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Discussions

Interactive comment on "Impact of climate change on groundwater in a confined Mediterranean aquifer" *by* Y. Caballero and B. Ladouche

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Note for the authors and editor

The following review was written by a student of the MSc programme Earth and Environment at Wageningen University. As part of the course Integrated Topics in Earth and Environment, students are asked to prepare a review of a scientific paper. The supervisor of this review process is Ryan Teuling. The manuscript by Caballero and Ladouche was one of the manuscripts that was selected for this exercise. The review is written as an official review in order to comply with the course guidelines, but it should be considered by the authors as a regular comment which they can use to improve the

manuscript. I hope that this comment will positively contribute to the review process and that it will help the authors to improve their manuscript for possible publication in HESS.

A review of: "Impact of climate change on groundwater in a confined Mediterranean aquifer" by Y. Caballero and B. Ladouche (2015)

Summary

The manuscript by Caballero and Ladouche (2015) examines the impact of climate change on the groundwater resources of a confined coastal aquifer affected by major pumping utilization. The study takes place at the Roussillon basin located along the southernmost part of the French Mediterranean coast. With the use of a wavelet analysis approach the authors present an inverse transfer modelling method to describe the hydraulic behaviour of the confined multi-layer aquifer. Existing research is now mainly based on complex hydrodynamic models (see for instance the recent studies by Green et al., 2011 and Ali et al., 2012), which requires detailed descriptions of the geometry and hydraulic properties of the different layers for the aquifer. The suggestions are particularly focused on the relevance of using the wavelet analysis, because it seems that the sole of aim of predicting future groundwater levels could have been solved in an easier way. The second issue which could be improved upon is the estimation of the effective rainfall in the transfer model to predict the recharge. The method presented by Pinault et al. (2001) does not take into account the actual evapotranspiration (AET) and possible hydrophobic conditions, which could hampers the discharge and following recharge of the aquifer. The third and final issue concerns the description of the method in general. A short explanation of the chosen approach will improve the clarity of the paper. Detailed information can be found later in this review.

Caballero and Ladouche (2015) do not represent a major scientific breakthrough, but rather apply and simulate existing tools on a regional scale in a simplified and refreshing method. Nevertheless, the paper is definitely useful for the hydrological community, because an alternative method is presented. Therefore my recommendation to the editor is that this paper should be accepted, after a detailed revision of several issues

mentioned below. Overall the paper is written clearly and due to a varying length of sentences comfortable to read. However, improvements can be made in the representation of the figures, because some of them contain a lot of unclear and vague lines. Furthermore, the authors could have addressed their explanation of chosen methods slightly better. A more detailed description about the structure is elaborated later in this review (see section minor issues).

Major issues

Wavelet analysis

The first major concern in this paper is about the relevance of the wavelet method. The initial aim of this research is to study the impact of climate change on groundwater supplies. This requires statistical techniques that are designed to detect long term trends. The authors have chosen for wavelet analysis, which is basically designed to study fluctuations rather than trends. Wavelet analysis is a powerful tool for analysing localized variations of power within time series (Torrence and Combo, 1998), and is thus useful for short-term analysis. What is the reason that the authors choose for applying wavelet analysis? Simple time series such as the linear univariate autoregressive moving average (ARMA) or autoregressive integrated moving average (ARIMA), described by Tankersley et al. (1993), are methods more suitable for predicting long-term groundwater levels. Furthermore, the discussion that the authors pose regarding the downward trend in groundwater levels could also be related to long-term climate variations, which questions the contribution of applying wavelet analysis. I would personally have chosen to predict long-term groundwater levels via the aforementioned methods of Tankersley et al. (1993). Wavelet analysis can subsequently be applied to analyse pumping fluctuations.

Calculation of the effective rainfall

Secondly, improvements can be made in the calculation of the effective rainfall. Teuling et al. (2010) mention that larger evapotranspiration losses in summer should lead to a faster streamflow recession and thus hamper water storage and recharge. Due to a Mediterranean climate described by the authors in this paper this effect could be

even higher. The method described by Pinault et al. (2001) assumed that the actual evapotranspiration (AET) is equal to the potential evapotranspiration (PET) in order to estimate the effective rainfall. In my opinion this assumption is not valid due to the fact that the study area is in a Mediterranean climate, and the AET is therefore not equal to the PET throughout the year. The authors should reconsider the calculation with the PET and redo the simulation with a better estimation of the evapotranspiration.

What the authors also could take into consideration is the influence of soil hydrophobicity. This phenomenon could have a large effect on discharge and infiltration for Mediterranean environments (Martinez et al., 2007 and Cerdà et al., 2007). Even when the soil has proper surface characteristics, the infiltration capacity can be strongly reduced. If the authors are able to implement soil hydrophobicity this would strengthen the calculation of the effective rainfall.

General method

The last major issue which needs adjustments in my opinion is to what extent the authors elaborate on their method used in general. Although the authors provide an additional figure to explain the approach of the paper in a visual way (Figure 9), it is still hard to understand the general approach taken in the paper, as little information is provided on the motivation of certain choices. The difficulty lies in the fact that the different methods do not merge correctly into each other. To illustrate this with an example, it seems unclear to me how the authors go from wavelet analysis to the transfer model, but this might be simple because of a lack of knowledge from my side. I suggest to give a short explanation of each method and why this would be in line with the previous method.

Minor issues

This section describes the minor issues that were found in the manuscript. They can be solved easily without too much work.

- The title of the paper must reflect the major subjects and problems in the paper. In this paper the impact of climate change is explicitly mentioned in the title, but in my opinion the title is too concise. I would suggest to include the word 'pumping' in the title, for example "The impact of major pumping exploitation and climate change on groundwater in a confined Mediterranean aquifer".

- The following part provides a suggestion to improve the clarity of the figures. In my opinion, finding the right information in the figures can be tricky. Furthermore, in general some colour distinction between different lines can be made to improve the clarity of the figures. Some particular examples are listed below.

In Figure 3 for instance, the left y-axis represents the cumulative pumping, however this is still not entirely clear to me. I would recommend to the authors to explain this cumulated pumping in more detail, because according to me this is not showed.

Continuing, in the text on page 10117, line 3 the editors explain the reduction around 1982 which is showed in Figure 4b. Maybe it is also interesting to find out what the remarkable recovery of this reduction is in 1993? Because it is not observed for the Argelès water level for the same period.

Also Figure 6 (and Figure 14) could be improved upon, because they give an overload of information and it is also difficult to see the figures (due to the small font size). I suggest to split up the different graphs to make the information in the figures more clear.

- The order of the reference list is not entirely correct. As an example, the publication years of the papers by Pinault are not in chronological order (2001 – 2006 – 2001a),

whereas you would expect that the articles follow the same guidelines. Furthermore, the papers by Aunay are also not in a chronological order (2007 – 2006).

- On page 10117 the authors use the Scale-Averaged Wavelet Power (SAWP) method. Why would the authors choose this particular method? They do include a reference of Torrence and Combo (1993), but I would recommend to explicitly explain why the authors choose this specific approach. For example, the paper by Mwale et al. (2005) could help to strengthen the authors choice. This paper mentions that SAWP can be used to examine the modulation of one time series by another within the same time series and thus support the method by the authors.

- In the text on page 10114, line 20 the authors state that in Figure 2 the monitoring network of the groundwater is showed and that it has a coherent seasonal evolution of the groundwater levels. But these are not all the piezometers showed in Figure 1. Do the other piezometers show the same behaviour and what is the reason that they are not in Figure 2?

Detailed comments

In this section I will points out things that can be easily fixed, for instance typos or grammar issues.

Page 10110, line 2: add the word ‘the’ before ‘assessment’.

Page 10110, line 8: I suggest to alter the sentence a bit and start with: ‘In quantitative terms, the results illustrate’.

Page 10112, line 21: I suggest to skip the word ‘one’ and replace it by ‘aquifer’, because I do not prefer scientific writing in first-person.

Page 10113, line 11: rewrite ‘indicate to ‘indicates’.

Page 10114, line 6: change ‘A lot of work has been done’ into ‘Numerous researches have been done’.

Page 10114, line 12: replace ‘this’ by ‘the lithological heterogeneity’.

Page 10114, line 14: add the word ‘rather’ before ‘the distributary’.

Page 10115, line 3: leave out ‘Two piezometers’.

Page 10120, line 10: replace second ‘Rs(t)’ by ‘Rf(t)’.

Page 10122, line 2: replace ‘1980 2000’ by ‘1980-2000’.

Page 10124, line 7: I suggest to replace ‘the aquifer’s response to recharge’ with ‘the response of the aquifer by recharge’. I have found several more examples regarding this way of constructing the sentence and it is more pleasant to read when avoided.

Page 10130, line 23: I suggest using climate change instead of ‘CC’, because when I was reading this paper for the first time this was not entirely clear.

Page 10145: on the y-axes of Figure 3a and b ‘Pumpig’ must be replaced by ‘Pumping’.

Also ‘seasonnal’ must be replaced by ‘seasonal’ in both figures.

Page 10146: on the y-axes of Figure 4b and d the parameter ‘m’ should also be a turned axis (the same as Figure 4a and c).

Page 10148: in Figure 6c the word ‘pimping should be replaced by ‘pumping’.

Page 10151: in Figure 9 the word ‘Transfert’ should be replace by ‘Transfer’.

Page 10153: ‘seasonnal’ must be replaced by ‘seasonal’ (twice) in Figure 11.

Literature

Ali et al., 2012 – Ali, Riasat, et al. "Potential climate change impacts on groundwater resources of south-western Australia." Journal of Hydrology 475 (2012): 456-472.

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46.10 (2010).

Torrence and Combo, 1998 – Torrence, Christopher, and Gilbert P. Compo. "A practical guide to wavelet analysis." Bulletin of the American Meteorological society 79.1 (1998): 61-78.

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