make this information clear through the manuscript.

Furthermore, and following both the reviewer and editor suggestion, we will include more information regarding how sap-flux measures were carried out in the field. Please, see our responses to the editor, who also highlighted this issue.

<u>Detailed comments:</u>

Study Area: I'm confused as to how the valley reach drained less of the catchment area than the headwater reach. Figure 1 indicates the sites are both located on the main stem of the river, which should mean the total catchment area being drained *at any point along the stream increases as you move downstream.* **Answer:** That's right; what we want to say is that the valley reach drains an additional 4.42 km^2 on top of the drainage area of the headwater reach (i.e. 11.16 km^2 in total). We will clarify this in the study site section.

Results Section 4.3: I'm not sure the approach presented here is the most fruitful. Lumping the whole dataset together for each sampling point to compare means in phases of the year probably confounds the interpretation. The authors have already removed storm flow data from their Q analysis, would it not make sense to also do that for the solute analysis? Surely the few rain events during the vegetative periods will lead to unique N & Cl responses than what the authors are striving towards; that is, N flushing as Qgw becomes more positive. Why not try to look only at solute differences during base flow? **Answer:** That's right, we completely agree. Indeed, to avoid any interference from stormflow data, we only used solute concentrations measured during base flow conditions when exploring differences between the two periods as well as when calculating mass balance approaches. We will clarify this procedure in the methods section.

Discussion Section 5.2: The authors point out the previous literature on losing reaches has found net nitrate removal from the stream water. It might be worth mentioning here that net nitrification leading to ammonium losses and nitrate increases in other stream type are not uncommon (Triska et al., 1990, 1993 for early data). This highlights the importance of in-stream (in-hyporheic zone) N transformations that would be (mostly) disconnected from whether the stream is gaining or losing water to the riparian zone. **Answer:** This is an interesting point; thanks for highlighting this issue. For instance, both Triska et al. (1993) and Dent et al. (2007) reported an increase in nitrate concentration along the stream despite measuring high denitrification rates at the stream-riparian interface. In both cases, such increase in nitrate concentration was attributed to in-stream (or hyporheic) nitrification. Ultimately, these results suggest that processes occurring within the stream or hyporheic zone may overwhelm those occurring within the stream-riparian interface, especially during the period of high hydrological retention. We will include this idea in the discussion section.

L35: change "relays" to "relies" **Answer:** OK.

L58-60: The tone set here is too negative to their purposes. "There is little empirical evidence" sounds like people have studied riparian ET – nitrogen cycling before and not found any linkage. I think the authors are trying to say that there has been very little investigation into this linkage. **Answer:** OK, the sentence will now read as follows: "there has been <u>little empirical investigation focused on</u> the influence of riparian ET on upland-riparian-stream hydrological exchange".

L106: were the forest inventories done as straight-line transects of 30-m length, or were they plots 30-m long, perhaps also by 30-m wide? **Answer:** Plots were 30 m long and their width varied from 5-20 m depending on the width of the bankfull area. We will clarify this in the manuscript.

L108: the upper case version of pi is used in the basal area calculation, rather than the lower case. **Answer:** OK.

L115: insert "transducer" or "sensor" after "water pressure." **Answer:** OK.

L121: more information is necessary on the piezometer. Was it a piezometer or a well? Wells are slotted throughout their length and measure groundwater level. Piezometers are only perforated for some specific interval (less than its entire length) and measure hydraulic head at that specific depth (which may differ quite a bit from the water table). **Answer:** Yes thanks for noticing. We have indicated throughout the manuscript that we installed wells.

L152-153: I feel more information on the tree transpiration / sap flow measurements is needed. I realize they were taken from the Nadal-Sala et al. (2013) study; however, it seems pretty central to the present paper and the reader should not have to go to another source for so crucial a measurement technique. **Answer:** Following the reviewer suggestion, we will provide more information regarding sap flow measurements. Please, see our responses to the editor, who also highlighted this issue.

L250-251: Figure 2c refutes this statement. There appear to be at least a few days around January / February, 2012 with Qgw < 0 in the valley reach. **Answer:** The reviewer is totally right; thanks for noticing. During the dormant period, days with Q_{gw} < 0 were nil for the headwater reach and infrequent (< 3% of the time) for the valley reach. We will change results and Table 2 accordingly.

L334: Change "suite" to "suit." **Answer:** OK.

L366: Change "stronly" to "strongly." **Answer:** OK.

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