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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Technical note: Water table mapping accounting for river-aquifer connectivity and human pressure" *by* Mathias Maillot et al.

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We thank you for this additional comment which helped us to identify the remaining unclear points of this paper. Please find hereafter a new rebuttal with the last version of the manuscript (supplements pdf).

I understand you do not want to make the suggested additional work, because you simply modified this question. It would have been more honest saying it explicitly: And

it is a pity: the paper would greatly benefit from highlighting the improvements induced by the proposed methodological refinements.

We apologize for the misunderstanding raised by our first answer to this point. Indeed, we want to keep our paper as a technical note. The comparison of the different interpolation methodologies is already the main topic of studies that are cited in to bibliographic review of the introduction (cf Introduction P2 L7-9). This comparison of kriging with other estimators was carried out in Varouchakis et al. 2013, we also added references that supports the choice of kriging methodologies (Emadi and Baghernejad, 2014, Adhikary and Dash, 2017 and Ohmer et al., 2017). The choice of UZD as a variable instead of hydraulic head is justified further in introduction (cf introduction P2 L33-35).

Regarding question (ii) stated at the end of the introduction: you explicitly replied that you do not address this question in this paper. Fair enough, but then, the question should be removed!!

Thank to point this out, we want to clarify our response. You must be referring to the question iv of the introduction which is: "(iv) finally, what are the consequences of such methodological refinements on produced maps of water table linked to hydrological events?". We believe that the paper answers this issue with a sensitivity analysis of the disconnection criteria. We remind that this paper is a technical note aiming at fully describing the method, and not to analyze the system functioning. This is what we meant by our former response: Âń These aspects are not developed in this study. We believe that such a development should constitute further works of the application of this study that we want to keep focus on the mapping issue. Âż.

P1 L14: Refer to Brunner et al. (2009) for these three connectivity status.

P1 L14-15: Done. We also added Dillon Liggett (1983) and Fox Durnford (2003) where these three connectivity status are described.

P1 L20: The reference to Bresciani et al. (2016) should better be put at the end of the sentence together with the reference already there.

Done, P1 L21

P2 L14: Bresciani et al. (2018) did not use fuzzy logic or neural network. They used the diffusion kernel interpolation method. Please correct this.

We add this methodology to our review of mapping methodology. All these information are added P2 L18-22

P2 L27: "Thereafter" should be removed and a new sentence should start.

Done P2

P2 L27: Change "shallow groundwater" for perhaps "relatively humid climate".

P2 L33-35 These words were replaced by the quote from the Haitjema and Mitchell-Brucker 2005 paper.

P4 L7-8: I still do not understand the reason for smoothing the DEM. How are your UZD measurements taken? If they are taken from dipper, I would think that the exact topographic level must be used, and not a smoothed one.

The reason for using a smoothed DEM is that with a high resolution DEM, topography contains more noise than water table. To remove this artifact we did a smoothing of the high resolution 25x25m DEM. This methology was proposed by Mouhri et al. 2013.

P4 L 8-9: Again, clarify the search radius. "in agreement with: : :" is cryptic. What did you precisely do?

The search radius is defined regarding two conditions: i) the DEM has to be smoothed enough to remove its high-resolution noise and ii) the information of river water level must be conserved in the final product. We tested many radius in order to fit with the conditions and found out a value approximating the average stream width (200m). We have added this explanation P4 L15-18

P4 L10-13: This is also unclear: what did you do about this bias?

The bias is induced by the smoothing effect. It is the difference between the true wellhead elevation and the smoothed DEM elevation. Thank to your comment, we found out that there was a mistake in the text P4 L23 where "between DEM data" was replaced by between smoothed DEM data".

P4 L18-29: This part is still very much unclear. First of all, there should be a conceptual discussion about the effect of pumping. Namely, it must be recognized that the effect of pumping is not only punctual. Hence, nowhere will the water table be really natural/unaffected. In this view, it is not even clear why any data should be removed. Secondly, you write that the location of pumping wells is required. Do you have these data? It seems not, otherwise you would not need to use a threshold value on UZD. But this should be said! Thirdly, a rationale for the employed 10 m threshold is still lacking. Fourthly, you write that the variographic studies are performed on the second category of data. Does this mean that the rest of the analysis uses all the data from both categories?

Some justifications were added P5 L3-7 to explain the identification of affected piezometers. The Dupuit-Forchheimer assumption is not valid within the capture zone of a pumping well. Therefore, the head is not hydrostatic (Grubbs et al., 1993), the piezometric value and the topography are not correlated within its capture zone. We noted that there is no correlation between the piezometric value and the topography for the locations where UZD exceeds 10 m (Fig.a). Without the exact knowledge of pumping wells location, we assume that these samples are located within the pumping wells capture zone. Indeed, this differentiation was only used to construct the experimental

variograms to which the variogram model are fitted. The rest of the analysis uses all the data from both categories.

P6 L19-30: Regarding the riverbed thickness (i.e. clogging layer), you replied that you assume that there is no clogging layer. Then, this assumption should be explicitly mentioned in the text.

P7 L3: mentioning the absence of clogging layer and its justification.

P10 L16: Here I guess you are referring to the transition case, and not the disconnection case. The way you wrote this is quite confusing.

We agree that mentioning the transition case improves the comprehension, it was corrected P10 L24.

P10 L30: Why would the optimal value be reached when the relative numbers of matched cross sections are equal?? This would only be true in the very special case where there are equal numbers of connected and disconnected cross sections.

We believe that in either case, the choice of the objective function to determine the optimal disconnection criteria is arguable. We would advice our readers to choose the more appropriate one depending on the case study, this was added P11 L6-10. We choose to change the used objective function from absolute number of cross-sections validation to relative number of cross-sections validation because we considered that it

would make it less dependent of the cross-section number. Indeed, this change does not change our conclusion because in our case, the number of disconnected sections is the same as the number of connected sections.

Fig. 2: River names are still lacking

Adding the river names in Fig.2

Also note that my comment on "favor river infiltration towards the aquifer" in Section 3.7 was not addressed at all. Please address it.

We apologize for the omission of this last point. We understand your comment about the map showed in Fig.6.b where stream is gaining in many portions of river. This issue is caused by the representation of the riverline of Fig.6 that conceals the local water table. Furthermore, the river water level is not represented in Fig.6.

We propose to add the information about the SW-GW relationship in connected sections (gaining, loosing and both) of river in the final maps showed in Fig.6. As it is mentioned in the corrected text (P12 - L1-5), the SW-GW relationship is established analyzing the difference between river water level and water table at a 2 pixels (50 m) distance from the river. The obtained relationship is attributed to riverline representation as a color code in Fig.6. Thanks to this new representation, the discussion about the effect of hydrological event on water table is reinforced (P12 L6-13).

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-101/hess-2019-101-AC3-

supplement.pdf

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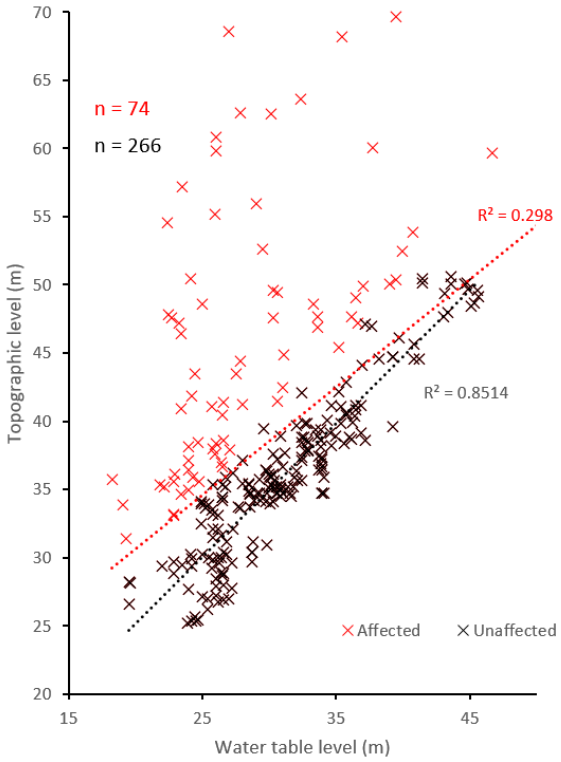

Fig. 1. Fig. a: Water table level compared to topographic level at LWC sampling points. The red symbols correspond to the samples affected by the pumping wells, the black symbols correspond to the unaffected