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Hydrology and Earth System Sciences Discussions

Interactive comment on "Investigating patterns and controls of groundwater up-welling in a lowland river by combining fibre-optic distributed temperature sensing with observations of vertical head gradients" *by* S. Krause et al.

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Dear reviewers & editor,

We would like to thank the reviewers for highlighting the originality and novelty of the presented work and providing very valuable, constructive and helpful comments to our manuscript. Please find enclosed our reply to the reviewer comments including an explanation of the intended changes to the manuscript.

We were delighted to learn that all three reviewers found our manuscript including the

development and application of the combined methodological framework innovative and exciting.

Apart from inconsistencies in notations and mentioned monitoring dates and periods in the manuscript, two reviewers suggested including a more detailed discussion of loosing conditions as well as streamlining the discussion and conclusions by also highlighting the wider implications of the findings of this paper.

We thoroughly checked the revised manuscript for inconsistencies in notation and dates of sampling times and adjusted these were necessary according to the reviewer suggestions. The discussion section has been revised in order to accommodate a discussion of the wider implication of the proposed methodological framework and its application in particular for the analysis of exchange flow patterns and biogeochemical cycling at aquifer-river interfaces. We emphasized the importance of these implications by more intensively referring to state of the art and recent publications in the discussion section. This revision of the discussion section similarly affected the focus of the conclusions as outlined in more detail in our replies specific reviewer comments below.

In regard to the requested of extension of the discussion towards loosing conditions as suggested by some of the reviewers, we aimed to emphasize in the revised manuscript that this paper is testing an approach for gaining streams as for the observation period there is no evidence of loosing conditions. Groundwater was overall up-welling during all DTS surveys. Locally inversed head gradients between groundwater and surface water were limited to short peak flow periods when neither DTS nor VHG were sampled. VHG observations carried out as soon as possible after the recession of the 31.07. peak flow event (after recession of the stream stage and the water course was accessible again) did not exhibit any negative VHG, hence did not provide any evidence for surface water down-welling or loosing conditions. This behaviour has been interpreted as indicator of the probably very short term and localised nature of exchange flow inversion (from gaining to loosing). Thus, there is no basis for interpretation of loosing conditions and we can only speculate about the conditions we would expect to

find in loosing streams – which is done in section 4.3.3. where we state:

“For example, in case of a river with similar groundwater-surface water thermal gradients, a low streambed permeability with only a few locations where the stream was losing water, temperature anomalies might be warm (in summer) combined with a higher variability of the strength of the anomaly. Temperature fluctuations in this case would not only result 15 from conduction but also from advection, and the temperature signal would, hence be controlled by (a) the surface water temperature variability and (b) the fluctuation in hydraulic gradients).”

Thus, we emphasize even more in the discussion of the revised paper that our findings are representative for gaining conditions. FO-DTS has mainly been applied in gaining conditions only. Future research will need to identify the potential for combined FO-DTS and VHG observations in loosing streams as FO-DTS application alone is usually not conclusive. However, this goes far beyond the scope of this paper – hence, while we provided a hypothesis for possible observations in loosing conditions but any further speculations would not be justified by the data of our study.

Again, we wish to thank the reviewers and editor for their useful comments which significantly helped to improve our paper. Please find our detailed replies to the reviewer comments below.

Kind regards

Stefan Krause Theresa Blume Nigel Cassidy

RESPONSE TO REVIEWER COMMENTS

Anonymous Referee #1 Received and published: 15 February 2012 General comments This manuscript aims to use 2 complementary measurement techniques to better understand spatial and temporal dynamics in surface water-groundwater exchange in a lowland river. The installed piezometers give information about the direction and strength of the vertical hydraulic gradient (VHG) while FO-DTS gives information about

locations of upwelling water. Because vertical hydraulic gradients can be caused by either a low hydraulic conductivity or by a large flux, it gives no conclusive information about upwelling water. FO-DTS gives information about upwelling water, but it does not give information about the structure causing the upwelling. The authors show nicely that by combining the two methods the results are more conclusive. This is novel and therefore worth publishing.

However, improvements should be made especially in the writing part: Many things are inconsistent within the manuscript. For example, In Eq (3) the authors define sd(AT (xi)), while in the second half of the manuscript they call it STDEV of temperature anomalies. Another example is that they mentioned that they did fieldwork between June and August 2009, but in Table 1 it is shown that at 21/5 they were also in the field. See below for more examples. In principle each of these flaws are minor, but in my opinion these are so abundant, that it adds up to more than minor revisions. Also many long sentences are used that makes it sometimes difficult to understand the sentence in one reading. Splitting the sentence in 2 separate ones will improve the readability. See below for some concrete examples.

Author comments: We thoroughly revised the manuscript according to the reviewer comments to adjust the mentioned inconsistencies. Please also see our comments to the specific remarks below.

Another point I miss is that no analysis is performed on loosing stream conditions. In 4.3.3 the authors state that this may be interesting to look at, but they did not do it although they had the data for this during and after the heavy rainfall at the end of June.

Author comments: We aimed to emphasize in the revised manuscript that this paper is testing an approach for gaining streams as for the observation period there is no evidence of loosing conditions. Groundwater was overall up-welling during all DTS surveys. Locally inversed head gradients between groundwater and surface water were

limited to short peak flow periods when neither DTS nor VHG were sampled. VHG observations carried out as soon as possible after the recession of the 31.07. peak flow event (after recession of the stream stage and the water course was accessible again) did not exhibit any negative VHG, hence did not provide any evidence for surface water down-welling or loosing conditions. This behaviour has been interpreted as indicator of the probably very short term and localised nature of exchange flow inversion (from gaining to loosing). Thus, there is no basis for interpretation of loosing conditions and we can only speculate about the conditions we would expect to find in loosing streams – which is done in section 4.3.3. where we state: "For example, in case of a river with similar groundwater-surface water thermal gradients, a low streambed permeability with only a few locations where the stream was losing water, temperature anomalies might be warm (in summer) combined with a higher variability of the strength of the anomaly. Temperature fluctuations in this case would not only result 15 from conduction but also from advection, and the temperature signal would, hence be controlled by (a) the surface water temperature variability and (b) the fluctuation in hydraulic gradients)." Thus, we emphasize even more in the discussion of the revised paper that our findings are representative for gaining conditions. FO-DTS has mainly been applied in gaining conditions only. Future research will need to identify the potential for combined FO-DTS and VHG observations in loosing streams as FO-DTS application alone is usually not conclusive. However, this goes far beyond the scope of this paper – hence, while we provided a hypothesis for possible observations in loosing conditions but any further speculations would not be justified by the data of our study.

Specific comments P338: Abstract: Make clearer why FO-DTS is not sufficient to explain surface watergroundwater exchange. Now it is only clear why VHG measurements are not enough as a single measurment.

Author comments: A more detailed discussion of DTS limitations for explaining groundwater-surface water exchange is provided in the revised manuscript. In it, we particularly stress that, while FO-DTS monitored streambed temperature anomalies

can help to interpret spatial patterns of groundwater up-welling, they are limited in their efficiency to identify surface water down-welling (see comment above) and do not provide explanation on the causes of heterogeneity in groundwater up-welling without comparison to VHG (or other observations of streambed structural architecture, e.g. geophysical profiles).

P339 L14: In the first half of the manuscript (i) and (ii) are used as counters, while in the second half (a) and (b) are used. Please be consistent throughout the manuscript.

Author comments: This has been adjusted in the revised manuscript.

P340 L21-26: This part does not fit in the Motivation section. It is the state of the art.

Author comments: We rephrased this to make clear that the ability of the named methods to adequately reflect groundwater-surface water exchange fluxes forms part of the motivation of this study and the application of heat tracers in particular – which leads over to the following section.

P342 L8: Hoes et al. 2009, also uses FO-DTS to identify upwelling water in large water bodies.

Author comments: Reference to this work has been included in the revised manuscript.

P344 L19-21: It is difficult to understand this sentence: please rephrase.

Author comments: The sentence has been rephrased in the revised manuscript.

P344 L22: How far is the Keele meteorological station from the site?

Author comments: Information on the distance to the field site (18 km) is provided in table 1.

P345 L7: Make clear that the seven tubes are only used to see if water could be extracted from a certain depth, and that this data is used to verify! if upwelling water is indeed blocked by a peat or clay layer.

Author comments: We clarified in the revised manuscript that this is what they have been used for in this work but also for chemical analysis of pore-water samples in other publications.

P345 L22: The Raman stokes are nearly temperature independent.

Author comments: We rephrased this sentence and discussed the Stokes/anti-Stokes ratio in the revised version.

P345 L25: Is the 0.05 _C the precision you obtained or is it given by the manufacturer? With a time interval of 10 s (as mentioned on page 346) I assume the precision is lower.

Author comments: We clarified in the revised manuscript that these are manufacturer provided precision values for 30 s sampling intervals. We also discussed that although we sampled for 10 s intervals, these were averaged over longer time periods.

P345 L26: Mention the temporal resolution here as well (instead of on the next page).

Author comments: Adjusted as suggested

P347 L21: Is the spatial average an average over all temperature points along the part of the cable that is in the water?

Author comments: Along the cable that is deployed in the streambed

P347 L26: T(xi) should be T(x). Otherwise it can only be an average over time.

Author comments: Has been adjusted as suggested.

P348 L4: From this point on (a) and (b) are used instead of (i) and (ii). Please be consistent.

Author comments: Has been adjusted as suggested.

P348 L4-5: Changes in hydraulic gradient does not always imply larger fluxes. If the upwelling water is blocked by peat or clay layers this will not be the case.

Author comments: Indeed – we rephrased this part with only ‘changes in water fluxes’ remaining.

P348 L5: ‘Temperature gradient’ should be a ‘difference in temperature’, since a gradient implies dividing by e.g. a distance.

Author comments: Has been adjusted as suggested.

P348 L9: same as P347 L26.

Author comments: Has been adjusted as suggested.

P348 L13: Explain what (xit) is and use Ak instead of AT and clarify that subscript k means either temperature or VHG.

Author comments: Should xit be xi ? If so – they are measurement locations along the cable as explained just above. Instead of introducing Ak as suggested we clarified in the revised manuscript that the equation provided for AT in 3. Is similarly applied to AVHG.

P349 L2-5: This is another sentence with which I had difficulties to understand. Please rephrase or make 2 sentences out of it.

Author comments: Changed as suggested.

P349 L23-24: Temperatures were indeed higher at the end of the observation period, but no maximum is present yet. The conclusion that the time lag is several weeks is thus a bit premature, since it can also be a couple of months.

Author comments: We adjusted this as suggested and rephrased to make sure that the time lag is at least several weeks.

P350 L13: Again: the 5_C is a temperature difference and not a gradient.

Author comments: Adjusted as suggested.

P350 L14: If the temperature difference exceeds 5_C most of the time, is the average

of 3.1_C (mentioned in L3) correct?

Author comments: Has been corrected as suggested in the revised manuscript.

P350 L20: Figure 6 is only partially supporting the mentioned range between 0 and 0.92, since it only shows gradients between 0.05 and 0.6.

Author comments: We clarified in the revised manuscript that Figure 6 shows averaged VHG but the mentioned VHG of > 0.9 referred to actual maximum levels at a certain observation date.

P351 L25: Do you mean P24-P26? P27 is further away from the spatial mean.

Author comments: Has been adjusted as suggested.

P352 L1: Is the spatial temperature variability of 2.3 degrees the difference between the boxplots at 0m and _130 m? This statement can only be true if both temperatures were measured at the same moment. Otherwise it is a spatial and temporal variability.

Author comments: Has been corrected for spatial and temporal variability.

P352 L6: Is STDEV the same as Eq (3)? Be consistent with the symbols!

Author comments: Has been revised as suggested.

P352 L7: Are the mean and stdev in Fig 9 the same points as the lines in Fig 8?

Author comments: They refer to the same data.

P353 L14: Change sentence to: Observed groundwater levels generally exceeded . . ..

Author comments: Changed as suggested

P353 L14-25: A Large part of this paragraph is results and should go to the result section.

Author comments: The paragraph presents interpretations of the results. Hence we

decided to keep it in the discussion.

P354 L2: There is not only upwelling water during the study period. But Fig 6 shows temporal averages which are indeed upwelling.

Author comments: We state that VHG observations indicate up-welling only – which they do. There is evidence by automatically monitored head observations that indicate possible flow inversion during a short peak flow period – but VHG observations directly after flow recession when access of the stream was possible again did not show any negative VHG. Hence, when referring to observed VHG patterns we can only identify up-welling. We aimed to highlight in the revised manuscript that short term conditions during peak flow could not be covered by VHG observations, preventing any conclusions about the spatial and temporal extend of possible down-welling.

P354 L7: I would remove the reference to Fig 2.

Author comments: We removed the reference to Fig. 2 as suggested.

P354 L11: replace ‘for instance, as’ with ‘because’

Author comments: ‘For instance’ refers to the previously mentioned faults and fissures. We therefore left it but replaced the ‘as’ with ‘because’ as suggested by the reviewer.

P357 section 4.3: Here the authors make clear why FO-DTS measurements alone are not enough. However, the reason why VHG is not enough as a standalone measurement is mentioned in section 4.1. For consistency, either mention them both in 4.3 or mention the FO-DTS shortcomings in 4.2.

Author comments: We changed as suggested and included the information of VHG limitations in 4.3 as well.

P356 L1: Where does the 0.05_C come from?

Author comments: From a roaming survey at the streambed surface – information has been added to the revised manuscript.

P356 L12-14: I suggest replacing ‘Furthermore . . . groundwater inflow’ with ‘Furthermore, since the temperature anomaly patterns prove to be temporally persistent, it can be assumed that groundwater inflow is also persistent’

Author comments: Rephrased in revised manuscript.

P356 L19: What is topography driven temperature variability?

Author comments: Temperature variability caused by spatial patterns of streambed topography as mentioned earlier in the text and referred to Krause et al., 2011 Ecohydrology observations. We included a reference to the same content at this location of the revised manuscript as well.

P356 L24-27: This part fits better in section 2.1.

Author comments: We discuss at this point the sensitivity and robustness of the approach and therefore decided to keep the discussion of L24-L27 together with the content before to ensure the internal consistency of argumentation.

P357 Section 4.3.1: I suggest to add a table or a point by point list to better represent the 4 different cases: small VHG and small dT; large VHG and small dT; small VHG and large dT; and large VHG and large dT and explain what the flux and structure should look like. That will make it easier for the reader to recall the different cases when they are discussed in the following sections.

Author comments: We included the VHG vs. dT information for the different cases in brackets as suggested and introduced a list of bullet points for the presentation of the case distinction as suggested.

P358 L3-6: This is a repetition of 4.3.1. and can be left out.

Author comments: L3-6 provide the first example for the application of the framework – they are not repeating 4.3.1 and therefore have been decided to be left in.

P358 L14: replace‘. . . in piezometers P5, P9 etc.’ with ‘. . . in these piezometers’

Author comments: Changed as suggested.

P359 L10: Isn’t the stream loosing during peak flow at 31/7?

Author comments: According to observed heads at one location in the surface water and several groundwater observation boreholes there is indication for flow inversion and local loosing conditions for a short period at the end of July. However, VHG observations in the streambed at 31.07., immediately recession from peak flow conditions did not confirm any surface water downwelling, indicating that short term surface water losses were likely to be reduced to localised shallow bank storage.

P359 L10-14: This sentence is difficult to understand: Does this example refer to winter conditions (although summer is mentioned here as well)? And are ‘similar groundwater- surface water thermal gradients’ similar to winter conditions or to the observed gradients?

Author comments: It refers to summer conditions in a loosing river as specified in the second half of the sentence: “temperature anomalies might be warm (in summer) combined with a higher variability of the strength of the anomaly”. The sentence has been rephrased (bringing the summer information upfront) in order to improve the understanding.

P366: The reference to Selker et al., 2006b is missing

Author comments: Has been included in revised manuscript.

Table 1: Piezometer IDs are P1, P2 and no T1 and T2.

Author comments: Has been adjusted in the table.

Table 1: Be careful with the ‘–’ sign, since it can be interpreted as a minus sign. I suggest using the symbols as mentioned in Eq 1 and 2.

Author comments: We assume the comment refers to the column headings? As in the table the ‘-‘ does indeed intend to depict a minus sign. We included Eq 1 + 2 symbols

in the column headings as suggested.

Table 1: What does '(nearest)' mean in 'DTS – difference to spatial mean'?

Author comments: It refers to the DTS measurement that is nearest to the piezometer where VHG has been observed – this has been explained more detailed in the revised manuscript.

Fig 1: What are the dotted lines close to GW1 and GW10?

Author comments: They represent the boundaries of the focussed study of topography induced hyporheic exchange flow and resulting thermal patterns we refer to in the manuscript as potential alternative cause of streambed temperature variation. Additional explanation has been provided in the revised manuscript.

Fig 2: How many cores have been used to draw the conceptual model?

Author comments: The conceptual model has been derived by cores taken from the streambed and riparian floodplain (total of 15) as well as observations on the multi-level mini-piezometers where inability of porewater extraction has been interpreted as presence of low conductivity clay and peat layers at the specific strata. More detailed explanation has been given in the revised manuscript.

Fig 3: The DTS has no sampling points. It gives spatial averages over 2 m (this is a result of the length (in terms of time) of the laser pulse send through the fiber.

Author comments: We adjusted the text as recommended.

Fig 4B: I suggest showing the precipitation in mm/d, so it is easier for the reader to see the total amounts.

Author comments: We decided to stay with mm/hr which represents a standard unit for precipitation. When looking at mm/d precipitation intensities tended to be smoothed out with high intensities potentially being masked.

Fig 8: Is stdev the same as STDEV and Eq 3? Again, be consistent.

Author comments: Has been adjusted throughout the revised manuscript as suggested.

Fig 8B: Indicate the relative location of the cable by either adding locations of piezometers at the x-axis or by adding distances in Fig 3B.

Author comments: We endeavour to discuss the statistical measures of both datasets in comparison in figure 9, particularly c-f. Figure 8 provides a detailed discussion of the statistics of each dataset on its own. We aimed to emphasise this clearer in the revised manuscript as well as highlighting that the requested comparison is provided in the next figure, figure 9.

Technical corrections P341 L28: 'cause measurable in temperatures', should be 'cause measurable anomalies in temperature'

Author comments: Has been adjusted as suggested.

P344 L17: 'Fig 2' should be 'Fig 2a'

Author comments: Has been adjusted as suggested.

P344 L19: In table 1 and on the next page it is shown that already on 21/5 field data has been collected.

Author comments: Has been adjusted in the text.

P346 L19: According to Table 1, the surveys were carried out between 23/7 and 19/8.

Author comments: Has been adjusted in the text.

P346 L26: Fig 7 should be Fig 6.

Author comments: Has been adjusted as suggested.

P347 L27: locations should be location

Author comments: Has been adjusted as suggested.

P348 L3: as result should be as a result.

Author comments: Has been adjusted as suggested.

P348 L10: piezometers should be piezometer.

Author comments: Has been adjusted as suggested.

P354 L19: replace 'sections – P1-3 + P25-27'with 'sections: P1-3 and P25-27'

Author comments: Has been adjusted as suggested.

P356 L1: remove '>'

Author comments: Has been adjusted as suggested.

P358 L23: indicated should be indicating

Author comments: Has been adjusted as suggested.

P359 L15: move 'hence' in-between 'and'and 'the'.

Author comments: Has been adjusted as suggested.

P358 L17: remove the parenthesis.

Author comments: Has been adjusted as suggested.

Figure 2: Change 'conceptual' into 'perceptual'?

Author comments: We feel 'conceptual' is the better fit.

References: Hoes, O. A. C., Luxemburg, W. M. J., Westhof, M. C., van de Giesen, N. C., Selker, J., 2009. Identifying seepage in ditches and canals in ploders in The Netherlands by distributed temperature sensing. Lowland Technology International 11 (2), 21–26.

Anonymous Referee #2 Received and published: 22 February 2012 General Comments The manuscript presents an analysis of spatially variable groundwater contributions to a lowland river. By combining distributed temperature sensing of hyporheic zone water temperatures with local observations of vertical hydraulic gradients between groundwater and surface water the authors could distinguish vertical hydraulic gradients caused either by upwelling groundwater originating from regional groundwater gradients or by local heterogeneities of streambed conductivities. The analysis presented in this paper is novel and will be a valuable contribution to the hydrological community. The manuscript is technically sound, the methods are fully described and the mathematical development is complete. Overall, the manuscript is well-written (reviewer 1 highlighted the importance of consistency already), but there are several points that require more clarification prior to publication.

As suggested by Reviewer 1 the authors should provide an analysis of losing stream conditions or at least discuss which phenomena they were able to observe in their data sets during the rainfall event at the end of July. How would you define the four CASES during losing conditions?

Author comments: Please see our comment to reviewer 1 as well: We aimed to emphasize in the revised manuscript that this paper is testing an approach for gaining streams as for the observation period there is no evidence of loosing conditions. Groundwater was overall up-welling during all DTS surveys. Locally inversed head gradients between groundwater and surface water were limited to short peak flow periods when neither DTS nor VHG were sampled. VHG observations carried out as soon as possible after the recession of the 31.07. peak flow event (after recession of the stream stage and the water course was accessible again) did not exhibit any negative VHG, hence did not provide any evidence for surface water down-welling or loosing conditions. This behaviour has been interpreted as indicator of the probably very short term and localised nature of exchange flow inversion (from gaining to loosing). Thus, there is no basis for interpretation of loosing conditions and we can only speculate about the

conditions we would expect to find in loosing streams – which is done in section 4.3.3. where we state:

"For example, in case of a river with similar groundwater-surface water thermal gradients, a low streambed permeability with only a few locations where the stream was losing water, temperature anomalies might be warm (in summer) combined with a higher variability of the strength of the anomaly. Temperature fluctuations in this case would not only result 15 from conduction but also from advection, and the temperature signal would, hence be controlled by (a) the surface water temperature variability and (b) the fluctuation in hydraulic gradients)."

Thus, we emphasize even more in the discussion of the revised paper that our findings are representative for gaining conditions. FO-DTS has mainly been applied in gaining conditions only. Future research will need to identify the potential for combined FO-DTS and VHG observations in loosing streams as FO-DTS application alone is usually not conclusive. However, this goes far beyond the scope of this paper – hence, while we provided a hypothesis for possible observations in loosing conditions but any further speculations would not be justified by the data of our study.

In Figure 1 the GW flow direction is indicated using gray arrows. When comparing the locations where upwelling groundwater was actually observed (CASES 1 and 4: P 1,2,3,4,8,12,25,26,27) with the groundwater flow direction in Figure 1, it seems, that groundwater is up-welling more or less perpendicular to the groundwater flow direction. The insertion of groundwater iso-lines in Figure 1 could help to highlight or to clarify this question. The relation between the spatial distribution of distinct areas, where the river is effectively gaining water and the orientation of groundwater table gradients should be analyzed in the discussion.

Author comments: As suggested by the reviewer the arrows indicating general groundwater flow direction have been removed from Figure 1. We decided against replacing them by groundwater contours as this critically overloaded the figure and a discussion

of the overall groundwater flow field in the riparian zone is not part of this paper.

In the Introduction (P.340 L. 14-20), the authors pointed out the importance of aquifer-river exchange in lowland rivers for nutrient export. I think, the discussion should be conveying the potential implications of the study findings to better quantify nutrient fluxes, in particular, in the context of the spatial variability of locations with groundwater up-welling and the spatial orientation of regional groundwater flux.

Author comments: We revised the discussion to reflect more on the potential implications of nutrient fluxes and processing at aquifer-river interfaces as suggested by the reviewers.

Specific comments: - In the abstract I am missing a sentence regarding the implications of your findings

Author comments: In the abstract we emphasize the implications of our findings for identifying exchange flow patterns and controls of exchange fluxes. We intensified the discussion of wider implications of these findings in the discussion section and also included a sentence in the abstract.

- P. 345 line 5 Where do you define PTFE piezometers. This is the only sentence where this abbreviation is mentioned.

Author comments: Provided an explanation of the abbreviation in the revised manuscript as suggested.

- P.349, line 22 I guess you meant Figure 4 not Figure 2

Author comments: Indeed. Has been adjusted as suggested.

- P. 357 You Speak of . . .further insight into the nature and streambed controls...". Shouldn0t it be ". . .nature of streambed controls. . ."

Author comments: Changed as suggested.

- P.357 Section 4.3.1 I would as well recommend inserting a table describing the four CASES/combinations of VHG and FO-DTS gradients, though the four CASES characterize your core findings.

Author comments: We included the VHG vs. dT information for the different cases in brackets as suggested and introduced a list of bullet points for the presentation of the case distinction as suggested.

- P. 358 L.15-21 This sentence needs to be shortened

Author comments: Has been adjusted as suggested by the reviewer

- P. 358 L.23 change to . . . indicating upwelling locations. . .

Author comments: Changed to 'indicated groundwater up-welling'.

Figures and Tables: - Table 1: Is the meteorological station in Keele, where air-temperatures were observed, the same station where you measured precipitation?

Author comments: Yes

- If you use colors in tab 2, I would use an additional color for CASE 1. Alternatively, add an additional column to the table which shows the four CASES.

Author comments: We used WHITE for CASE 1 as with the current colour choices differences in gray shadings were strong enough to recognise the differences of four cases even in b/w prints. Thus, we decided to keep a white background for CASE 1.

- In Table 2 you compare the VHG differences to the spatial mean with the DTS- differences to the spatial mean (nearest) (shouldn't it be FO-DTS differences. . .). Comparing these results with the spatial extent of the FO-DTS cable loop (Fig 3B), the space between P1 (in table 2 it0s T1,T2. . .needs to be adjusted) and the end of the cable loop seems to be too large to compare it directly (table 2), facing the spatial variability of VHG0s (your findings!). I would leave this data point out.

Author comments: Changed to 'FO-DTS differences' as suggested Piezometer labelling (P vs T) has been adjusted as suggested. Figure 3B has been adjusted as the cable loop has been located actually in direct vicinity of P1.

- Due to the resolution of the x-axis in figures 4 and 5, the implementation of shaded columns or bars into the figures 4a/b and 5a/b indicating the time of your field campaigns (may be different bars for VHG and FO-DTS surveys), could help to better understand the data sets. When visually comparing the timing of all field campaigns the temporal variability of observed temperature and VHG anomalies are more intuitively recognizable.

Author comments: Indication of timings of field observations have been included in Figure 4 a,b as suggested.

Anonymous Referee #3 Received and published: 25 February 2012

Overall comment:

Manuscript hess-2011-393 by Krause et al. presents an interesting empirical study of groundwater-stream water exchange dynamics in a lowland stream environment. In particular, the novelty of this investigation is the authors' use of two independent methods of observing water flux patterns across the stream bed. These methods consist of the well-established vertical head gradient technique, and the increasingly well-established fiber-optic DTS technique for temperature measurement. The authors make a compelling point that these methods can complement each other and create a more complete picture of groundwater-stream water exchange dynamics in streams, especially low-land streams during baseflow conditions. For this reason, I find the study compelling, novel, and appropriate for publication in HESS-D.

The manuscript itself can be significantly improved before publication. I find the core of the science in the study to be rigorous and interesting, but the presentation of the study does not clearly convey the novelty and context of this study in light of other literature.

Overall, I suggest that the discussion and conclusion be significantly restructured with this in mind. Correcting these manuscript structural problems will likely amount to moderate revisions, but will greatly improve the clarity of the paper and the likelihood that this paper will have a large impact in the hydrology community. Below I provide suggestions to the authors which I hope will help strengthen this manuscript.

Author comments: We are delighted that the reviewer acknowledges the novelty and potential impact of the presented methodological framework and its application presented in our manuscript. The manuscript has been revised according to the reviewer suggestions. Please find detailed responses below.

General comments: -Manuscript structure should be improved. The major structural problem lies in the discussion and conclusion section content. The minor structural problem lies in the sentence structure used throughout the paper. The major structural problem should be addressed before publication, while the more minor issues with sentence composition should be noted by the authors so that they can strive to improve composition in the future. Suggestions for improving these structural aspects are provided detailed comments below. Of note, the introduction, methods, and results do not suffer from any major structural problems – they are good, just occasional sentences that should be revised. These good sections amplify the structural problems in the discussion, namely the lack of discussion showing how this study is novel and related to theory and previous work.

Author comments: Please see our detailed reply to specific comments below. Sentence structure and phrasing has been adjusted where necessary as suggested by the reviewer. The discussion section (and partly also conclusions) have been revised in order to accommodate a discussion of the wider implication of the proposed methodological framework and its application in particular for the analysis of exchange flow patterns and biogeochemical cycling at aquifer-river interfaces. We emphasized the importance of these implications by more intensively referring to state of the art and recent publications in the discussion section.

-The mathematical formulae, notation, and discussion need to be improved, especially section 3.2. The authors must take care to layout these equations properly, and then be consistent with notation throughout the entire manuscript. I do not doubt that the authors made the correct calculations in their analysis, but Section 3.2 does not clearly convey what they calculated. There are certain notation used that is not defined, and the expressions do not seem to be in proper form given what the authors calculated (e.g., standard deviation). The notation in Section 3.2 is incomplete, and the notation that they do present in the equations is then presented differently in the results and discussion sections.

Author comments: Consistency of notation as well as mentioned monitoring dates and periods has been checked and adjusted were necessary in the revised manuscript

-Dates of VHG measurements do not appear to significantly overlap in time (or hydrologic conditions) with the FO-DTS measurements. How to you address the possible uncertainty of comparing two different measurements collected at two different periods in the surface and groundwater conditions? The discharge was _1.5 to 3 times larger and variable during most of the DTS measurements compared to the more stable baseflow conditions under which the VHG measurements were collected. This variability in catchment conditions and in stream head is causing changes in the gw-sw exchange dynamics, and yet this is not addressed in the paper.

Author comments: We revised the manuscript to better highlight how monitoring periods of VHG and FO-DTS overlapped with each other and coincided with hydrological conditions. This revision helped to improve the transparency of the monitoring strategy and the relation of VHG and FO-DTS surveys. It also highlights that all VHG monitoring (with single observation dates spread out over the entire observation period) revealed groundwater up-welling only. Surface water down-welling was only indicated by inversed groundwater and surface water heads in the automated monitoring programme and appears to be restricted in space and time. Hence, we are confident that the observed patterns of VHG and FO-DTS cover representative conditions. The vari-

ability of signal strength in both, VHG and FO-DTS observed temperature anomalies are actually part of the classification scheme of the presented framework.

-Figures are good. Many are very nice, but many of them have a lot of very small graphics which may make them less effective. I think there are some simple modifications which will enhance these compelling figures. I provide these suggestions in the detailed figure comments below.

Author comments: We increased the axis labelling and text in most figures as suggested by the reviewer – please see detailed replies at the reviewer's specific comments below.

-There are inconsistencies in the dates of the study which arise in the text, tables, and figures. See detailed comments for cases.

Author comments: These inconsistencies have been adjusted in the revised manuscript.

Detailed Comments: Introduction: Need to clearly define in the introduction what you mean by "streambed structure." Discuss surface features, subsurface features, porous media properties (K values and heterogeneity).

Author comments: We elaborated and specified the discussion of 'streambed structures' in the revised manuscript.

P341 L26 – P342 L3: Runon sentence, break into 2 statements.

Author comments: Changed as suggested. Sentence has been separated after '... smaller headwater streams.'

P341 L28: add "changes". . ."measurable changes in temperatures. . ."

Author comments: Adjusted as suggested.

P342 L14-16: This sentence is unclear; reword.

Author comments: Introduced comma after 'both', which clarified the sentence structure.

Materials and Methods: P344 L2: replace "section" with "of the study reach"

Author comments: Rephrased as suggested.

P344 L4: replace "exemplary" with "example"

Author comments: Changed as suggested. P344 L5: replace "for" with "of"

Author comments: Changed as suggested.

P344 L5-L11: runon sentence; reword in separate sentence statements.

Author comments: Changed as suggested and split into two sentences

P344 L9: (and global comment): remove use of "/" as a character between words. It has an ambiguous meaning, and your study should be reducing ambiguity. Use explicit words in its place here and in all uses of it throughout the text. For example, in this sentences replace "existence/absence" with "presence or absences of"

Author comments: Has been adjusted throughout the manuscript as suggested.

P344 L19: (and global comment for text, tables, figures): Period of study varies within 2009 according to dates here in text, in table 1, and in the figures. Take care to make sure all dates are consistent. These small inconsistencies cast doubts in the reader's interpretation of the study.

Author comments: Has been thoroughly checked and adjusted throughout the manuscript.

P346 L8: write out the word "approximately"

Author comments: Changed as suggested.

P346 L15 & 20: Briefly explain what "double-ended mode" means and make sure to

use it consistently – with our without the hyphenation.

Author comments: Brief explanation of the principle of measurements in double-ended mode has been added in the revised manuscript and consistent use has been checked.

P347 Line8-9: Are you stating that you assumed that the piezometer terminated in a low conductivity zone of the streambed if you were unable to draw a pore water sample from it? Be clear in stating the assumption as this seems to come up later on in the results and discussion.

Author comments: We clarified that such assumption was based on observations in the up to seven sampling tubes of the piezometer, not just on the location of the bottom end of the piezometer.

P.347-348 Section 2.3 Data Analysis: This entire section needs to be redone with care, especially in the mathematical equations and notation. There are inconsistencies and ambiguities all throughout this section, which cast major doubt on the interpretation of data throughout the rest of the paper. I assume that the calculations were done properly, but taken at face value, this section does not clearly show what was done. Each equation should have proper notation, notation definition, and parameter indexing.

Author comments: The manuscript has been thoroughly revised in order to remove any inconsistencies in notation. Please see also response to reviewer 1 comments for specific changes.

P347 L11: replace “indicating” with “indicate”. Change runon sentence by placing a period “.” after “surface water”, should read “. . .and surface water. The VHGs were determined from. . .”

Author comments: We decided to leave this sentence as it stands as it only assembles a length of 2.5 lines and seems to clearly describe the content.

P348 L4-5: do not use a), b), etc for a listing of 2 items. This is done here and a few more times in the manuscript. This is not needed for a list of two statements. Just use

delete use of a) and b) in this case.

Author comments: We feel that a) and b) or numbering works quite efficient for separating and thus, highlighting the distinctiveness of listed arguments or processes as in this case, leading to better clarity and transparency of the argumentation. We therefore decided to keep it.

Results: P350 L2-5: There is no data, even anecdotal to support your assumption that the bed was entirely immobile while your FO cable was buried? Did not someone do visual inspections to make sure it was not exposed during the course of the data acquisition period? Beds, especially lowland streams have constantly moving bed material (e.g., ripple and dune formations and migration will cause multi-centimeter variations in burial depth alone).

Author comments: We added that visual inspections confirmed that the cable was not exposed during the monitoring period.

P349 L25: Again remove use of “/”

Author comments: Changed as suggested

P350 L7: replace “at” with “on”

Author comments: Changed as suggested

P350 L16: VHG keeps getting redefined. Is this proper format?

Author comments: Removed the repeated definition of VHG as suggested.

P351 L190-21: remove “(a)” (b)” not necessary. Remove uses of “/” in these sentences too. Reword sentences to make statements more clear.

Author comments: Sentence has been rephrased as suggested and ‘/’ removed.

P352 L6 (and throughout results discussion): Why are you using STDEV here? This is not consistent with Section 3.2. I know what you mean, but be consistent with defini-

tions and notations throughout the manuscript.

Author comments: Notation has been thoroughly checked for consistency and adapted in the revised manuscript as suggested.

Discussion: ***The discussion needs to be restructured. It needs to focus on what new contributions this study makes in light of existing literature and theory. Start off by stating what makes this study novel and then put your work in the context of other literature. It has virtually no connection to the literature which was nicely introduce in the beginning of the paper. This is a major issue that needs to be fixed in the discussion. Also, do not bury the novel concepts (i.e., your framework in Section 4.3) way below a significant amount of rehashing results in Sections 4.1 and 4.2. Together, these issues are likely to decreases the clarity and impact of the paper for most readers. I suggest the following potential fix for the authors to consider: The key point is really that of a new methodological framework to describe certain gw-sw systems. Right? And you state that point, roughly on P355 L7-16. . .after two sections of ambiguous discussion. So lead with that idea and how it naturally leads you to your framework discussion in Section 4.3. Then develop your general framework as you do in Section 4.3.1. And illustrate it as you have in Section 4.3.2., but with addition information from cases in the literature and your specific examples from Section 4.1 and 4.2 that show own VHG or DTS on their own lead to ambiguity, but how your data and methods get rid of some of the ambiguity under certain conditions. This suggested restructuring will greatly reduce the length of the discussion, put the most important contributions up front for the reader, and then expand on the concepts through your data and other existing literature.

Author comments: The discussion has been substantially revised in order to discuss the relevance of the findings of this study for analyses of exchange flow patterns, residence time distributions and related patterns in chemical cycling at aquifer-river interfaces. We therefore, also referred more intensively to recent literature in the discussion again, in particular outlining which knowledge gaps and limitations in process under-

standing can be improved by the presented methodological framework.

Also for section 4.3.1. I recommend you putting your different cases in a table or bulleted format, because it is hard for the reader to keep referring back to them while reading the rest of the discussion. Basically you are stating: Case1: increase SD, increase VHG – increase flux Case2: decrease SD, decrease VHG – decrease flux Case3: decrease SD, increase VHG – K limited Case4: increase SD, decrease VHG – dh/dl limited (head limited)

Author comments: We included the VHG vs. dT information for the different cases in brackets as suggested and introduced a list of bullet points for the presentation of the case distinction as suggested.

P359 L3: The title of this section has “Uncertainty” in it by uncertainty is not discussed here or anywhere else in the manuscript, which raises a concern. Only limitations in applying this approach to other systems are discussed here. So a proper discussion of uncertainty is needed. I am not asking for quantitative uncertainty analyses to be added, but to at least discuss how the disparate timing and hydrologic conditions in the measurements of VHG vs DTS may impact the interpretation of the data and findings. You specifically, state summer baseflow conditions in this section, and yet elsewhere and in the data we see that the flow conditions were clearly not just baseflow – they varied almost 3-fold during the observation period and predominantly when you were measuring DTS temperature and not when you were measuring VHG.

Author comments: Please find our reply to the general reviewer comment above which also relates to this remark. We revised this part of the discussion by emphasizing potential uncertainties of the applied approach as part of its limitations. We also highlighted that both, FO-DTS and VHG observations were carried out during variable stream flow conditions, both aiming to cover site specific temporal variability. We aimed to highlight this better by presenting the respective monitoring times in Figure 4.

P359 L16-17: delete “(a)” and “(b)”, and “)” after “gradients.”

Author comments: In this case (a) and (b) have been removed in the revised manuscript.

P359 L26: delete and replace usage of "/"

Author comments: Changed as suggested.

Conclusions: Unfortunately, the sentence structure used throughout most of the conclusion really limits the comprehension of the main points. Much care is needed to restructure the sentences throughout the paper, but especially in this section. More effort needs to be paced on removing all of the qualifying prepositional phrases from the beginning of the sentence. The most effective short sentences typically have a simple structure: 1. Noun, 2. Action verb, 3. Modifying statement adding information about the noun and action. In many cases in this paper, and especially in this section (e.g., P360 L17- 28), there are one or two prepositional phrases before we get to the noun and verb. This makes the reader struggle to know what the point of all the upfront details are because they have not context as to why it is important. Specific sentences that need to be addressed structurally are pointed out below. Hopefully, simply rearranging the components of the sentences and cutting out some of the redundancy in this section will help make the conclusions more effective and clear.

Author comments: Conclusions have been rephrased where needed – please also see more detailed replies to the reviewer comments below.

P360 L4-5: What is this about "proportionally smaller groundwater contributions to the overall discharge? This is the first time this comes up, and makes me wonder why this was not addressed elsewhere? Is it really relevant? If so, it needs to be discussed, not presented as a conclusion.

Author comments: Actually, we refer to this already in 1.2 (last ten lines in comparison to headwater streams). However, in the revised manuscript we also included a reference to the impact of proportionally larger groundwater contributions in lowland

rivers under 1.1. in order to highlight the importance of these investigations for spatial patterns of groundwater born nutrient inputs into rivers.

P360 L7-12: Condense and reword this sentence per above recommendation.

Author comments: Has been rephrased in the revised manuscript as suggested.

P360 L17-28: These 3 sentences need to be restructured and honed.

Author comments: L21-28, the last two sentences have been rephrased in the revised manuscript.

P360 L28- P361 L3: I am not sure I understand this sentence or the point. How does this comment connect to anywhere else in the manuscript?

Author comments: This provides a suggestion of possible future applications of the framework. We clarified this in the revised manuscript.

Tables: Table 1: First of all, great idea to put this information all in one place. Very effective. Second, this is another place where the dates show inconsistencies within the other parts of the manuscript. Specifically, the early part of the season May or June seem to have date inconsistencies here and elsewhere.

Author comments: Inconsistencies have been removed and adjusted in the revised manuscript – please see alsoearlier comments above.

Table 2: Just so you know it becomes very hard to read the numbers under the color once you look at or print this table in grayscale. Maybe you can highlight these rows in different ways?

Author comments: Please see reply to reviewer two comments who suggested an additional colour for Case 1 – we tried to optimise the colour scheme that even in b/w print the grey scales are distinguishable.

Figures: All figures are very good, just minor recommendations on how to possibly

improve them and deal with the issue of very small figures and numbers. Figure 1: text in labels is just about as small as you can go, and if it gets compressed down in publishing then they may become unreadable. Might consider making label text larger.

Author comments: Arrows indicating general groundwater flow direction have been removed in the revised version – this also improved the clarity of labels.

Figure 2 caption: replace “exemplary” with “example”. Nice figure, but again very small text verging on unreadable.

Author comments: Replaced exemplary as suggested.

Figure 3: again, nice figure but text is too small, and you cannot tell for certain by looking at the piezometer diagram that they are multiple nested piezometers. The image is just too small. This is how you make apparent identification of low K zones in your stream bed so it might be worth enlarging this figure and text font size. Plate C is clear to read so maybe target that format.

Author comments: We followed the reviewers suggestion and increased the A and B parts of the figure, increased the font sizes in A and B by one point or more and included an indication on the multi-level sampling depths which are the basis for the discriminations of low K zones.

Figure 4: Recommend showing in box shading or with vertical lines when you did your VHG and FO-DTS sampling. This will clearly show when and over what hydrologic conditions you got your data.

Author comments: We followed the reviewers suggestion and included vertical indicators in the figure that highlight the timing of the piezometer observations as well as -FO-DTS monitoring

Figure 5: try to make label text larger.

Author comments: Adjusted label text in all figures where needed.

Figure 6: Suggest removing the northing and easting labels because they take up a lot of plot space and add nothing to the figure. . .in fact they are distracting. They are not necessary because you have a scale bar and we already know where in the world we are from Figure 1. Getting rid of the N and E labels will free up space for you to enlarge the other text in the figure and make it more clear.

Author comments: Following the reviewers suggestion we increased the text-labels in the figure by at least two points, in some cases more. We, however, decided to keep the N, E axis labels as these are important to guarantee transparency of the presented results and the reader can extract exact information on the location of presented results.

Figure 6 caption: just another date example that seems inconsistent “25 May?”

Author comments: As mentioned above, inconsistencies in sampling dates and notations have been thoroughly checked and adapted throughout the manuscript, including tables and figures.

Figure 7: these are cool, but are too small to really read. One suggestion is to get rid of the N and E text that takes up so much space and adds nothing. This will allow you to expand the actual images by at least 20% without having to increase the overall size of the figure. You will need to add North arrow and a scale bar after removing the N and E values.

Author comments: Figures have been adapted and increased in the revised version which substantially improved the clarity of the presented content.

Figure 8: y-axis labels are very small and make sure they are consistent with your text notation after you make the above section 3.2. corrections.

Author comments: Consistencies have been checked and adapted where necessary as suggested.

Figure 9: again x and y-axis are verging on unreadable, be consistent with new notation

once selected. For C, D, E, F plates, again get rid of N and E which are unreadable and add nothing, then put in a scale bar.

Author comments: Axis labels as well as overall size of figure tiles have been increased in the revised version as suggested.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 337, 2012.