

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

Anonymous Referee #2

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The manuscript “Impact of climate change on groundwater in a confined Mediterranean aquifer” by Caballero and Ladouche presents a comprehensive methodology to estimate the impact of climate change on confined aquifers. The methodology is applied to a coastal-multilayer aquifer, located in the South of France. Motivations of the research (in particular the use of wavelet analysis and transfer function approaches, instead of physically-based modelling) are well explained and appear to be suitable in the context of the case study.

However, the general structure of the paper is quite confusing: my first suggestion is to develop a methodological section that includes all the developed methodologies (wavelet analysis, transfer function model, scenarios etc.) and the links among them, neglecting the partial results. A figure showing the modelling flow chart (similar to Fig-

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ure 9) could improve the readability of the manuscript. For each method the following information should be clearly given: a) goal of the model (what the Authors wish to compute/estimate); b) motivations about the choice of the method; c) brief but comprehensive presentation of the method; d) input variables; e) output variables; f) method adopted to estimate the related uncertainty (if necessary). Most of this information has already been provided in the current version of the manuscript, but in my opinion are not well organized. A more schematic structure of the methodological section (avoiding to alternate methods and results as now, see for example the section 3.2 about the wavelet analysis) could help both the Reader to follow the paper and the Author to present methods and results without duplications.

Apart from the comments on the structure of the article, I have some major concerns on the basic assumptions and, consequently, on the adopted methodology and the results.

1. The starting point of the study is clearly stated at pag. 10115 line 20: “We assume that annual variations in the piezometry are linked to the SP, while the long term trend is linked to the PP from drinking water withdrawals”. On the one hand, this is clearly in contrast to the statement “the groundwater level’s variation is affected by recharge from rainfall, and pumping for domestic, industrial or agricultural needs” (pag. 10115 line 5). On the other hand, in the conclusions the Authors claim, “the predominant influence of pumping, as the main factor controlling drawdown of the water table, has thereby been highlighted” (pag 10132 line 13). In other words, starting assumptions and conclusions are somehow coincident. In my opinion, this discrepancy invalidates the whole processing analysis, at least for the case study. I will give more details about my concerns in the following.

2. The assumption that seasonal variations of h are linked to SP and long-term trend to PP must be strongly supported (and currently it is not). Figure 3 shows the water table variation at Perpignan and Argelès (by the way: what does the arrow “effective rainfall” mean?). Of course the water table is decreasing, but the fact that such a decrease is

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due solely to an increase of PP is not demonstrated. Moreover monthly effective infiltration (as shown in Figure 13) take place during late autumn, winter and early spring), this it presents the same periodicity of the SP pumping, preventing from distinguishing (also after filtering through wavelet analysis) the contribution to the seasonal water table variations due to effective infiltration and pumping. Ascribing the whole variability of the water table fluctuations to abstractions (permanent and seasonal) necessarily lead to wrong (or trivial) results on the impact of climate change: if I assume (and consequently calibrate the modelling chain to estimate the impact) that the whole past variability is due to pumping, certainly the impact of climate change in the future will be null or very small.

3. A more detailed analysis of the trend of precipitation and temperature in the past should be included in order to discuss the observed variations of the piezometric heads on a sound basis.

4. The assumption on the dependency of the variation of the piezometric heads on the abstractions strongly impact the calibration of the coefficients λ in eq. 1, as QSP and QPP have been estimated through the wavelet analysis under the hypothesis that they explain the whole variability of h .

5. More details should be given about the estimation of the effective rainfall. Some terms in eqs 3 and 4 ($R_{\sigma} \tau_a(t)$) have not been explained. Moreover it is not clear how the slow and fast components have been separated each other: in particular, it is not clear to me how the factor $\alpha(t)$ has been calibrated. Please, clarify this point.

6. Concerning the convolution equation, I have the following doubts: a) The spatial resolution adopted for applying the convolution equation is not clearly reported, as well as the time step. Please, clarify this point. b) the recharge areas affecting the time changes observed at the selected piezometers could be far from the location of the piezometers itself: therefore the variability in space of the precipitation should be analysed and discussed before using the computed effective infiltration in the convolution

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equation. In general, it is not clear the extension of the areas that have been taken into account for computing the transfer function of each piezometer; c) A storage term has been neglected. This can be acceptable for the confined sectors of the aquifer, but not for the aquifer as a whole. As the Authors reported in the introduction, some sectors of the study area are unconfined: in this case the storage term cannot be neglected. This issue should be cited and discussed. d) The QRIV in eq 1 can be strongly non-linear, as exchanges between surface and groundwater depend as a first approximation on the difference between piezometric and hydrometric head. It is not clear to me how this issue has been taken into account in the implementation of the convolution model.

7. The climate scenarios, based on 5 selected climate models have been developed using the perturbation method. However, I have some concerns about the validity of this method, particularly for the case study (located in a Mediterranean climate area). In fact the Authors modify the observed time series of precipitation and temperature only on the mean monthly variation of such variables with respect to the baseline. This implicitly assumes that in the next future only the mean values are expected to change, whereas the variability (i.e. the shape of the probability distribution) and the time autocorrelation is considered constant in time. In the context of climate change impact studies, this is a very strong limitation, as such an assumption contrasts to the majority of the projections on the Mediterranean area, that is expected to be characterized in the next decades by an increase of the extreme both positive and negative. In particular, drought episodes are expected to be increasing in the probability of occurrence, duration and intensity. A simple bias-correction could not take into account these kinds of changes.

8. The impact scenarios have been developed under the assumption of stationarity of SP and PP. Moreover it has been assumed that abstractions do not depend on the actual precipitation, therefore neglecting the secondary effects of the climate change. Although the majority of the impact studies neglect this issue, in my opinion in the framework of this case study possible increase of the abstractions due to a decrease

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of the recharge to the aquifer should be taken into account. Please, refer to these papers as an example: Treidel et al., 2012; Leng et al., 2015; Fischer et al., 2007, Elliott et al., 2014)

9. As shown in Figure 7, the estimate of the pumping is affected by uncertainty. Such an uncertainty should be taken into account in the discussion.

On the ground of these comments, although the manuscript is interesting and describes methodologies not new but applied in new ways, I suggest the Editor to decline this contribution in the present form. Moreover, I suggest the Authors to revise the paper along the lines exposed in this review and in the reviews published in the public discussion and to resubmit it. Considering the abundance of information, methodologies and results appearing in the manuscript, in my opinion it could be useful and more effective to split the submitted paper into two different papers: one devoted to the methodology itself (the combination of wavelet analysis and transfer function), including the calibration and validation procedure. The second one devoted to the impact of climate change on confined aquifer. Such a splitting will allow the Authors to get more insight into the details necessary to exhaustively describe the study. My feeling is that many details necessary to be convincing have been neglected for space reasons.

Minor issues are listed here below.

- Pag. 10109. The title of the paper is too general. A more detailed title is in my opinion more suitable
- Pag. 10120 Please, check carefully the notations of eqs (3) to (6)
- Pag. 10111 line 13. Maliva et al. is reported as 2011 here and as 2010 in the references
- Pag. 10115, line 1. "Two piezometers" should be removed
- Pag. 10115, line 13. Please, reformulate the sentence correctly.

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- Pag. 10119, line 1. “to characterize” instead of “to characterizing”
- Pag. 10122 line 2. “1980-2000” instead of “1980 2000”
- Pag. 10122 line 13. “2020-2040” instead of “2020-40”
- Pag. 10122 line 14. “2040-2060” instead of “204060”
- Pag. 10130, line 5. “was” instead of “were”
- Fig.2 For clarity reasons, in my opinion Fig. 2 should be put in colors.
- Fig. 3a title of the y-axis. “Pumping” instead of “Pumpig”. “seasonal” instead of “seasonal” - Fig. 6c. “pumping” instead of “pimping”
- Fig. 7. It is not clear to me how the error bars (both for $y_1 - y_0$ and for pumping) have been computed. Please, clarify this point in the manuscript
- Fig. 9. “Transfer model” instead of “Transfert model”

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