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Hydrology and Earth System Sciences
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.

Introduction

The manuscript of Caballero and Ladouche is about modeling a confined aquifer in the Rousillon Basin in the Pyrenees Orientales, and investigating the influence of climate change on the piezometric head variations in this aquifer. To model this confined multi-layer coastal aquifer, the authors apply wavelet analyses, inverse modeling, a transfer model and five different climate models. This combination of methods is not performed very often on confined aquifers, but the authors claim that the area is geologically too complex to use a complex hydrodynamic model. This research is a good addition to existing research, because it makes it possible to model the piezometric level of this complex geological system (Roussillon Basin) (Aunay et al., 2006; Bento et al., 2009; Pinault et al., 2004). It is also a good addition to climate change related research of aquifers (Green et al., 2011), which is here very important considering the drinking water supply.

The method which is used in the study consists of different parts and is therefore also quite complicated. I think the manuscript would benefit from a better explanation of the modeling and the connection and coupling between the wavelet analysis, inversed modeling and the transfer model. This would improve the manuscript and the research will be easier to reproduce. This is especially important as this method has not often been carried out before and the manuscript wants to convey the model to the readers.

Furthermore it is a good research, because it is well elaborated, carried out consistent, has a nice calibration and validation (figure 11 of the manuscript) and a good reproduction of the piezometric heads is achieved by the wavelet analysis. Besides this, it

is socially a very relevant research as this area is intensively used for drinking water supply. I think the setup and elaboration of the research is quite good, I did not find any fundamental flaws. Therefore I want to advise the authors that this manuscript should be published with minor revisions as some parts can be improved. The suggestions will be explained in more detail in the next parts.

General comments

Climate models

To investigate the effect of climate change on hydrological systems it is a common method to use the output of climate models (Mastrandrea et al., 2010). In this study five different climate models are used to investigate the impact of climate change on the aquifer, namely: CNRM-CM3, HadGEM1, IPSL-CM4 and NCAR-CCSM3.0. These models have been chosen on the base of the Coupled Model Intercomparison Project Phase 3 (CMIP3) by Meehl et al., 2007. The research of Brands et al., 2011, gives a comparison between different ENSEMBLES global climate models for the region North West Europe. This study investigates 4 out of 5 chosen models by Caballero and Ladouche, not the NCAR-CCSM3.0 model. This intercomparison concludes (in the conclusion) that the HadGEM2 is the best predicting model for North West Europe. In the manuscript the first version of this model is used, but the second version came out as the best predictor for North West Europe. Why did the authors choose to use the first version of this model and not the improved second version? Furthermore Brands et al., 2011 conclude that CNRM-CM3 performed worst from all the twelve models they compared for this region. Why is this model a good choice for the climate models of this study? If it turns out to be the worst predictor in the study (biggest deviation compared to the mean or compared to the other models) I would suggest to take this model not into account for the calculations of the multi-model average.

Climate change or pumping?

The study area is influenced by major exploitation for drinking water supply for already 40 years. One of the results of this study is that the pumping has more influence on the piezometric head than the fall of the piezometric head caused by climate change. This is visible in figure 14 of the manuscript and discussed in the discussion part of the manuscript. But the title of the manuscript says: 'impact of climate change on. . ..' But as the piezometric head is more influenced by the pumping level, I would suggest that it is may be more appropriate to say something about pumping or water management in the title of the manuscript. As it turned out that the impact of climate change is not so big compared to pumping. So for example: 'The impact of pumping (or water management) on. . ...'

Thereby is the impact of salt intrusion not included in this study. Salt intrusion in coastal confined aquifers is maybe even more important than the fall in the piezometric head considering climate change. In the paper of Dorfliger, 2003, the very important role of coastal aquifers in France is explained and among others functions it is a very important source for drinking water supply. Therefore it is really important that salt intrusion is investigated in these coastal aquifers. Both the influence of climate change and pumping on the salt intrusion needs to be investigated, to get a good overview of the influence of both processes on this aquifer. I think it would have strengthened the manuscript especially in a view of climate change if the salt intrusion was taken into account, as was done in Pinault et al., 2004, for example. In this paper the authors use an inverse model to characterize a coastal karst aquifer, the hydraulic head and salinity are both investigated. It would have been a good addition to your study. But as the authors already state a more detailed model would be needed for this. So I would suggest that this should definitely be a recommendation for future research. Furthermore the vulnerability of this aquifer with respect to climate change has to be put in a context now as the salt intrusion is not taken into account.

Water balance

For a good and complete hydrological model you need all the different component from the water balance to be included, as for example in Iribar et al., 1997 (modeling a confined aquifer in Spain). All the different components of the water balance are mentioned in a paper of Beven and Kirkby, 1979. The components which are taken into account in this study are shown in figure 9 of the manuscript. There you see that some components of the water balance are not taken into account. Because the only recharge/input flux of the study is effective rainfall and river recharge (are combined further in the study). But I miss input from deeper groundwater into the aquifer, like water from the mountainous areas, or is this completely absent in this region? If this is the case, it would make the manuscript more clear if you mention this. Because maybe this flux is even more important than the flux from effective rainfall in your study area. Furthermore I miss for example the runoff, the water from outside the catchment which flows into the area and irrigation. Is the water balance conserved in this study? If so, then I would suggest to state it in the manuscript so that it is clear. If not, explain why this is not needed in this study.

Piezometers

The confined aquifer is modeled by a network of 12 piezometers, the modelling study is based on the data of two of these piezometers (Perpignan and Argeles). Those two piezometers are not located in the typical sediment of this aquifer. And when looking at figure 2 of the manuscript, these two piezometers are not really representative for the area. The authors already state that in the discussion. But I was wondering why did the authors still chose these two piezometers? And not more representative ones, I miss this argumentation in the manuscript.

Aim of the manuscript

The aim is stated in the introduction: 'This paper aims to present a modeling method based on wavelet analysis . . .. .pumping exploitation'. So it says that the method is the most important message of this study. But the results, discussion and conclusion are more about this specific aquifer and the data obtained for this area. I would suggest to add a paragraph in the conclusion just about the model and how it can be used in other areas and what are the advantages and disadvantages in general and not specific for this area. Because as the authors state in the aim, that should be the most important message from this manuscript.

Specific comments

Page 2, line 6: 'two representative piezometers', is in contradiction with a part of the discussion chapter (page 23, line 8 to 19). Here is first stated that it can be questioned if the two piezometers are representative for the whole aquifer. Secondly only the results for the Argeles piezometer are taken representative for the whole aquifer, and not the data from the Perpignan piezometer.

Page 5, line 11: 'The data acquired indicates that the groundwater. . .'. Spelling mistake the 's' after 'indicate' is missing.

Page 5, line 28: 'They could be. . .. . .from private wells.'. It is a bit contradictory with the line before. Because there is said that the withdrawals from the Pliocene-Quartinary aquifer other than drinking water are less well documented. Then the next sentence starts with the word 'could', I would suggest to use 'will be around' or 'are expected to be around' instead of 'could.'

Page 7, line 1: 'Two piezometers The daily variation of the water levels observed. . ..'.

The first two words ‘Two piezometers’ should be left out.

Page 14, line 2: ‘1980-200 (reference)’. I would suggest to write down the arguments why you chose this period as a reference period. Because for example climate change can already be present in this period, so why not the period from 1970-1990 as a reference?

Page 14, line 13: ‘near term (2020-40)’ I would write down ‘(2020-2040)’.

Page 14, line 14: ‘medium term (204060)’. It should be ‘medium term (2040-2060)’.

Page 15, line 5: ‘These results were obtained without taking into account any recharge by the Tech river.’. So the piezometric level is reproduced well without the recharge from the river, what is the cause for this? Normally the most detailed information is needed to get the best model predictions. On page 21, line 1 to 3, it is said that the Tech contribution is synchronous with the recharge. But does it have the same catchment? And how much does the river contribute to the effective rainfall?

Page 19, line 16: ‘They also allowed us develop. . ..’. Add ‘to’ between ‘us develop’, so ‘us to develop’.

Page 22, line 10: ‘. . .by the projections in Figure 13 which show that in the medium term the simulated piezometric level for Argeles.. . ...’. Should this not be Figure 14 of the manuscript? Because that figure shows piezometric levels and Figure 13 of the manuscript does not.

Page 24, line 1-9: ‘This article. . ... measurement series.’. This paragraph is more a summary of the manuscript than a conclusion about the article. I would suggest it can be skipped and just start with the conclusions.

Figure 1: colors would be a good addition to this figure, it would make it clearer and therefore better to read. Also the French database index code would be a good addition, because now it is not possible to see where the piezometers from Figure 2 are topographically located.

Figure 2: First of all is it not clear which lines belong to which axis. Is it only the one with ‘FIGUER’ in its code? Or also the one above that line. It can be shown clearer. Following from that if the highest two lines (FIGUER and PONT) or only FIGUER lines are on the right axis, I doubt their representiveness for the whole aquifer as the order of magnitude is very different from the ones on the left axis. Secondly, this figure is very hard to read without colors, the different lines barely differ from each other. Third, the lowest lines (mainly between 0 and 2 meters) are too close together, it is not possible to read them out separately. As a last comment I would suggest to use real names in the legend for the different piezometers, this would make it more clear and understandable for people who do not know this area very well. The French database index code can be placed between brackets.

Figure 3: I think this figure is too complex. I would suggest to place the permanent pumping line in a separate figure, now the figure contains too much information, which makes it more unclear and confusing. Because what is the water level line now, the one from the permanent pumping or the effective rainfall? Or a combination? Furthermore is the difference between the ‘cumulative volume’ line and the ‘water level’ line not visible.

Figure 4: Especially figure 4a and 4c are too small to read out the differences between the y1 and y0. They would be clearer if they were bigger. Figure 6: I would suggest to leave out the parts of the axis for which there is no data. Because then you can see in more detail the data for the part where it is available. Now you see five half empty graphs.

Figure 7: I should write down in the caption what the horizontal and vertical bars are. Now this figure is not self-explanatory.

Figure 8: This figure is too small to see the difference between the seasonal and permanent pumping line. And what are the + signs in the lines in figure 8a? These figures can be made a lot clearer.

Figure 10: Colors would make it easier understandable and also easier to distinguish the different lines in especially figure 10a and 10b.

Figure 11: Again colors would be a good addition and in figure 11a it is hard to see the distinction between the modeled and measured line.

Figure 12: Colors would make it easier to read and distinguish the different lines, especially in figure 12b.

Figure 14: The top two graphs do have a caption 'Without pumping' but the graphs beneath miss the caption 'with PP and without SP', and 'with PP and SP'.

References

Aunay, B., Dörfliger, N., Duvail, C., Grelot, F., Le Strat, P., Montginoul, M., Rinaudo, J. D. (2006). Hydro-socio-economic implications for water management strategies: the case of Roussillon coastal aquifer. In Colloque international sur la gestion des grands aquifères, 150è anniversaire de la loi de

Darcy, 50ème anniversaire de l'AIH (Association Internationale des Hydrogéologues) (pp. 9-p).

Bento, S., Fatima, D., Errahj, M., Faysse, N., Garin, P., Audrey, R. F., ... Varanda, M. (2009). Farmers' relations to climate variabilities and changes: the case of groundwater users of coastal aquifers in France, Portugal and Morocco. In 9th Conference of the European Sociological Association.

Beven, K. J., Kirkby, M. J. (1979). A physically based, variable contributing area model of basin hydrology/Un modèle à base physique de zone d'appel variable de l'hydrologie du bassin versant. Hydrological Sciences Journal, 24(1), 43-69.

Brands, S., Herrera, S., San-Martín, D., Gutiérrez, J. M. (2011). Validation of the EN-

SEMBLES global climate models over southwestern Europe using probability density functions, from a downscaling perspective. Climate Research, 48(2-3), 145-161.

Dörfliger, N. (2003). The state of the French Mediterranean coastal aquifers.Tecnología de la Intrusión de Agua de Mar en Acuíferos Costeros: Países Mediterráneos. Instituto Geológico y Minero de España, Madrid. Green, T. R., Taniguchi, M., Kooi, H., Gurdak, J. J., Allen, D. M., Hiscock, K. M., ... Aureli, A. (2011). Beneath the surface of global change: Impacts of climate change on groundwater. Journal of Hydrology, 405(3), 532-560.

Iribar, V., Carrera, J., Custodio, E., Medina, A. (1997). Inverse modelling of seawater intrusion in the Llobregat delta deep aquifer. Journal of Hydrology, 198(1), 226-244.

Mastrandrea, M. D., Field, C. B., Stocker, T. F., Edenhofer, O., Ebi, K. L., Frame, D. J., ... Zwiers, F. W. (2010). Guidance note for lead authors of the IPCC fifth assessment report on consistent treatment of uncertainties.

Meehl, G. A., Covey, C., Taylor, K. E., Delworth, T., Stouffer, R. J., Latif, M., ... Mitchell, J. F. (2007). The WCRP CMIP3 multimodel dataset: A new era in climate change research. Bulletin of the American Meteorological Society, 88(9), 1383-1394.

Pinault, J. L., Dörfliger, N., Ladouche, B., Bakalowicz, M. (2004). Characterizing a coastal karst aquifer using an inverse modeling approach: The saline springs of Thau, southern France. Water Resources Research, 40(8).

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