"Assessing the effect of flood restoration on surface-subsurface interactions in Rohrschollen Island 'Upper Rhine River – France) using integrated hydrological modeling and thermal infrared imaging", by B. Jeannot et al.

Reply to comments raised by Rev #1

The discussion reported hereafter remind us with the comments of the Reviewer typed in straight font as our answers appear below in italic. In the reviewing process of HESS, a reply should be sent before being allowed to propose a revised manuscript. This is why some of our answers might appear as declarations of intend to which we would then try to stick in writing the revised version of the manuscript.

Thank you for giving me the opportunity to review the work by Jeannot and coauthors "Assessing the effect of flood restoration on surface-subsurface interactions in Rohrschollen Island (Upper Rhine River – France) using integrated hydrological modeling and thermal infrared imaging". The authors have set up a surface-subsurface physically-based model to investigate the effects of flood restoration on an island of the Rhine river. After manually calibrating the model on groundwater heads following a flood event (injection), the authors have validated model parameters and their hypotheses on groundwater heads of another flood event, and further checked that modeled exfiltration patterns matched observations from airborne thermal infrared imaging. This allowed them to study the mechanisms of groundwater exfiltration in restored conditions, and to compare those results to a simple case of pre-restoration conditions. They have showed that in this case restoration indeed enhanced groundwater exfiltration. They further compared two injection scenarii, high rate/small volume or small rate/high volume, and showed that injecting less water but with high rates over short periods maintained exfiltration over longer periods due to the modeled processes (time scale differences between surface water and groundwater response to floods).

- *We are grateful to the Reviewer for his (her) sharp and synthetic view on the material constituting this hydrological study. It is right that we mainly focused the modeling task on mimicking the hydrological behavior of the system over short periods of time but associated with very transient flow conditions. This is the main added value of the study, as very transient features are still challenging to model in the various compartments of the system due to contrasted characteristic times between flow processes and the spatial resolution needed to clearly grasp surface-subsurface flow interactions. This motivated the use of an integrated hydrological model in a form reducing the dimensionality of the subsurface compartment with the idea of rendering tractable simulation highly-resolved in time and space. The main drawback is that, in the context of restoration, which can be a long-term objective, we do not completely evaluate the benefits of restoration works, our investigation being limited to grasp how behave water bodies after forced flood periods.*

This very well-written manuscript, shows in a short and concise way through this case study how a surface-subsurface hydrological model can be used to investigate complex interactions involving short wavelength and small amplitude topography, fast (overland flow) and slow (groundwater) compartments. The authors did a good job in describing clearly the processes at the origin of the surface-subsurface exchanges directions and amplitudes, backed-up by appropriate figures. Although I have some minor comments which I think should be addressed before publication, I think this work is of high quality and suitable for publication in HESS. I think this is a significant step toward improving tools for bridging the gap between hydrological research and water management stakeholders.

- *As told just above, our main goal was to address the feasibility of simulating very transient features with highly-resolved models. We thank the Reviewer for his (her) very positive appraisal on the way we handled this task.*

The two main points I want to raise are related to hypotheses which need to be mentioned or discussed: 1) Why setting-up a lower cost numerical model by simplifying the Richards equation if this does not result in using stochastic methods for calibration, or any other specific advantage? Please also give the runtime for those simulations. For instance, having a low cost model could allow sensitivity analysis to help showing which features of the restoration or paleo-geomorphology mostly impact the exchanges. It could also help to give an uncertainty estimates to the soil parameters and to the restoration effects, which could further help stakeholders.

- *We fully agree that, among the various interests associated with simplified (tractable) models, the "Monte Carlo philosophy" duplicating simulations for various purposes such as sensitivity analysis, inverse problems, tests on hypotheses, etc., is very useful. We did not start our study with this option in mind, simply because we ignored how a simplified model could render a valuable hydrological simulation of the system. Exploratory calculations with a fully-dimensioned model showed that modeling the system was cumbersome, because of: 1- the flat topography needing for spatially refined grids to delineate the flooding of wetlands and ponds, and 2- the very contrasted times of response between the surface and the subsurface after forced injections. One may therefore consider that employing our integrated model over Rohrschollen Island is a preliminary test, before further investigations. We envision to couple inversion procedures to the integrated model with the aim of providing equiprobable configurations of hydrological systems that all match up observation data. The revised manuscript could better justify our choice, probably in the Section presenting the "Hydrological modeling strategy" Mean runtimes of simulations on a standard computer (approximately 5 hours to simulate the first 7 days and 24 h to simulate the whole 45 days) could also be given knowing that independent simulations, as performed in "Monte Carlo" approaches, easily benefit from distributing the calculations tasks over the multiple cores of modern processors.*

2) I am probably biased, but I think a surface-subsurface hydrological model with no surface boundary condition or source/sink term has to be justified. While the high rates, volume and resistance coefficients together with rather short periods involved in the present study probably moves evapotranspiration uptakes to a lower order, and one may assume that no rain happened during the studied periods, those points need to be written down and eventually discussed, for instance for future applications where such a model could be applied over longer periods. Also the calibration and validation periods concern different season, likely to be under different evapotranspiration regimes. Also was the vegetation– and ET uptakes- the same before and after restoration? Although I agree that it is likely that ET has minor effect in this study, those points need to be discussed or mentioned. Finally, I also find curious that no mention to the impacts of surface-subsurface exchanges on ecosystem services for the specific case of the Rohrschollen island are discussed in the introduction, which is rather general, while the case study aspect of the paper clearly appears in the manuscript title.

- *Rev#1 feeds our response in his (her) interesting question! As already mentioned the study is focused on mimicking short-term responses associated with flash floods subsequent to forced flow conditions. The volume of water injected in the system and the varying boundary conditions at the banks of a riverine island associated with dam storages and releases in the river are the main features controlling the evolution of* *water bodies in the Island. Even under a continuous routine base flow injection of 2 m^3s^{-1} through the artificial new channel, the volume of water brought by the Rhine River to the Island is approximately 6.3×10^7 m3 in a year, which would correspond to an equivalent infiltration of 15 m of water in a year over the whole Island. Most of the base flow injection infiltrates via the new channel and the BGW (almost no flow exits the BGW to the North), but even with 20% of infiltration of the base flow injection, this would still correspond to 3 m of rainfall infiltration. This rapid calculations (not reported in the manuscript, we agree) led us to consider that rainfall infiltration and ET were negligible within the modeled short periods of intense flooding. Regarding the eventual benefits brought by floods to ecosystem services, we note that the title of the manuscript only mentions the effects on surface-subsurface flow interactions. That being said, we agree that the Introduction could let room for a few sentences regarding ecosystem services. The revised manuscript could be amended accordingly.*

Specific points:
- Introduction: could you provide example of hyporheic processes that have specific ecological importance for the Rohrschollen? Because the study site is in the title, I would expect some mentions to it in the introduction, which is overall a little long and vague… maybe cutting off some repetitions and adding up a brief section on the specific targets of the restorations on the ecosystems of the Rohrschollen would encourage the reader? This is just a suggestion to improve the quality of the paper.
  - *We agree that the Introduction could be slightly trimmed at a few places and let appear a short paragraph mentioning the specific features reactivated such as sedimentary transport along the BGW, renewal of water in the wetlands and streams with consequences, for example, on "temperature refuges", or on retrieved biodiversity for fish populations, riparian woodlands, waterfowl, etc.*

- L162: “degraded the hydrological, geomorphological, and ecological functioning of the hydrosystem.” This line and the following sentences would benefit from indicating a proper reference. Consider using ‘impacted’ instead of degraded if no detailed description (or reference) of those functioning can be given.
  - *We agree that without clear evidences regarding the degradation, except relying upon multiple notes and reports mainly published in the grey literature (and hardly available), one could prefer to employ the notion of "impact" which is probably less negatively oriented.*

- Fig. 3: What is the base flow in this case ? How is it obtained ? It is not discussed in the main text.
  - *The base flow is here associated with the continuous routine injection of 2 m^3s^{-1} in the new channel. As it is compared in Fig. 3 with injections of 70 m^3s^{-1}, for visibility on the plot, base flow is marked with a red dashed line (just above level zero). The text associated with Fig. 3 could specify the value of 2 m^3s^{-1}.*

- Fig 2 and 4 are not really the same… how have you decided to change the spatial structures ?
  - *We agree that there exist differences in the zonation of hydrodynamic parameters compared with the spatial distribution of gravel bars in Fig. 2. Most differences appear as specific additional parameter zones along portions of the new channel and the BGW that are partly clogged (which is not witnessed by gravel bars) with delayed or smoothed responses of local subsurface head values to infiltration. Additional zones have been*

delineated and parameters values have been adjusted during the calibration process to fit the local variation of heads in the subsurface.

Fig. 5. KGE: I understand that you show both RMSE and KGE, because KGE does not change much when the simulation clearly matches less to the observation. This is probably due to compensations in the KGE terms and can be discussed in the main text by giving the three KGE terms (variance and bias ratio, and correlation coefficient).

- *We agree that the 3 KGE terms could be mentioned when the results are described. It will be done in the revised version*

- Fig. 5 and 6.: What are the boundary conditions time series on West and East sides ? Would it be relevant to show it? How much of the water comes from the side and how much from the flood? It could help to add up West and East bank boundary conditions and injection time series in this figure, for instance by lowering the size of the scatterplot.

- *In the context of both calibration and validation periods simulated in this study, the lateral (East and West) boundary conditions might slightly vary (as shown for instance by the measured groundwater level before peak injections in Figs 5 and 6), but we do not have enough data (water level measured each 15 days) to better condition boundary conditions. Therefore, it is useless to build an additional Figure. That being said, under routine injection in the new channel, the transverse (East-West) hydraulic head gradient in the Island is almost flat, very few water entering or exiting the system by the East and West boundaries. During peak injections, the increase in subsurface water levels inside the Island might change this relationship, even though groundwater head maps in Fig. 8 show that the main flow direction is still from South to North. One could if needed calculate the mean flow rates that escape through the East and West boundaries and mention the result in the manuscript.*

- L 297: "Results from particle size analysis also helped to predefine variation ranges of crucial parameters, such as the hydraulic conductivity and retention curve parameters of the sediments and the exchange coefficient between surface and subsurface.". This is key, have you any validation data of the calibrated values? Particularly over the different patches? Did you use pedotransfers functions? Which ones?

- *We do not have specific data, for example from infiltration experiments, permeameter tests, or well interference testing, to check on the relevance of the calibrated hydraulic parameter values. It is worth noting that such experiments could reveal not representative of parameter values at the scale of the zones that we employ to define the spatial distribution of parameters at the Island's scale. Nonetheless, when the piezometers (that are used for calibration and validation) were installed, soil cores were taken and analyzed in the lab to determine textural and granulometric characteristics at different depths and locations. We then relied upon the Rosetta model from US Salinity Lab (Riverside) to link textural properties of soils with main hydrodynamic parameters. The only validation that we can propose is to state that these calibrated values allow to fit heads (which is a poor validation given the well-known equifinalities on groundwater head distribution and transients resulting from the heterogeneity of a system, boundary conditions, etc.). The revised version of the paper could be amended to better explain this specific point.*