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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Time-lapse cross-hole electrical resistivity tomography (CHERT) for monitoring seawater intrusion dynamics in a Mediterranean aquifer" *by* Andrea Palacios et al.

Anonymous Referee #1

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The paper describes the application of CHERT (Cross-Hole Electrical Resistivity Tomography) in order to monitor the seawater intrusion in the time during a very long period (two years). I think the described technique is an interesting methodology in order to contribute on the monitoring by DC methods when the observed phenomena is deeper then the normal ERT capacity of observation. Anyway, the use of CHERT needs more attention because the distance between each boreholes is a crucial point. Even if the authors did a good job, from my point of view, they should rewrite some parts because there are a lot of information that are not correlated with the DC measurements or the text was not sufficient to explain the relations (i.e. the wind velocity and the wave height).

According authors results, the classical ERT method is not able to observe deep phenomena due its low resolution compared with the depth. Therefore, the CHERT is a good opportunity to help the DC method to observe deep phenomena, even if it is a little bit invasive because it needs a borehole. On the contrary, to monitor an aquifer or seawater intrusion the boreholes are necessary. . .and the application of steel electrodes is a good opportunity. The work is very interesting because the authors describe a long monitoring approach by CHERT method even if the apparatus (steel electrodes) are located in an aggressive scenario due to high salinity in the aquifer. From the showed results, the EC images seems good in the time, even if the authors miss information on the data quality and if there was some decrease of electrical contact between the electrodes and the subsoil in the time. Moreover, the authors miss also some important consideration and description on the acquisition (i.e. what is the electrode distance?). The authors introduced the used protocols but they did not described which was the best one. A large analysis and comments are important when a new approach is introduced. From my experience, to merge different protocol all together is not the best solution. The authors did not wrote any comment on the distance between the borehole (borehole distance) may be for their low experience on CHERT method. This is a crucial point on CHERT. From my experience and experiments that I did in my lab, the borehole distance should be maximum the half of the largest distance between the electrodes (distance from the superficial electrode and the deep one). If the distance increases the quality of the data decreases (the figure 3e highlights this point). In fact, the deep high EC value zone is well observed from the 35m to 80m from the coast (figure 4). On the contrary, this zone is not detected between the two boreholes (N225 and N325). I suppose that the low data coverage between the boreholes, due to large distance (21.5m), should be a reason for that. In fact, on the water electrical conductivity measurements taken on water samples from piezometers highlight high EC values on N225 hole. Finally, I suggest to improve the description on the CHERT data quality and to reduce the part where the authors describe the correlation between the result and some data far from the electrical resistivity data (i.e. wind velocity and

wave height). Therefore, I suggest to add a strong paragraph on the CHERT method.

Here is a list of detailed points that in my opinion deserve attention: Line 50: I suggest to describe better this sentence.

Line 57-62: Even if there are not CHERT works on SWI phenomena, but I suggest to cite some papers on the CHERT application, in order to highlight the potentiality of this methodology.

Line 89-90: I suggest to cite some papers on this point or to indicate a personal information on that.

Line 99-100: the cited paper is not completed; there is only authors and title. . .the same for the paper Martinez et al.

Line 127: There are several software that visualize the distribution of the apparent resistivity data as a pseudosection view.

Line 131: It is not clear why the acquired data were 5800 (line 113), but the data used for the inversion were 2677. I suggest to describe this point.

Line 132: The authors indicate “forward modelling”, but in the paper there is not indication on a forward approach.

Line 258: Figure 5n-p

Line 232-233: If the authors would like to combine the “wave activity data” with the EC data, I suggest to describe why these data can be compared with the SWI phenomena.

Line 253: The figure 7a has a different scale then the figure 5. . .also the figure 8.

Line 265: I suggest to delete the discussion on the wind velocity. It doesn’t add some important information on the paper.

Line 292: why steel electrodes only in the piezometer 25 m length corrupted the IL data. The problem should be the same everywhere. I suggest to improve the information on

the text.

Line 355-414: I suggest to merge the paragraphs Time lapse study (long term, short term saltwater event). Moreover, I suggest to make a sketch on the figure 5 in order to detect the three main zone as the water samples data highlight: upper, transition and lower. Moreover, I suggest to explain better or delete some “weak” part. In example, the “freshwater event” is not well observable in the ratio bulk EC model (figure 7a). The “storm event” is not so clear and there is some confusion between the indicated period and the figure 8. I suggest to rewrite the paragraph.

Conclusions: Even if I agree the different points, I suggest to rewrite some sentences (i.e. point 4) after the revision of the paper.

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