

Interactive comment on “Impact of climate change on groundwater point discharge: backflooding of karstic springs (Loiret, France)” by E. Joigneaux et al.

Anonymous Referee #2

Received and published: 17 May 2011

1 General comments

Certainly, the article is original in both the object of study and the methods used. The study is at the local scale, but the methods used are interesting to the scientific community in general. However, the study is not complete, as the tests carried out are not sufficient to attain the objectives initially set. Thus, the authors are forced to end with conclusions which are very general and not directly derived from their results. However, this unsatisfactory result is partly due to the very originality of the study. Therefore, this is a very interesting work, which needs maturing. As a consequence, I recommend a

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major revision of the article, to complete the gaps and improve the justifications of the choices taken by the authors.

2 Specific comments

The description of the study area is not entirely clear for someone who does not know the area. It is necessary to inspect Figure 1 several times to understand it well enough to be able to understand the paper. I suggest the use of colors to enhance it.

In section 2 it is mentioned that there are observations of backflooding events from 1997 to 2001, but in the next section (data and methodology) this is not remembered. This is confusing because, at first, the reader does not know whether the authors have this data or just are citing the paper of Albéric (2004). This should be mentioned somewhere within section 3.

In section 4.2 the flow of the Dhuy is reconstructed using a lumped hydrological model, but not much is said on how the model was calibrated, over which period, etc. The correlation and the quadratic deviation are mentioned, but the Nash-Sutcliffe efficiency (NSE) is not used. I suggest using the NSE, which is related to the quadratic deviation, because, being normalized, it gives a better idea of the performance of the simulation. This section should be improved.

In section 4.4 the authors find a relationship between WT and backflooding events. To achieve this, it is considered that a given WT affects the flow of the river of the following 3 days, with different weights. But nowhere in the study it is explained why these assumptions were made. As the paper is written, this looks very arbitrary and should be justified.

Previously, the authors mention that: *although the appearance of backflooding is, on the daily scale, linked to the local river flows, its frequency is associated with the rainfall*

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sequences of the order of several years. Later, analyzing two different short periods, they suggest that it is important how the sequences of different WT are. Therefore, I understand that the occurrence of a backflooding events not only depends on the WT of the day, but also on the previous days and months. This is expected, as the Dhuy and the Loire are non linear and the karstic system is non linear too, therefore, the resulting system might hide a complexity that the method used is not able to comprehend. As a result, Fig. 7 shows that the abilities of the different WT to produce backflooding events is very low. For example, JJA4 and IJA5, which are considered that trigger these events, have a ability between 5 and 10

As it is mentioned that the Loire must be low to have an event, then, it might be interesting to study the probability of a WT triggering an event conditioned to the level of the Loire. This might improve the results.

Finally, section 4.5 inherits the previous problems. From the results of the previous section, it is difficult to really discriminate backflooding events from WT information, therefore, the conclusions drawn from the analysis of the climate simulations is not very solid. Furthermore, there is not much agreement between climate model runs, as Figure 9 shows. Therefore, the uncertainty inherent to climate simulations severely increases the uncertainty of the whole study.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 2235, 2011.