Effectiveness of distributed temperature measurements for early detection of piping in river embankments

REPLY TO COMMENTS BY REVIEWER 1

Corresponding author: Silvia Bersan

The comments from the reviewer are very useful and will certainly help improving the paper.
The main requests by Reviewer 1 are:

- add references and comparison to previous works in the field of surface water - groundwater interaction;
- rearrange the structure of the paper: group the theory in a single chapter (also asked by reviewer 2) and add a section with a conceptual model about the functioning of DTS for piping detection;
- link experimental data and numerical model and clarify some aspect of the numerical model.

I went through the suggested literature and I agree that it is relevant to this study. I rearranged the paper as suggested and I found that the new structure is much clearer. Finally, I acknowledge that some details regarding the numerical models and a better comparison between numerical and experimental data would make the paper more clear.
In the following I will answer to the comments (reported in italics) point by point, also on behalf of the co-authors.

First, the authors start from DTS applications in geotechnics like dam monitoring but do not take advantage of the large amount of literature that is available in the hydrological community using high resolution DTS measurements to interpret the hydrological system, especially groundwater-surface water interaction (see work of e.g. Selker, Krause, Mamer, Fleckenstein, Cassidy, Briggs and many others to start a literature review). The here-described experiment has many similarities with published ground- surface water interaction studies and awkward statements can then be avoided like P3L27: "heat transfer in soils mostly occurs by conduction". So I suggest to write a generic intro on heat and water transport and how it can and will be used in your study. The paper should add comparison with published groundwater–surface water studies here and as such way stress the novelty and show the relevance for the hydrological community.

I will briefly describe in the introduction the working principle of the thermometric method and some data interpretation methods used in dam monitoring and in groundwater–surface water studies. I will try to add comparisons in the discussion of the data.
I will also try to explain what is the novelty of the study. In my opinion the main novelty consists in the fact that data interpretation models so far mostly consider steady water flow. This work focuses on the effect of a transient flow of small duration. The work also focuses on permeable soils, which are the soils most prone to backward erosion piping, in opposition to low permeability soils of which dams are made of. The effect of the size of the water retaining structure is also investigated in this paper.

Awkward statements can then be avoided like P3L27: "heat transfer in soils mostly occurs by conduction"

That can be rearranged as:
"The [thermometric] method is based on the principle that, in the absence of seepage (or in presence of moderate seepage flow), the temperature distribution in the upper portion of the subsoil fluctuates seasonally in response to the variations of the air temperature."

Second, the paper needs a clear methodology section in which the theory coupled heat and water flow modelling and the large-scale experiment are given. Describe both methods in the section 2 (bring 5.1 to section 2).

I do agree. Section 5.1 and the first part of 5.2 have been moved to section 2

How is the DTS calibrated?

The measurements were performed by a company different form the company that designed and led the test. The calibration procedure was not specified. It was guaranteed that temperature changes were measured with an accuracy of 0.1°C.*
* O. Artières and G. Dortland (2012) IJkdijk All In One Sensor Validation Test. Report of the measurements made with TenCate GeoDetect® system on the East, West and South dikes, TenCate Geosynthetics

How did you make sure to distinct temperature from strain induced changes as there is quite some strain on the FOS cable which influences your measurements?

The DTS system exploited the Raman effect, which is not sensitive to strain.

The authors should clearly explain why the numerical model is needed/used (to what purpose do you model), why this formulation has been used and what assumption they made. Also add scientific references here.
I think the main aspects of Bersan model (PhD thesis) should be included in the paper (maybe as supplement) as it is very relevant for this research. Furthermore, I found parts of this description unclear and confusing.

The model is used to understand the impact that factors as geometry, time-scale of the hydraulic loads, initial conditions, thermal dispersion etc. have on the temperature field. The results in Fig. 11 show the effect of time for a given geometry (the geometry of the test dike)
Dimensional analysis is used to clarify the effect of geometry for steady state conditions. A (steady state) numerical model is used to verify the soundness of the dimensional analysis. This model is not intended to reproduce the field measurements but to extrapolate the results to a more general context.
The path we chose to generalize the results of the test was splitting the problem in two parts: a steady state part, addressed using dimensional analysis, and a transient part, addressed using the thermal front velocity.
I will include more details about the numerical model.

For example, why you use specific discharge rate and not Darcian velocity

With specific discharge rate I mean Darcian velocity. I have now clarified it in the theory section.

or P10L30 "neglects effect of conduction" followed by P10L31 "describes the effect of heat conduction". P11L8 the authors start writing annual temperature fluctuation under the assumption that "u=0" (P11L10). But this does not hold for your condition so elaborate on this assumption and why it can or cannot be used. This effects strongly the interpretation of your results. Can the authors elaborate on this?

Good point. I will clarify this in the paper.
The assumption u=0 holds true if we want to determine the temperature of the test embankment at the beginning of the test, because no seepage occurred from the moment the embankment was built until the beginning of the test.
The results are also valid for real cases where u is small enough that advection is negligible (excluding sporadic flood events). This holds true unless we are dealing with a strongly gaining or loosing stream. Whether advection is negligible depends on the hydraulic gradient between river and groundwater and the hydraulic conductivity of the sediments.
Therefore, the condition is not always satisfied in the field but I believe it is representative of a category of a large number of real cases.

Third, the authors give some confusing if not conflicting description on what they expect from DTS under an embankment. Moreover, they do so in different parts of the paper. I suggest to include a structured section on the perceptional and conceptual model the authors have of using thermal measurements at the toe of the embankment (fig 13). This in terms of expected advective and conductive heat transport (see also first lines of section 4). A first statement on the expected flow travel time underneath a dike which you will elaborate later.

I do agree on the fact that a section containing a conceptual model would be useful. So far, I prefer to insert this section after that presentation of the data from the field test, because I consider the conceptual model an output of the research, since it was elaborated observing the data. The data will be then presented with no or little interpretation in section 3 and will be discussed in the section 4 where the conceptual model will be elaborated.

Maybe also interesting to say something on expected temperature difference in river water during a year and during a flood (do you know if river water temperature changes a lot during typical flood events?).
Basically, under which seepage conditions could DTS be informative for detecting piping (initiation) and when not. Obviously, if all water has identical temperature, nothing will be detected using DTS. Second, if the river water is warmer/colder than the GW, it is not expected to see piping developing at the toe of the embankment.
In contrast to what the authors write (P7, L22-23), if you detect T differences due to river (reservoir) water the preferential flow path (possible pipe) already exists from river to toe of embankment and does not need to develop anymore. And your objective is detection the initiation and backward development of the pipe.

A preferential flow path already existing between river and landside could represent a region of higher permeability (f.i. a paleochannel), which is the best location for the formation of a pipe. Therefore this information is useful for identifying spots where the risk of piping is higher rather than for detecting existing pipes. I will explain this in the text.

Backward eroding cavities do not connect yet to the river and will logically first see inflow of the surrounding water (no T-difference). However, in case of warmer/colder water at the toe of an embankment compared to water underneath the embankment, piping initiation starting from the toe and eroding backwards, can be detected (as fortunately the case in this experiment). Note that strictly speaking one does not detect piping with DTS but rather preferential water flow. In my opinion such structured concepts will help describing your field data analysis and theoretical modelling results.

I'll try to make use of this description in rearranging the conceptual model.
Fourth, the field results (current 3.3). This can be reduced to only data as concepts are given already.

As said earlier, the data will be presented with little interpretation next to it in section 3 and will be further discussed in the section 4 where the conceptual model will be elaborated.

I suggest to the authors to stop describing the experiments after t=90hours as hereafter it is not a piping experiment using DTS anymore but a failure experiment which is not part of the paper's focus.

I think temperature data after 90 h are still interesting and helped in elaborating a point in the conceptual model that would not be otherwise possible to explain.

Lastly, the results of the dimensional and numerical analysis can be presented and the influence of the model and boundary assumptions be discussed.
I, as a small example, do not understand why Figure 11 presents data using a constant hydraulic load of 3 m was used while the authors have time series of hydraulic load during the experiment.
The dimensional analysis presented in the manuscript allows to quantify the relative importance of conduction versus advection. This conclusion is important for the propagation of the front assumption which requires advection to prevail. The results of such analysis are based on the assumptions of steady-state boundary conditions as acknowledged by the authors. Then the question arises if the model used for determining the thermal front propagation is representative of the actual conditions of the experimental set up?

First, the model was used to reproduce the experiment. The complete model had non-homogeneous initial conditions and variable inflow temperature as well as variable hydraulic gradient. Some details will be given about this model.
* Once calibrated, the model was used to run other analysis to extend the results to a number of field situations. Homogeneous initial temperature, constant temperature of the inflow water and constant hydraulic load were used as a more generic boundary conditions. Therefore data figure 11 are not to be compared directly with the results from the test. This will be clarified in the text.
Next to the model reproducing the experiment, another model with constant hydraulic load has been run. This was done to simplify the problem and highlight the time scale of the problem. This can be better explained in the text to avoid erroneous comparisons between model and data.
The model was always run in transient mode, initially applying transient BC representing the actual conditions at the experimental site and, later, constant BC condition to simplify the problem. This will be specified better in a brief description of the numerical model.

Minor points:

- Comparison with use of DTS in concrete dams seems not so relevant in your study
In some concrete dams the sensor is placed in the foundation soil to detect underseepage. In concrete face dams DTS is used to detect leakage from joints. The basic working principle is the same. I will clarify it.
- Change terminology "Downstream and upstream of water under embankment" into "landward and river side"
I agree. In the description of the field test I'll keep using upstream-downstream, but in the description of the conceptual model and other discussions I'll use riverside and landside, because when talking about rivers the terms upstream and downstream would be otherwise confusing.
- P4L10: "easily installed also in existing embankments" That is quite a statement. This only holds if the FO is installed in the toe (landward side) of the dike. But it is unclear if that is an optimal location for the FO in case of piping detection. I believe this is far from optimal location. Authors should explain this more.
True. This is maybe more accurate:
"Optical cables can now be installed both in new and existing embankments lengthwise the dike. In existing embankments the cable is generally placed at the toes or along the slopes, since installation at these locations simply require the excavation of a narrow trench. Installation at other locations would require the use of directional drilling or similar techniques."
I will also elaborate about the optimal position of the sensor in the discussion or conclusions.
- The derivation of the thermal front velocity starting from the heat transport equation in porous media is used to determine the "Retardation" coefficient. This coefficient is presented in equation 3 but its derivation is not yet clear.
I don't understand what the reviewer would like to find in the text. Showing the derivation of the retardation coefficient means showing how the advection-diffusion equation is determined for a porous medium, which is out of scope here. Maybe adding a reference to the theory is beneficial. For instance: J. Bear (1972) *Dynamics of Fluids in Porous Media*, American Elsevier Publishing Company, Inc., New York.
- Time scale of figures could be homogenized to facilitate interpretation for reader
I agree.
- Figure 6: It is written that the toe location temperature (F1) is highly influence by the air temperature whereas in the plot it is observed that it seems to have only influence between t= -7 days and t= -2 days air temperature is constant between t=0 days and t=5 days but the temperature at F1 continuously drops. Please explain.
The temperature at F1 is highly influenced by the ait temperature when there is no water flow, therefore before the beginning of the test (t=0). In presence of water flow the temperature at F1 is determined by the temperature of the seepage water.
- Why F1 reaches a temperature lower than any measurement in subsurface? Did you take into account that the FO cable could be strained due to displacement and therefore shows deviating temperature measurements (this links to the calibration question of the DTS.).
The lowest temperature measured at F1 is very likely the temperature at the base of the dike at the beginning of the test somewhere between line F3 and F4. It is likely that none of the 5 measuring lines was placed at the coldest spot under the dike. The coldest spot is not exactly under the crest because of the exposition of the dike with the slopes facing south and north.
- Figure 7: plot this figure in color gradient for improving the readability. OK
- Figure 9 shows negative gradients in X=18. Why is this? I don't see negative gradients in Figure 9.
- Figure 10 could be explained based on the possible variability of the hydraulic conductivity.
Although the test dike was built as homogeneous as possible, some minor heterogeneities were present due to replacements of materials after a previous series of tests.
However, in this case the gradients at the two locations start diverging exactly after the developments of the first pipes…quite suspicious.

- Figure 11 seems like is not considering the same initial conditions as the experiment. Also, the figure 8 at t=60h (approximately 2.5 days) shows a boundary temperature of around 14 degrees but the model results remain in 12 degrees. This may be interpreted as a large error taken into account that the steady state is achieved by a total increase of only 4 degrees.

See answer above (*)

- Figure 12 is not explained neither the variables or the system. This figure is a copy (very minor modification) of the original presented in Van der Kamp and Bachu 1989.

The figure is indeed not very informative. I will eliminate it.

- Figure 15 and 16 show the temperature gradient along the x axis but it is not clear if they are located in y=0 or further. Please explain and compare with the experimental measurements.

The coordinate x in Fig. 15 and 16 is the coordinate y in the graphs depicting the results of the field test. I could change it if it is confusing. The measurements of field test can only be qualitatively compared to these results, because the test embankment was built only a couple of months before the test. For comparison, we can only consider the north slope where the influence of solar radiation can be neglected. The temperature variation between line F4 and F5 is 1°C while the model predicts 0.5°C.

- In addition, include the information of the moments when sand boils are observed as in figure 5 in other relevant figures.

Ok. I would do that for figure 7, 9 and 10.

- The grammar should be checked as there is an indiscriminate use of commas and frequent use of very long sentences.