The reviewer's comments are in *italics* and the responses in regular text.

*I recommend this manuscript to be published after making several major changes. First of all, I would like to congratulate the researchers on their great work. Estimating seepage rates is incredibly hard and this work helps others in their estimation. I recommend several changes that, from my point of view, would help readers/others.*

Dear Bas des Tombe,

First, we would like to thank you very much for your recommendations and the time spent to comment our article. We are pleased to read that you appreciated our work and its implications. Your constructive comments allowed us to clearly identify the points that need to be improved in the manuscript. We will make sure that these points will be addressed and included in the revised version of the manuscript.

Please find in the following responses to your main comments and concerns.

Kind regards,

Nataline Simon & co-authors

First, several comments were about the sources of uncertainties, the uncertainties on estimates, the boundary conditions, the possibility to quantify seepage fluxes given these uncertainties or the meaning and representativity of the estimates. Instead of responding to each comment separately, which would have led to many repetitions, we gathered all these comments below and provide then a general response.

*- The uncertainty of the estimates is discussed at the end (..2 cm offset of the fiber introduces an error in the seepage estimate of 50%..) to a (very) limited extent. It is currently presented as an afterthought and I would like to see this as a significant part of the body of the manuscript. The uncertainty of the estimated seepage rates is an important part of the discussion and allows for better comparison of the different methods. I expect the uncertainty of the estimates to be so large that I wonder if estimating flow is even possible with the presented methods, and it would rather be identifying locations where the river is gaining. Maybe the manuscript is better of estimating locations where the river is gaining, it would be a very valuable contribution to the field.*
*- After accounting for all the uncertainties, are you still confidently able to discuss estimates, or maybe weaken/loosen the goals by discussing locations of inflow. Which already an achievement on its own, considering the difficulty of this type of fieldwork.*
*- I would have liked to see a detailed description of the initial boundary and boundary conditions of both the active and passive heat tracer test. And to which extent the simplifications of initial/boundary condition affect the estimated fluxes. A few lines are devoted to this at the end but I see this as something essential that should be part of the beginning.*
*- The reduction from a complicated river system to a 1D flow model is not well supported with arguments. Apart from all the uncertainties introduced by this simplification, rough estimations of thermal properties introduce additional uncertainty.*
*- I would like to see the heat tracer test results including the uncertainty in the depth the cable is buried. This is now presented as an afterthought in the discussion. I would like to see it incorporated from the start.*

The manuscript describes and compares the application of active and passive heat tracer tests to locate and/or estimate groundwater discharge into streams. Here, you are perfectly right that the question of the uncertainties of the estimates must be addressed. To achieve this, it is important to clearly separate both approaches (passive vs. active).

Concerning ***the passive experiment***, we fully agree with your comment. In the manuscript, we conclude that the analysis of the passive heat tracer test is not sufficient to estimate fluxes since the uncertainties of the estimates are too large with this approach. Thus, we explain that this approach is more suitable for identifying locations where the river is gaining. As you mentioned in your comments, the uncertainties of the estimates for passive experiments are due to several sources of uncertainties, especially:

1) the uncertainties on the thermal properties of the sediments, on which we mainly focused in the manuscript. We widely discuss the projection of the uncertainty of the thermal conductivity on the estimate of the seepage rates and clearly show (for instance in the section 3.1.3 and in figure 9) that the assumption on thermal conductivities highly affects the fluxes estimates.
2) the uncertainty on the burial depth of the cable, which is rapidly addressed in the manuscript as you correctly underlined.
3) the uncertainties linked to the boundary conditions of the model, which are not directly discussed in the manuscript.

Because the uncertainty of estimates resulting from the uncertainty on thermal conductivities is so large, we considered and concluded that the passive experiment is not sufficient to estimate seepage rates. Considering this, it appears that discussing more about the assumptions on the burial depth of the cable and the boundary conditions would be not actually relevant (the quality of the estimates being not satisfactory enough). Of course, it would have been very interesting but we think that such sensitivity analysis would overload the manuscript, which is in itself rather long, and would digress from the main objective of the study, which is the comparison of active and passive experiments.

However, we are keen to strengthen these points in the revised version of the manuscript. Thus, the sources of the uncertainties would be clearly identified and each point would be discussed.

Concerning the ***active experiment*** however, we are convinced that the approach allows locating **and estimating** fluxes. Indeed, compared to the passive experiment, the uncertainties are very low and the estimates are significant. Here are the reasons why:

First, as explained in the manuscript, the analysis of the temperature evolution during heating periods leads to estimate as a first step the thermal conductivity of the sediments with a very good accuracy (as demonstrated in Simon et al., WRR 2021). Thus, the approach does not require assuming the value of the thermal conductivities which considerably reduce the uncertainties of the fluxes estimates. Note also that in our previous study (Simon et al., WRR 2021), the sensitivity of different parameters (thermal properties, groundwater flow, heat injection) on the temperature rise has already been studied in details. Errors induced by noise in temperature measurements were also discussed in this study.

Then, concerning the burial depth of the cable, we mentioned in the manuscript that it might potentially affect the thermal response to heating, if the cable is too close to the stream and we explained that further investigations should be done to quantify the effect of the near stream on estimates. Here, we are confident that the burial depth has low effects on the temperature evolution. The active experiment was conducted straight after the installation of the cable, ensuring that the burial depth was sufficient to limit the effect of the near stream (results from modeling showed that the heating is particularly localized around the heated cable).

Then, one of the main advantages of the application of the active experiment in gaining streams is that the temperature evolution is independent of temperature boundary conditions, as long as the groundwater temperature is constant over time (which is the case here). Indeed, in gaining conditions, the stream temperature variations do not (or just a few) propagate in depth meaning that the temperature variations recording along the FO cable would exclusively due to the heat injection. However, it is essential to confirm this assumption by analyzing the temperature variations recorded in non-heated buried sections of the FO cable. This verification was made but is not clearly explained nor described in the manuscript. This will be corrected in the revised manuscript. In losing conditions, since diurnal water temperature variations propagates deeper, it would be necessary to separate the temperature evolution induced by the heat injection and the one depending on stream temperature variations.

Except for the thermal conductivity, it is true that the potential sources of uncertainties are not clearly identified in the manuscript. This will be improved in the revised manuscript and we will make sure that the effects of both the burial depth of the cable and the temperature boundary conditions will be discussed in order to strengthen the conclusion about the efficiency of active experiment for estimating seepage fluxes.

*Thus, how to go from a complex 3D world to a highly simplified 1D model.*

The use of simplified 1D model is classic when using heat as a tracer. For passive experiments, this was already proposed in many applications (Anderson, 2005; Constantz, 2008; Goto et al., 2005; Hatch et al., 2006; Keery et al., 2007; among others). The FLUX-BOT model used to interpret passive-DTS measurements was proposed by Munz and Schmidt (2017) especially for quantifying water fluxes between groundwater and surface water. The main assumption of the model is that fluxes are strictly vertical to the stream, which is a classical assumption made in all studies using heat to quantify seepage fluxes. We are aware this is a main issue but this is why this point is discussed in the discussion section. The same assumption is made when interpreting active-DTS measurements. Thus, it is true that it is not possible yet to estimate groundwater seepages the 3D, but it allows at least estimating fluxes in the direction perpendicular to the river, which should be the main direction of fluxes. However, it should be noted that the measurements achieved during active experiments are still significant and can therefore be interpreted.

*The manuscript makes a lot of references to other studies of passive heat tracer tests but their findings are not well enough incorporated. Alternatively, present it as a case study.*

You are perfectly right. First, the interpretation of passive-DTS measurements allows discussing the effect of the assumption on thermal conductivities on estimates, which is seldom done in studies involving passive heat tracer tests. Then, we show how the results of the active-DTS experiment, and especially the estimate of the thermal conductivity, can be used to improve the fluxes estimates from passive measurements. Thus, the introduction will be improved in order to highlight these points and the originality of our work compared to other passive heat tracer tests.

*It needs to be clear from the start that you only discuss river sections that are gaining water. As rivers are meandering, even rivers that are mainly gaining water can locally lose water. It is stated in the title, but a sentence on it in the abstract would help the reader.*

You are right. This point is essential since the effects of both the burial depth of the cable and the temperature boundary conditions are different in losing and gaining conditions. This will be included in the revised version of the abstract. The application of the approach would also be more clearly discussed in the discussion section.

*In the presented passive heat tracer test, the temperature at the location of the fiber is governed by heat transported by groundwater flowing into the river. No/little attention is paid to heat entering via the river's water surface (and accounting for variations in water depth). It is mentioned that the water levels vary with time (L314).*

Concerning stream temperature variations, this is important to distinguish the passive and the active experiments.

Stream temperature variations are fully considered when interpreting the passive-DTS measurements, since the stream temperature measured at the bottom of the stream is used as the upper boundary condition of the model. The model allows deducing groundwater fluxes into the stream, and therefore gradient variations, from temperature changes.

For active-DTS measurements conducted in gaining conditions, we assume that the temperature evolution is independent of the stream temperature variations that do not (or just a few) propagate in depth. However, you are right that in losing conditions, since diurnal water temperature variations propagates deeper, stream temperature variations could affect the measured temperature evolution. In this case, it could be necessary to separate the temperature evolution induced by the heat injection and the one depending on stream temperature variations. This point is not clearly explained in the manuscript and will be added in the discussion of the revised manuscript.

Either in gaining or losing conditions, we suggest monitoring the "natural" temperature variations along a buried section of the FO cable. If temperature variations are recorded in non-heated sections during heating periods, the user could filter the data accordingly (especially in losing conditions). This was made in this study, as explained in the supplement material (section 3.1) :
"*During this period, temperature has been also recorded in sediments with the non-heated FO cable and shows an average temperature of 12.1 °C and a standard deviation of 0.12 °C. This shows that i) the streambed temperature is not affected by potentials air/stream variations during the experiment duration, meaning that the temporal variations are exclusively due to the heat experiment and ii) the heat experiment induces only a small and local thermal perturbation of the streambed around the buried FO cable.*"
This point will be more clearly explained in the revised manuscript.

*Seasonal variations in water depth are not discussed. I can imagine that the section next to the golf court is more canalized then next to the wetlands, and therefore reacts stronger to variations in discharge. Might this also affect the temperature variations and standard deviations presented in Figure 4? Image an aquifer in which the water moves as a plug flow (golf court?) of which the water has a narrow distribution of the residence time. This would mean that the seasonal temperature variation of the infiltrating water is found with little damping. In the wetlands I am expecting a much broader distribution of the residence time and much more damping of the temperature of the water that seeps into the river. For a river that gains the same along their length, I would already expect more temperature variation at locations where water moves as a plug flow, with a higher SD (Fig. 4)*

Indeed, the assumption of broader residence times in the wetland could potentially explain the evolution of the SD. However, similar stream temperature variations are observed in the wetland and in the golf area. It would require very short residence times in the golf area to obtain such results - the residence times in the wetland being high as shown by the constant temperature signal measured in the piezometer near the stream in the wetland. Thus, it does not seem very consistent since it would imply very peculiar behavior.
This assumption could be proved or disproved by modeling the watershed considering a broader distribution of residence times in the wetland. However, such model would be difficult to achieve and time-consuming. Moreover, we are not sure that it would be fully relevant, since previous studies showed that the behavior of the aquifer is relatively "simple" and did not highlight that the water moves as a plug flow in the golf court.

*Punctual means exactly on time. In the manuscript it is often used to refer to an exact location.*

Sorry about this confusion. This will be corrected in the revised manuscript.

*The number of measurement locations is too limited to quantify the flow, as is concluded in an early stage of the analysis. As this manuscript is posed as a discussion paper the passive heat tracer test does not seems to contribute much to the discussion*

Sorry, but we don't really understand this comment. For passive-DTS measurements, the FLUX-BOT model was applied on almost all data collected along the cable deployed in streambed sediments in the wetland area, for d < 150 m, which represents 545 measurements points. Thus, although the model is not applicable for d > 150 m, the results of the passive heat tracer test remain relevant for the discussion. Concerning active-DTS measurements, fluxes were estimated for 172 measurements points along a 60-meter section of cable, which is also an excellent performance. To our knowledge the first time that active-DTS measurements have been applied along such a section. To better highlight the number of measurements relevant to each method, we propose to add this information in the revised version of supplementary materials.

Once again, we thank you very for your comments and the time spent to review our article. We will make sure that all your concerns and all the comments included in the .pdf file will be addressed and included in the revised version of the manuscript.