Response to comments from Referee 1

Black: Our comments and responses

Blue: Comments and questions from Referee 1

We would like to thank the referee for the thorough review work. We appreciate the referee acknowledging the difficulties of working in a hostile environment for steel electrodes We generally agree with specific comments, which we address in detail later.

We acknowledge the comment that "Too much information not correlated with the DC measurements or relations not sufficiently explained", but we disagree with the resulting recommendation 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)." In fact, it is the hydrological interpretation of the CHERT inversion that we consider the most valuable contribution of this study, which has led to a review of the current seawater intrusion paradigm as explained later in the specific comments. One of the novelties of our paper is the use of CHERT in a coastal aquifer context. However, the most important finding is the field demonstration that seawater intrusion differently than in textbooks. Moreover, it is also nice that CHERT identifies many other processes (e.g., storm surges). The fact that you can use CHERT to analyze the impact of so distant causes as the impact of sea waves is a nice rare example of the unity of Earth Sciences. However, we take this comment by the referee as an indication that our description can be improved and we will rephrase the corresponding parts of the text to improve the manuscript.

"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."

We follow the strategy proposed by Bellmunt and Marcuello (2011) for the quality control of the data based on the comparison between normal and reciprocal measurements. In our case we choose a threshold of 10% difference between the normal and reciprocal data in order to keep the measurement. Figure 1, that will be shown and introduced in the final manuscript, shows the time evolution of the data percentage that satisfies our quality control. The panel between boreholes PP20 and PP15 is the one with a lower quality likely due to its proximity to the coast. It also corresponds to the zone where lower resistivities cover a thicker vertical zone. The decrease in data quality with time is probably related to corrosion processes of the electrodes in contact with marine water. Furthermore, the electrical contact resistance between the electrodes and the subsoil was checked before each data acquisition. Although the specific values of each pair of electrodes were not recorded, they were low in general. The deepest electrodes, in contact with the SWI, had contact resistances values in the order of 1 kohm and the ones closer to the surface had values of a few 10's of kohm.

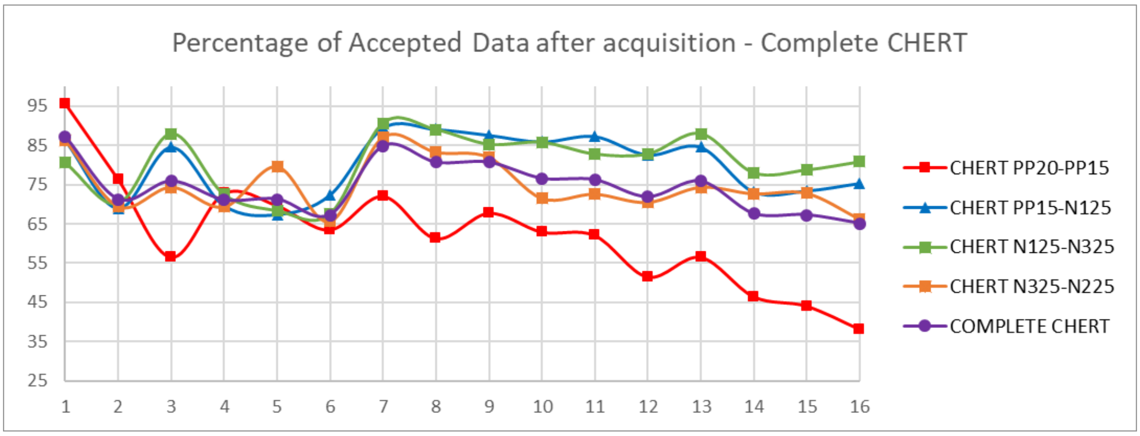

Fig 1. Percentage of accepted data points after the quality checking of the acquired data.

"the authors miss also some important consideration and description on the acquisition (i.e. what is the electrode distance?)."

We thank the referee for raising this point and apologize for the missing information in the article about the electrodes distances. All piezometers have 36 electrodes and the distance between electrodes is 70 cm, 55 cm and 40 cm in the 25 m, 20 m and 15 m depth piezometers respectively. A new paragraph describing the acquisition geometry will be added in the revised version of the manuscript.

"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."

In our experience (Bellmunt et al., 2016) it is better to use different configurations (dipole-dipole, pole-tripole and Wenner) with different sensitivity patterns in order to obtain the maximum information about the subsurface. Moreover, we were aware that given the environment in which the steel electrodes were located some of the data measurements could be not repeated over time, so we decided to maximize the number of acquired data. The different configurations used were already described by Zhou and Greenland (2000) and Bellmunt et al. (2016). We also want to make it clear that the focus on the present work is not on experimental design. In the revised manuscript, we will point to the relevant literature.

"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".

We are aware that a key point to consider when defining a CHERT experiment is the aspect ratio between the horizontal distance of the boreholes and the maximum vertical distance between the electrodes located in each borehole (e.g., Labrecque et al., 1996). We agree with the referee that smaller values of the aspect ratio will be better, but the location of the boreholes was conditioned by several factors including logistics and requirements for other monitoring methods as well as experiments planned at the experimental site. Furthermore, there is a trade-off with the overall investigation area implying that larger borehole spacings are sometimes motivated. Nevertheless, Labrecque et al., (1996) suggest that the aspect ratio should be between 0.5 (ideal situation) and 0.75 maximum. Numerical simulations by Hagrey (2011) suggest that larger values of the aspect ratio can be used if constraints about the resistivity structures are considered during the inversion procedure. In our case the aspect ratio for the different panels considered ranges from 0.6 to 0.8.

Comments on specific sentences:

Line 50: "But, ironically, it is consistent with the fact that salinity profiles measured in open wells often display salinities much lower than that of seawater. So, it might be questioned whether the current paradigm is wrong." Describe better.

This statement refers to the fact that, on the one had, surface ERT derived conductivities and those often measured in fully screened wells are much lower than what should be expected with the current paradigm, according to which freshwater floats above a seawater wedge. Given this paradox (field measurements with low salinities at the intrusion wedge and paradigm with high salinity), one must conclude that either the measurements or the paradigm are wrong. One of the results of this paper is that all of them are wrong (surface ERT underestimates salinities, open wells do not really measure true aquifer salinity, and SWI is a lot more complex than the current paradigm). But, at the introduction, we feel it is sufficient to formulate the paradox.

We will revise the paragraph in the final version (we will wait for other referees) to express something along these lines:

> Beaujean et al. (2014) and Nguyen et al. (2009) showed that ECs derived from surface ERT may contain important errors due to poor resolution at depth. The computed EC at depth is typically much lower than what would be expected if pores were filled with seawater, which is the generally accepted paradigm of seawater intrusion (a seawater wedge beneath fresh water). Paradoxically, surface ERT results may be consistent with salinity profiles measured in fully screened wells, which often display salinities much lower than that of seawater (Abarca et al., 2007). It is clear that either measurement methods, or the current paradigm, or both, need to be revised.

Line 57-62: "CHERT has never been used for monitoring SWI, most likely due to cost constraints, the high risk of electrode corrosion in saline environments, and because it typically covers a smaller scale than surface ERT or time-domain electromagnetics (the most typical geophysical technique in saltwater intrusion studies)."

We will indeed cite some CHERT papers in the revised manuscript (i.e. Bellmunt et al., 2016; Bergmann et al., 2012; Kiesling et al., 2010; Leontarakis and Apostolopoulos, 2012; Schmidt-Hattenberger et al., 2013) but none of them has been used for monitoring SWI.

Line 89-90: “The corrosive nature of saline environments causes the limited lifetime of the installation to be a main concern when planning the monitoring experiments.”

Actually, the reason why salinity favors corrosion is explained in the next statement. We will revise the paragraph in the final version (we will wait for other referees) to express something along these lines:

> The objectives of the CHERT experiment are to image SWI in order to improve the geological conceptual model, and to infer SWI dynamics. This requires installing metal electrodes in a corrosive saline environment, especially at the electrodes because electrolysis due to current injection accelerates the corrosion process, which limits the lifetime of the installation. Therefore, addressing corrosion was a main concern when designing the system and planning the monitoring experiments.
> Stainless-steel mesh electrodes were permanently attached to the outside of the seven deepest PVC piezometers (Figure 2a). Still, the parts most sensitive to corrosion are the connection points between the mesh electrodes and the copper cables that bring current.

Line 99-100: “Details on the set-up and installation are described by Folch et al. (2019).” the cited paper is not completed; there is only authors and title. . .the same for the paper Martinez et al.
Indeed, these papers are still in progress. If this paper is accepted, there are two possibilities by the time it is ready for publication: (1) they are already published, in which case we will write down the full reference, or (2) they are not and we will write down the stage they are at (in press, under review, accepted, or whatever). In any case, those manuscripts will be uploaded with the revised version of this one, if required.

Line 127: “Pseudo-sections of the apparent resistivities are easily created for surface ERT surveys, but there is no corresponding visualization technique for CHERT surveys.” There are several software that visualize the distribution of the apparent resistivity data as a pseudosection view.
Indeed, but only for surface ERT, where apparent resistivities are easily assigned a depth. The representation of the apparent resistivity is not a simple task with CHERT data, as we have shown in Bellmunt et al. (2012), because it involves more than two parameters (e.g., depth, level, orientation, etc.). Bellmunt et al. (2012) proposed to have a rough image of the subsoil electrical structure with CHERT data through an apparent resistivity pseudosection equivalent to the case of the equatorial dipole–dipole on the surface that was built considering only data in which the current and potential electrodes A and M are at the same depth as the current and potential electrodes B and N, respectively. Nevertheless, the resulting interpretation is not straightforward and can be confusing for non-CHERT specialists.

Line 131: “for time-lapse studies it is important to ensure that changes observed are due to subsurface processes, and not to changes in the survey setup. Consequently, the sixteen datasets were scanned and compared to keep only identical electrode configurations. This resulted in a reduced set of 2677 identical measurements that were extracted from each complete CHERT before being used in the time-lapse inversion." 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.

We have decided to only consider electrode configurations for which the resulting data at all measurement times passed our quality control. The consequence is that we only have 2677 left for each panel.

Line 132: "For forward modeling and inversion, we make the common assumption that the bulk EC distribution is constant in the direction perpendicular to the complete CHERT transect." The authors indicate "forward modelling", but in the paper there is not indication on a forward approach.

Here we refer to the forward calculation required to do the inversion, but to avoid confusion we will delete it.

Line 232-233: "Below, the time-lapse changes described in the previous paragraph will be interpreted along with precipitation and wave activity data to understand the origins of long-term and short-term behaviors in the dataset." 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.

We do not combine EC data with wave activity data. The wave data are simply used as a proxy to indicate times at which large waves might have reached the surface close to the seaward side of our ERT transect in order to create a saline pond whose infiltration might be responsible for the shallow conductors appearing in this region. This is simply done to assess if EC changes derived from time-lapse inversion make hydrological sense. Our comparison indicates that large waves reaching inland might play an important role in SWI dynamics at the scale of study. To avoid ambiguity, we will write something like:

> We analyze below the origins of long-term and short-term changes described in the previous paragraph by comparison with precipitation and wave activity data. The wave activity data are used as a proxy to indicate the likely timing when water from large waves might have formed water ponds at the surface.

Line 253: The referee points out the color scales of the figures are different.

Indeed, Figures 7a and 7b represent different variables. The different color scales in figures 7a and figure 8 are meant to highlight the specific EC changes related to the precipitation and storm events.

Line 265: "In Figure 8b, we show data from precipitation records and wind velocity and significant wave height models." I suggest to delete the discussion on the wind velocity. It doesn't add some important information on the paper.

We respectfully disagree with the reviewer as we explained above. For this reason, we have decided to keep this discussion while improving the description to make its importance more clear.

Line 292: "IL logs were performed in the piezometers of 20 m length of each nest, because the stainless steel electrodes installed in the 25 m length piezometers severely corrupt the IL signal." 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.

The induction logs within the ERT boreholes only show the position of the electrodes because the steel mesh alters the electromagnetic field emitted by the Geovista EM51 tool that is placed inside the PVC casing (see figure Fig.2). For this reason, the induction logs were acquired in neighboring piezometers without electrodes that were located at least 1.45 m away from the ERT boreholes (Fig. 4 and Table 1). Indeed, the range of influence of the electromagnetic field of the logging tool lies within 0.57 and 0.83 m (Ellis and Singer, 2007) (Fig. 3).

We will modify the text to say something like:

> Inducion logs were not performed in the 25 m deep piezometers because the stainless-steel electrodes installed outside the casing severely corrupted the recorded signal. Instead, they were performed in neighboring 20 m deep piezometers that do not contain any electrodes.

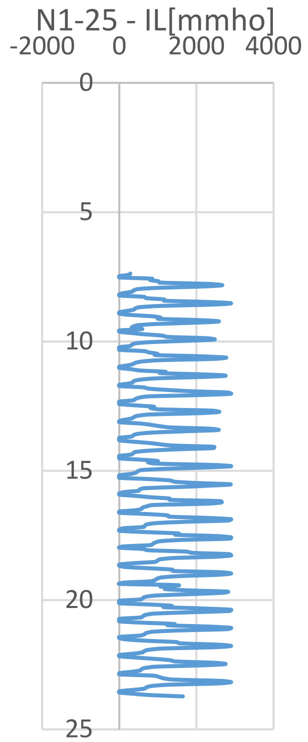

Fig. 2.- Induction log of N1-25. The higher values od conductivity correspond to the location of the electrodes steel mesh

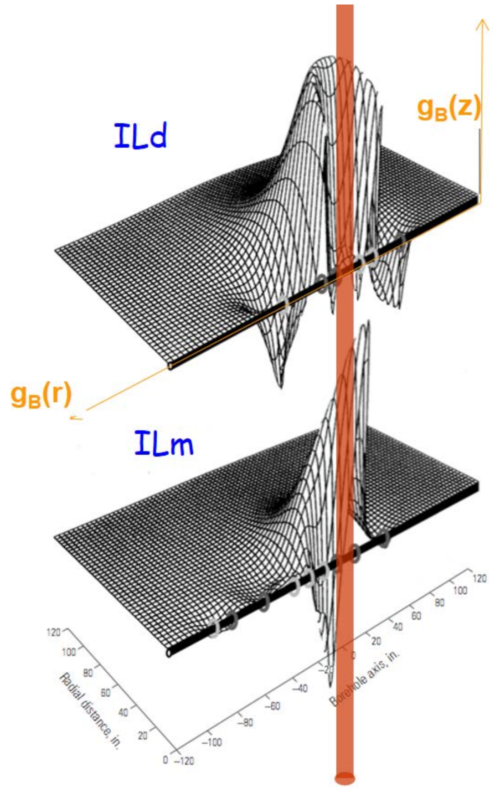

Fig. 3.- Range of influence of Induction Logging (IL). *Courtesy of Philippe Pezard.*

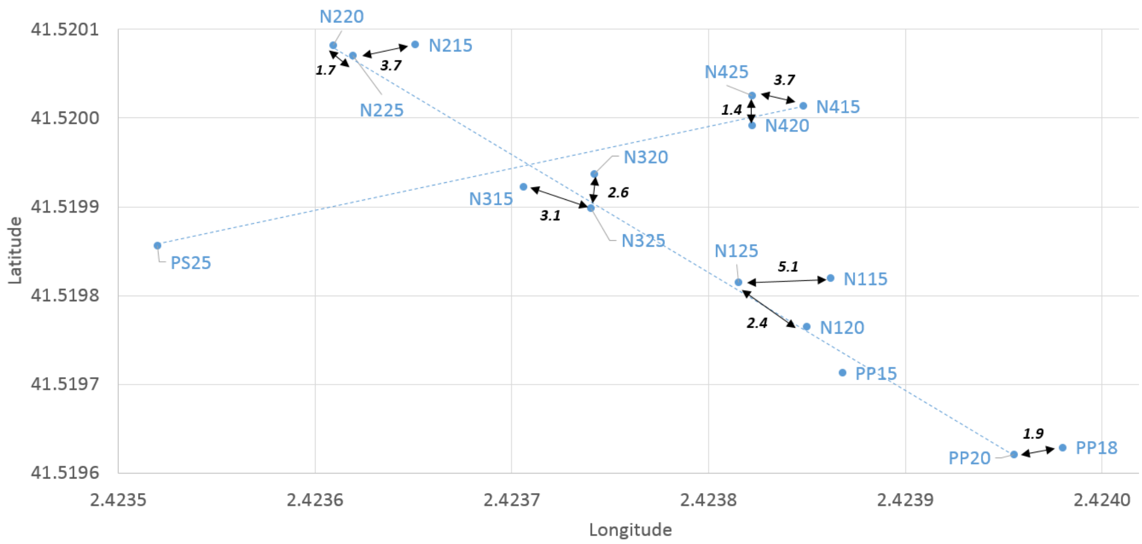

Fig. 4.- Distance from the deeper boreholes and boreholes withouth electrodes.

Well with electrode	Well without electrode	DISTANCE (m)
N225	N220	1.7
N225	N215	3.8
N425	N415	3.7
N425	N420	1.5
N325	N320	2.6
N325	N315	3.1
N125	N120	2.4
N125	N115	5.1
PP20	PP18	1.9

Table 1.- Distance between boreholes.

Line 355-414:
6.3 Time-lapse study: long-term effects
6.3.1 Seasonality: the natural dynamics
6.3.2 The drought: long-term salinization
6.4 Time-lapse study: short-term effects
6.4.1 The heavy rain: a freshwater event
6.4.2 The storm: a saltwater event
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.

We do not agree about merging the paragraphs. The goal of the paper is to show that CHERT can be used to gain insights into different processes operating at different time scales. If we merge the paragraphs, the final text would be too confusing. Nevertheless, we will rewrite it following the indications of the referee to improve clarity.

Conclusions: "4) Time-lapse CHERT has also been successful in capturing long-term and short-term conductivity changes. Long-term changes include (a) seasonal fluctuations of groundwater flux that cause the seawater-freshwater interface to move seawards during periods of high flux or landwards during periods of low flux; and (b) the long-term salinization of the lower aquifer due to an intense drought in the study area during the monitoring period. Short-term changes include (a) a decrease in conductivity related to a heavy individual rain event of 220 mm of precipitation (a third of the annual average rainfall) in only one day; and (b) an increase in conductivity in the beach area, coinciding with storms that caused strong winds and enhanced wave activity." Even if I agree the different points, I suggest to rewrite some sentences (i.e. point 4) after the revision of the paper.

After receiving the comments from the other referees, we will carefully revise the text.

In the original version of the paper one of the authors of the paper (Laura del Val) was not included in the list of authors by mistake. In the corrected manuscript the list and order of the authors will be the following:

Andrea Palacios, Juan José Ledo, Niklas Linde, Linda Luquot, Fabian Bellmunt, Albert Folch, Alex Marcuello, Pilar Queralt, Philippe A. Pezard, Laura Martínez, Laura del Val, David Bosch, and Jesús Carrera.

Reference that will not be included in the final manuscript: Ellis, D. V. and J. M. Singer (2007). Well logging for earth scientists, Springer.