Dear reviewers, editors,

Thank you for the very constructive comments. Specific responses to each of the comments are detailed below, and in particular, concerning 1) the restructuration of the manuscript asked by rev#3 (update of the table of contents) and 2) an enhanced discussion about the water table impact and hydro-modeling perspective, asked by all reviewers.

The planned modifications have been introduced, and developed when required.

Almost all specific and technical comments have been approved.

Yours sincerely,

The authors

Content

Rev#1

Although this paper has the potential to be a very interesting contribution to Hydrology and Earth System Sciences, I think that the following major issue of concern exists.

Since the geomorphological context (fluvial paleo-channel) of the survey area and the proximity of the present-day Seine river, it should be expected the presence of the water table hosted in the near-surface porous sediments investigated by the geophysical survey. Actually, this aspect is hardly discussed at all and, since the presence at depth of water hosted in sediments affect the bulk electrical resistivity, it is crucial in for the interpretation of the electrostratigraphic units from ERI in terms of lithology and/or sedimentary facies association and, thus, for the three-layer model adopted all over the site to represent the studied area Considering that the results obtained are very intriguing, I suggest the Author to add a more focused discussion regarding the presence of the water table (or its absence), it its depth below ground surface and the chemistry of groundwater (i.e., the electrical conductivity). Alternatively, I suggest the Authors to explicit if this data were available to them (or not) and, if so, how they were considered in the discussion of results. I think that this discussion will greatly improve the scientific value of the results because can help geologist/geophysicist that have to face a similar problem.

The water table was measured in the last series of auger soundings done in June 2015 (PTA02 to PTA04 and PTA11 to PTA13) during a low water period. The clay infilling is always saturated. The upper topsoil/loam unit is never dry, but its degree of saturation could probably vary from 50% to 100% (which is most likely the case during high water periods).

Because the resistivity of the clays is close to 10-20 Ohm.m, and the water conductivity (measured from a piezometer located 1km apart from the site, is about 640 µS/cm ~15 Ohm.m) the change of the saturation of the topsoil/loam formation (~ 80 Ohm.m from the half meter spaced ERI) is not sufficient to lower the resistivity down to the level of the clays.

A qualitative XRD (X-ray Diffraction) experiment has been carried out on an old recovered sample of the clayey infilling, which gives the following results for a geological formation that can be described as a marl: ~60% carbonate, ~20% quartz, ~20% illite/montmorillonite and traces of kaolinite. Even fully saturated, the first decimeters (up to 1 m thickness in the southwestern part of the survery) of the topsoil/loam could not reasonably reach the conductivity level of the clayey formation, and its electromagnetic signature is almost undetectable (considering the configuration of the CMD explorer device) for thicknesses lower than 30 cm.

We agree: an extended discussion on that aspect should help, and will be proposed in the revised version of the manuscript.

SPECIFIC COMMENTS Minor issues of concern are listed in the following.

1) *When describing ERI Measurement setup, considering the use of 48 channel georesistivity meter and 0.5 and 1 m electrode spacing it is not clear how the procedure of rollalong of resistivity data for subsequent transects was accomplished.*

We did not use a classical roll-along sequence. Because each pseudo section was measured in less than 15mn (multi-channel Syscal Pro from Iris Instrument), we performed successive pseudo sections with overlaps (half the ERI profile length=24m). Text will be annotated accordingly.

2) *Apparently, no motivation for defining the topsoil as "resistive" (line 272) is furnished. A motivation for this could be that the soil is plowed (as it can be seen form aerial view in Fig)?*

The resistivity/conductivity value for the topsoil is inferred from the half meter spaced ERI, southwestern part or ERI section in Figure 5). The surface is covered with grass and the logs clearly indicate the topsoil-loam cover.

Text will be annotated to specify that the site was a grassy meadow during the survey and the weather conditions will be described (sunny weather during all the survey).

TECHNICAL CORRECTIONS

1) Fig. 3: the location of hand auger drilling are notdisplayed. It can be useful for the reader in order to facilitate the comparison between data. Will be done.

2) Fig. 5: The SW-NE orientation of the ERI transect is not displayed. It can be useful for the reader in order to facilitate the comparison between data. Will be done.

3) Fig. 5bis: it could be useful to represent in the ERI model the location at depth where the auger soundings achieved by a refusal. Will be done.

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Rev#2

REVIEW COMMENTS

0- OVERALL

I would like to address your approach towards apparent conductivities and electrical conductivity in general. First of all, as both properties are repeated quite often, I would suggest using the abbreviations EC (true) and ECa (apparent). Second, the difference between both is often unclear in the presented work. It can't be stressed enough that apparent electrical conductivity (ECa; as defined by McNeill (REFERENCE); 'apparent') shouldn't be compared to electrical conductivity (EC; a value of the half-space model; 'true'; retrieved after inversion of EMI data) of the subsurface (see also Figure 5).

Also, the symbols used within the paper should elucidate this difference. At present, you use σ for both EC and ECa. I suggest using σ and σa, respectively, to avoid confusion and enhance the distinction between both.

EC and EC_a will be used in the modified version of the manuscript.

Be consistent when using abbreviations, and stick to these once defined. You use the abbreviation EMI at the beginning, though later on use the full notation (e.g., L156, L162). Will be done.

Some obvious questions arise during reading:

(1) why use a reference line to calibrate the data where no sampling overlap exists between the two survey modes?

To be honest, the current ERI/EMI calibration process (Lavoué et al. approach) was not planned; it has been decided afterwards during the processing of the data. A planned reference common line is clearly the best solution, but it is also interesting to illustrate what can be obtained if just crossing lines are available.

(2) Why use a 3 layered inversion model for the EMI data when the ERI shows 2 layers?

Throughout the entire "blue" zone (Thickness 1 < 10-20 cm) Fig 8, a two-layer model should have been ok (similar SRMR -standardized root-mean squared residual- values). The 1-meter spaced ERI is mostly located in this blue zone which corresponds to thickness 1 less than 20 cm.
Nevertheless, we kept a three-layer model because:
1- the logs clearly showed a distinct layer over the clay infilling (without presuming of their respective contrast of resistivity).
2- of the specificity of the southwestern part illustrated by the results of the half meter spaced ERI (Figure 5), where the thickness of the resistive top layer above the clay infilling exceeds 1 m.
We must admit that the question of mixing 2 and 3 layered model over the site was discussed a lot, but not kept (essentially because of 1-, and thanks to 2-). It is clear that the "blue" areas of Figure 8 for Thickness 1 correspond to zones where the top resistive layer can be considered as inexistent (from a geophysical point of view, with the resolutions of the method used).
The text will be slightly modified accordingly to specify this point.

(3) Why is there no comparison of the inverted ERI data to the inverted EMI data?

The comparison is implicit as the ERI results have been set as the reference for the depth of the clay infilling – substratum interface. EMI results have been scaled and shifted to fit ERI interpretation. It is the purpose of Fig 5 which actually shows the inverted EMI data with the estimated bottom depth of the clay infilling (as resistivities were fixed during EMI inversion with the help of the ERI interpretation).

(additionally: you could include an isosurface indicating the shape of the river? This is ultimately the goal of the presented work, i.e. retrieve the shape/morphology of the river.)

The clay infilling (the conductive formation) is without doubt, associated with the presence "at a moment" of the river. However, the past evolution of the meanders is very complicate with multiple crossing and overlapping over time. It is only possible to delineate the clay infilling, and difficult to retrieve the river shape at a given time from the measurements of the electrical conductivity only. It would require to link the information obtained from geochemical measurements with geophysical data which is far from being straightforward from EMI data only. Consequently in the present paper, we prefer not to draw the isosurface, and rather let Thickness #2 as the lone paleoriver geometrical information. Text will be annotated accordingly.

1- INTRODUCTION

L49-51: EMI devices are increasingly used for a large number of near-surface geophysical applications, as a consequence of their ability to produce 2D images of the apparent electrical conductivity, σ, over a large surface.

This is an example of my previous overall comment. 2D images of ECa (σ_a) are actually spatially lateral maps of the ECa; apparent. 2D cross-sections (inverted) of the EC (σ) are what is of interest in this article. I would suggest to rephrase this sentence, based on what you exactly mean with this.

"2D images" has been replaced by "mapping".

The focus of this study is to evaluate the reliability of EMI at meso-scale to image globally in 3D, even if it is interpreted in 1D locally. ERI is not meant for providing 3D image of such "large object". ERI and logs are highly recommended as "the best geophysical/direct observations" calibration support for EMI in this context. Text will be modified accordingly.

L60-63: "This shift can be conveniently represented by a complex number, comprising quadrature and in-phase (respectively, real and imaginary) components, which can be inverted and then interpreted in terms of an apparent conductivity and an apparent depth of investigation (DOI)."

Should be: (respectively, imaginary and real). The quadrature (or imaginary) and in-phase (or real) components. Will be done.

After inversion it is the EC (not ECa; example of overall comment) I'm not really sure what you exactly mean with apparent DOI (I now only know that it is opposed to the real, L72). So I assume a specific DOI which you attribute to a certain setup independent of the soil model?

Indeed, "inverted" is misleading in the present context. It will be removed. Here, it's all about apparent property and its corresponding DOI.

L67-70: "This interpretation relies on the fact that, for a given soil model, one specific apparent DOI is defined by three device setup parameters: (1) the offset between the transmitter and receiver magnetic dipole, (2) the orientation of the dipole pair, and (3) the frequency of the transmitter current oscillations. "

I think the fourth setup parameter: (4) instrument elevation or instrument operation height is of great importance and worth noting as well. Agree. Text will be modified.

L78: The word 'typical' should be specified more. E.g., low, non-Ferro… Will be done.

L80-84: "In a resistive or highly conductive environment, such as that presented in the present study, the McNeill equation is no longer valid, and EMI recordings, in particular their in-phase component, must be interpreted within the specific measurement context, taking all of the physical properties of the local environment into account."

I suggest to list the physical properties (i.e., EC, mag. susc., diel. perm.) instead of mentioning 'all'. Will be done.

2- DESCRIPTION OF THE STUDY AREA

What were the weather conditions when the measurements took place? Maybe worth to note, as they could have their influence as well (influence of watertable, moister content). In how many days or during which period was the survey conducted? This could have its influence on the results later on (see 2-layered vs 3-layered model).

Details concerning the site conditions will be added, as well as a new discussion concerning the influence of the water table and the hydro-modeling perspective.

An EMI survey is fast compared to an ERI survey and can be used to determine the location of the ERI survey. Was the EMI survey used to determine the location of the ERI survey to incorporate more lateral variations. If not, why not? In case of calibrating your signal, it is very important to cover as much as possible of the present variation.

It is a wise and usual strategy of prospection to map "quickly" and "roughly" with EMI, before doing ERI to characterize depth and lateral variations accurately: we totally agree. In the present case, little time was available for a wide area to be investigated before setting up the ERI section.

We define the strategy of prospection from the LiDAR map and the old hand-auger soundings (done one year before the survey). Actually, we must admit that the EMI/ERI calibration procedure was not planned, but decided afterwards during the inversion process.

L138: this □ these these

3- METHODOLOGY

Include instrument survey height here as well. Will be done.

L154: …a reference transect of almost… Will beone

L166-167: Three different offsets were used between the centers of the Tx and the Rx coils, namely: 1.48m, 2.82 m and 4.49 m, each corresponding to a distinct DOI.

I suppose you mean a distinct apparent DOI in this case? Based on each coil separation, without further knowledge of the soil model. Indeed. "apparent" will be added.

L170: The word attempting makes this sentence sound like you just tried something. Assuming this was done deliberately, I would use another word. "Attempting" will be removed.

L195-199: "When compared to the analysis achieved using auger soundings, the electrical properties of the topsoil/loam formation appear to be merged with the clayey formation, with the exception of the western portion of the cross-section, which has significant sand and gravel content. This outcome could also be due to the finer spatial resolution of the ERI measurements (electrode spacing of 0.5 m)."

Based on the fact that later on a 3-layer model was used, I assume that the finer spatial resolution is given as the reason why there are only 2 distinct layers in the ERI profile? Maybe add a little information about the sensitivity distribution of the used ERI array setup?

The array used is a mixed Wenner-Schlumberger (reciprocal configuration in order to allow a strong multi-channel parallelization). Theoretically this configuration has enough sensitivity (Furman et al., 2003; Dhalin and Zhou, 2004). With hindsight, a gradient or multiple gradient array should have probably be more efficient to discriminate the first decimeters with a 1m-spacing.

Text will be modified accordingly.

Is it justifiable to calibrate an assumed 3-layer profile with a 2-layered inverted ERI model?

See previous response to a similar comment of Rev#1 (L95-107 of this reply).

The inversion of ERI data is also an inversion with parameters and uncertainties. It is unfair to say that this model is 'true'. 'True' will be replaced by 'interpreted'.

What were the weather conditions when the measurements took place? Maybe worth to note, as they could have their influence as well? Dry and sunny weather all the time during the 3 days campaign. A discussion about the water table impact will be added.

L205-208: I would suggest to rephrase in a more comprehensive way. The sentence is will be reformulated.

L227-232: "During the field data acquisition we faced several difficulties that prevent us to do a CMD profile exactly on the reference profile. Actually, the EMI data used for the calibration have been taken from the mapped data closest to the reference profile. This has led to several positioning and alignment errors : 1) the EMI data do not exactly cross the reference profile, 2) the EMI data are irregularly spaced along the ERI profile, and 3) the orientation of the CMD device was not exactly the same, for each measurement retained for the calibration."

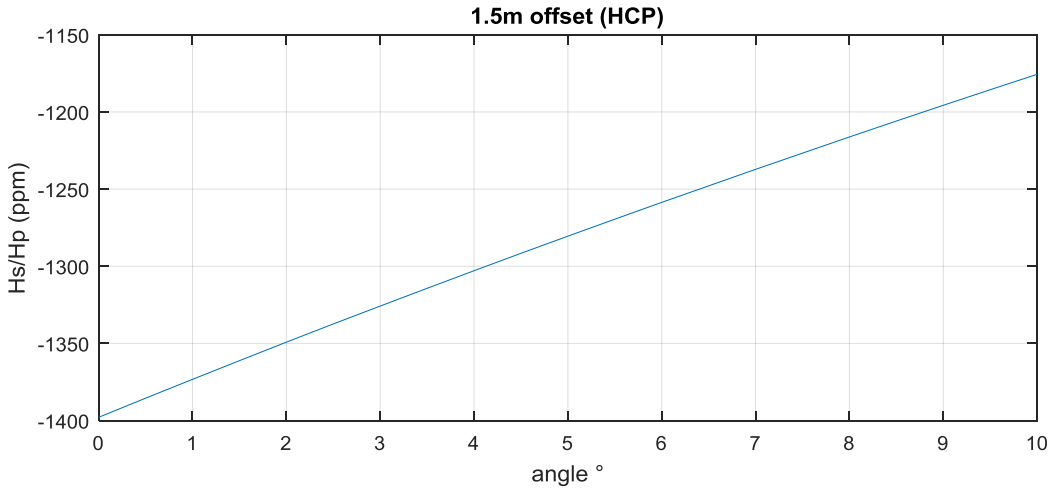
I don't really get why you draw a reference profile on a location where you can't perform a CMD survey. This is the core of the calibration process. Because the present EMI/ERI calibration as developed here, was not planned. (L90-93 of this reply)

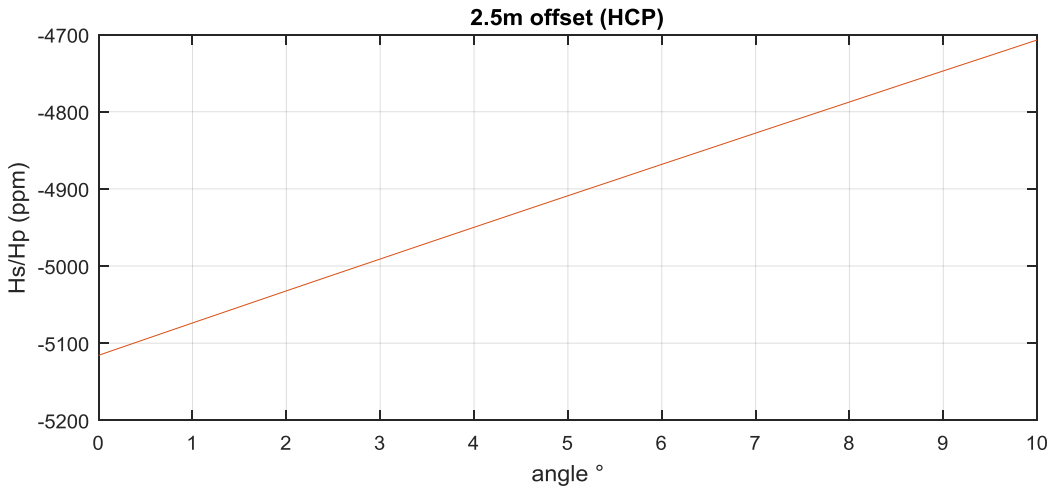
Also add the fact that (4) the height above the surface is changing constantly (as you are wearingthe instrument?) for each measurement. Will be done.

The changing orientation has a great impact on the calibration as other sensitivity distributions are constantly used to attain the results.

You are naming these errors that are included in the process but do not really assess how to contribute to the results. What is their impact, is this not too big?

It is difficult to assess quantitatively from *in situ* measurements. There are different for each offset. Apparent conductivities measured are a little bit noisier for the smallest offset, nothing abnormal. During the campaign, the carriers encountered difficulties to cover the area because of the presence of dense vegetation; the pitch angle was oscillating of a few degrees at least. Below, two plots show the theoretical variation of the quadrature part in function of the pitch angle (< 10°) for the 1.5 and 2.5 meter offsets. For example, for the CMD configuration, a pitch variation of 2° (which corresponds to a height variation of 7 cm for the Tx coil, 3 cm for Rx 1.5 meter offset, and <1cm for the Rx 2.5m meter offset) shows 4% and 2% changes, for the 1.5 meter and 2.5 meter offsets respectively (16% and 8% for 10°). This is not 0% but can be considered as usual field errors. Moreover, the pitch is generally changing smoothly from sounding to sounding.





L244: Once calibration is done… Done.

L252-265: "Step (3) does not guarantee that estimated interfaces will match the ERT interfaces 1) if the fixed/chosen resistivities are not correct, or 2) if EMI does not integrate the ground in the same way as the ERI in case of strong anisotropy, which seems not to be the case here, since a good match is obtained."

The correlation coefficients are comprised between 0.5 and 0.7. Such values can be explained by several sources of errors in the estimation of the EMI apparent conductivities along the reference profile: 1) the differences in the location between the EMI measurements used for the calibration and the ERI profile, 2) the fact that the one dimensional model used for the EMI modeling is extracted from the inversed 2D resistivity section, 3) the difference of sensitivity between the ERI and EMI data. The regressions indicate the need of a stronger correction for the VCP configuration than for the HCP configuration. The scaling correction decreases as a function of offset, particularly for the HCP, which can be explained by the fact that small offsets are more sensitive to positioning and orientation errors, as well as natural near-surface variabilities.

Based on the correlation coefficients it is hard to say that a good match is obtained. The correlation isn't that high (i.e. it does indicate anisotropy). This is also visible in the VCP configuration, which is more influenced (compared to the HCP conf.) by the anisotropy (also due to the 1 m instrument operation height). The VCP configuration has a highly concentrated sensitivity close to the instrument compared to the HCP which reaches this high sensitivity (in 1D) at a lower point (more spread compared to the VCP). This results in an increasing correlation for bigger coil separations (due to a smaller relative impact on the response of the present anisotropy).

We agree it is a coarse match. The primary reason is that the EMI performed on the reference profile have been extracted from perpendicular cross lines: the idea of calibration from ERI, has come afterwards.

But comparing to Lavoué's *et al.* (2010) data, where an EMI profile has been specifically acquired for the calibration, the dispersion is of **the <u>same order</u>** (unfortunately no correlation coefficients provided). It is not perfect, and linear correlation is, as expected, more difficult to obtain for the smallest offsets for which exactitude of the measurement locations of the 2 methods is more critical (and the different integrated ground volumes by the 2 methods are more sensitive to small scale changes). But despite this, Figure 5 shows that the interface from the EMI inversion better matches the ERI all along the profile after calibration, especially for VCP, while calibration has a minor effect on the HCP results.

L271-273: Consequently, a three-layer model seems reasonably justified all over the site during the inversion process to represent the studied area: a resistive topsoil, a conductive clayey filling, and a resistive sand/gravel layer.

Is it justifiable to use a 3-layered model for the inversion after you calibrated the EMI data using a '2-layered' model, i.e. the inverted ERI results?

See response to Rev#2 lines 95-107 of this reply.

Shouldn't the ERI spacing be adjusted such that the small top layer can be detected? (Like in the western part). Yes. Next time, it would be clearly an asset to do some additional small-offset ERI to evaluate the very near surface resistivity. Text will be annotated.

Maybe discuss the characteristics of the sensitivity distribution of the ERI array setup? Discussion will be added, regarding also the multigradient configuration.

L844-286: Maybe use the abbreviation SRMR (or SRSR?) to indicate the standardized root-meansquared residual and then also in the formula (L286): SRMR = … Will be done.

4- EMI INVERSION RESULTS AND DISCUSSION

Overall, I think there should be an increased focus on explaining why something is occurring and on the validation of the inversion.

I think it would be an asset to show the 2D slices of the inverted EMI data on the location of the reference ERI profile. This could provide a means of comparing the inversion results of both techniques. Actually, it is the case in Fig 5, where the position of the clay-substratum interface from the EMI inversion before and after calibration is shown. Showing a full 2D slice for the EMI inversion results is not pertinent as the resistivities are fixed during the inversion and the thicknesses of the first two layers inverted only.

"L333-335: The combined HCP&VCP data inversion naturally leads to the occurrence of higher values of data residual, than in the case of the individual HCP or VCP inversions."

Why is this the case? Because, at least theoretically, you add extra information into the inversion process.

The data residual is a quantitative assessment on how the model "explains" mathematically the data. Theoretically, comparison between data-residuals should be done for a single dataset. In the present case: a) the two measurements in HCP and VCP modes have been carried out in 2 times => not perfectly identical positions, heights and orientations a bit different for both data sets, b) HCP and VCP modes do not integrate the ground in the same way. If the ground within the footprint of the system is a bit far from a tabular model, then the interpretation with local 1D models can be more difficult with both data sets inverted jointly than with one of the two sets only.

Conclusion will be annotated.

Is this the best approach? Should they be inverted together? Or both separately and use them in a complementary way?

It depends on the characteristic size of the anomalies and variations that need to be mapped; using HCP, VCP or both brings specific information. Using both is a mean to mix information from both setup, but with a weighting depending of their respective sensitivity (i.e. DOI). Figure 8 illustrates the results of inverting HCP and VCP alone, and both at the same time. Two conclusions expected: 1) the near surface variability is inferred more accurately by VCP, 2) the low frequency variability is almost the same for all configurations.

EMI results discussion will be annotated.

5- CONCLUSION

Overall, the limitations of the presented technique can be stressed more, as they are obviously present. Will be better highlighted.

L343-345: "In order to correct the sensitivity issues arising from EMI measurements, a calibration procedure was implemented, based on the use of a linear correction with ERI inversion results and auger soundings."

These aren't sensitivity issues, but drift and factory calibration issues. Text will be modified accordingly.

L360-362: This is unnecessary to mention, it is more a future practical goal based on specific information regarding the institutional framework of the research. Research programs have to be mentioned in acknowledgements, not in the body of the paper. Will be removed.

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Rev#3

Dear Authors and Editor,

This paper presents a case study for testing the utility of multiconfiguration EMI surveys to characterize the interanl structure of a representative paleochannel in an alluvial plain setting of the river Seine, France. There is a growing interest in using near-surface EMI techniques for mapping relict geologic features, such as; paleochannels, towards improving our understanding of how these features influence groundwater dynamics as well as how they control the development and evolution of the modern landscape. The results from this study show an interesting application of EMI, ERI, and auger soundings to map the internal structure of a paleochannel. However, I think there are several key pieces that are missing regarding the link between methods and the "bigger picture" attempting to understand the long-term hydrological processes. Thus, it is my opinion that the paper is incomplete in its present form, but could improve if there is more emphasis on the main considerations I have outlined below. I have made comments and questions throughout the manuscript, roughly following the order of the paper, which should be considered as suggestions for helping to improve the paper.

Main considerations:

1) *In the abstract, the authors state that "A detailed knowledge of the internal heterogeneities of such paleomeanders can thus lead to a comprehensive understanding of its long-term hydrogeological processes." Similar statements are made in Lines 44-48, however, the findings of this study are not described within a framework of how EMI, when calibrated with ERI and auger soudings, contributes to a better understanding of the hydrological processes of the river Seine alluvial plain "La Bassée." I realize that the main focus of this paper is to map the internal geometry of the paleochannel, but I am left wondering why the authors make the above statements without any discussion throughout the paper? The authors end (Lines 358-362) by stating that their technique "could significantly improve the accuracy of hydrological modeling…" but this will be debated later (it is unclear whether this is another phase of the project, conference?). It is my opinion that this is a critical piece that is missing from the paper. Without this important discussion, the paper is missing a key aspect of how EMI methods provide an innovative way of characterizing the geological controls on hydrologic processes, and as a result, falls short of satisfying the aims and scopes of the journal http://www.hydrology-and-earth-system-sciences.net/about/aims_and_scope.html.*

Ok.

A discussion will be added concerning the impact on the EMI results of the water table in the present context. In a near-surface "clayey" context, resistivity methods are less sensitive to pore water content. In addition, when the upper formation is quite thin (less than half the ERI electrode spacing) and because the clayey infilling is always saturated, the influence of the water table on the loam/topsoil resistivity is hardly detectable.

Hydrogeological modeling is not proposed here, but planned by our colleague hydrogeologist. It will be limited by our (geophysicist) capability to set a relationship between the electrical properties and in the present case the water, clay and salinity contents (even mineralogy proportion). Text will be annoted accordingly.

Why didn't the survey go beyond the expected boundaries of the channel, visible in the LiDAR data? In otherwords, the surveys were only performed within the channel, making it difficult to fully characterize the variations in lithology/hydrology inside and outside the channel. Although vegetation cover (treeline) seems to be one limiting factor for the survey design, based on the LiDAR map, it seems feasible that the survey could have extended further to better capture the transition between outside and inside the paleochannel.

Not only treeline but also: 1- cultivated area, 2- unauthorized access to private fields, 3- ERI / EMI survey to manage sequentially and just 3 days to perform all the campaign.

The structure of the paper in the Methods and Results/Discussion sections is confusing. There is a mixing of methods and results in the Methods section, and nearly all of the results and figures are presented in the Methods section, with no figures presented in the Results/Discussion section, which is only two pages long? If the authors can 1) restructure the Methods, and Results/Discussion sections, 2) incorporate a more in-depth discussion of the hydrologic influences on the EMI measurements, water table information, weather conditions, and survey design, and 3) relate the results of the EMI surveys to how the "estimation of the geometry of the Seine river can provide valuable insight into its paleo-hydrology…" then they will have a paper that is beneficial for geologists, geophysicsts, and hydrologists interested in these complex problems.

Ok. The structure of the paper will be modified and better balanced with a discussion focused on the theoretical impact of the water content as well as the hydro-modeling perspective as suggested. Water table values in some of the hand auger soundings as well as the water conductivity (recorded in a nearby piezometer) will be discussed.

Concerning the point 3), it will be first reminded that without a clar link between geophysical and dating datas it will be difficult to propose an accurate "past and future hydro-scenarios".

Below, an updated table of content:

Specific comments/suggestions:
Abstract:

- Lines 23-25: As stated above, there is no discussion about this later in the paper and how the methods used in the present study can help address this important problem. Ok. Discussion will be added.

Introduction:

- In general, the Introduction is not referenced enough (e.g., Lines 34-37; 54-63; 64-72). There are several other studies that have looked at very similar problems that the current paper is trying to address, and should be cited. For example, please refer to Fitterman et al. (1991); Maillet et al. (2005); De Smedt et al. (2011), which also used similar procedures to investigate paleochannel geometry, thickness, etc. Ok. The literature concerning EMI in general, even for the lone paleo environment mapping is huge. De Smedt, Fitterman, Delefortrie, and Huang will be added.

- Line 39: I suggest defining electrical conductivity as: σ, and apparent conductivity as: σa, and use this notation consistently throughout the manuscript. In fact, apparent electrical conductivity (Lines 50-51) is mislabeled (not σ as stated) and should be σa. Text will be modified with EC , EC_a

- Line 40: Fine sediments do not necessarily correspond to conductive, and coarse sediments to resistive materials. Fine and coarse sediments that consist of the same mineralogy (e.g., quartz) should in principle have similar resistivities. What is missing here is that the mineralogy, quartz, clay, etc. is also an important property. In addition, the porosity and fluids within the pore space, whether freshwater or saline water, also have an important influence on σ. This needs to be clarified. Ok. Text will be annotated.

- Lines 44-48: Similar to my above comment for the Abstract. The idea that EMI can be used to provide valuable insight into the paleo-hydrology and as the author's state, climatic fluctuations, does not come out later in the discussion of the paper. Text will be annotated as suggested.

- Line 51: "over a large surface," or is it that EMI methods are capable of covering large areas/distances over relatively short periods of time? Text will be annotated as suggested.

- Lines 54-63: There are no references in this paragraph, and citations are needed as this information regarding the background EM physics is probably not general knowledge to the reader. Ok reference will be added.

- Line 61: This should be "respectively, imaginary and real" Ok

- Line 63: I haven't seen this term used before in the literature: "apparent depth of investigation," and have only seen it reported as the depth of investigation (DOI), see Huang, (2005), and references therein. Will be corrected.

- Lines 67-70: I think a fourth point to add is that the DOI is also a function of the height of the instrument above the ground. Ok.

- Line 78: What are "typical conductive properties"? Perhaps give a few examples here. Low ferromagnetic…text will be modified.

Description of the study area:

- What is missing from this section is a description of any information on the depth of the water table, as this is important for data processing and interpretation. Information will be provided, as well as discussion concerning the impact of the water table.

- *Lines 105-107: This is already stated in lines 47-48, and could either be removed or combined with the earlier statement in the Introduction.* Ok. Reference will be moved to the introduction.

- *Line 116: What kind of soundings? Borehole soundings from a hand auger?* Mechanical not hand borehole soundings reaching between 6 and 10m depth. Text will be modified accordingly.

- *Line 138: Please change "this" to "these"* Ok

- *Line 144: This sentence should be referenced* Ok.

- *Lines 145-149: This last paragraph seems a bit out of place in the Study Area section. The objectives of the study should be listed in the last paragraph of the Introduction.* Ok

Methodology, Measurement setup:

- *Overall, I am surprised to see that most of the results and nearly all of the figures are discussed in the Methods section and not the Results section? It is confusing to the reader and I am left wondering why the authors chose to structure the paper in this way? I think the clarity of the paper could be improved if the basic background of the methods is described in the Methods subsections, and the results be left for the Results/Discussion section. In fact the Results/Discussion section is only 2 pages long, compared to 6 pages of Methods!* Structure of the paper will be modified as suggested. See the new outlines L412-422 of the present reply.

- *Line 153: Please provide the details of where you got the LiDAR map, i.e., what database, the dates of data collection, how it was produced, etc. Also include a citation.* The LiDAR map was provided by the Seine Grands Lacs public organism (http://seinegrandslacs.fr/) to the PhD thesis of B. Deleplancque referenced in the current paper.

- *Lines 155-157: This sentence is repeated in Line 162, and is Line 158 intended to be a separate paragraph, or part of the same paragraph?* Ok. Text will be modified.

- *Lines 162-164: Electromagnetic induction (EMI) is already spelled-out before, and I don't think it is necessary to write ElectroMagnetic (EMI); Horizontal CoPlanar – HPC, and Vertical CoPlanar – VCP, like this. In other words, I don't think it is necessary to capitalize the beginning of each abbreviation as this is already common knowledge in the literature, i.e., electromagnetic induction (EMI), not ElectroMagnetic Induction.* Ok.

- Line 167: What is the approximate DOI for each offset? It would be useful to include this instead of just saying "a distinct DOI." Ok. Approximate values of DOI will be mentioned. *Additionally, it would be helpful to mention what the instrument height above the ground was, as well as what the step-size was (e.g., 0.5 m), what was the acquisition mode (stationary/fixed spacing, continuous mode, random walk). In other words, what were the specific survey details used in this study? Also, what is missing here is a description of the weather conditions, and how long the surveys were performed, when they were performed, as these are also important for the reader to understand what the conditions were during data acquisition.* Will be done.

- *Line 168: Why were "slightly different sampling intervals used"? This needs to be explained. Shouldn't the sampling intervals be the same if the intention is to compare different dipole configurations at the same acquisition point?* Acquisition was made with the continuous mode (0.6 s

time step, walking at approximately 2-3m/s). 1) In continuous acquisition the instrument can be used for a single orientation at a time, 2) the survey was performed with GPS, 3) we faced GPS reception issues. Consequently the walking paths are not the same for each orientation (Fig 3). Text will be annotated.

- Line 170: Please change "attempting to merge" to "merging" as attempting to do something implies that you were not able to do it. Ok.

Auger sounding results:

- Much of this section is results and not methods. Is it possible to briefly summarize the methods that you used for the auger sounding here and present the results in the Results section? This also follows for the other subsections in the Methods section, which are a mix of methods and results. Ok. Structure of the paper will be modified. See the new outlines L412-422 of the present reply.

- Line 183: Missing PTA 06, as this also contains a peat layer according to Figure 4. Ok.

ERI results:

- Again, much of this section is mixing methods with results.

EMI calibration from ERI:

- Have the authors performed any other site-specific calibrations such as; instrument drift, temperature effects, topographic effects? These have been shown to be important for data processing (see Sudduth et al., 2001; Delefortrie et al., 2014) and is not discussed in the current study. No additional calibration has been done. But concerning the quadrature part, the CMD instrument drift due to temperature is not significant with this instrument for usual daily variations (+ or – 10°C). This not the case for the in-phase part, not presented here.

- Line 207: "near surface" should be hyphenated "near-surface" Ok.

- Lines 217-222: This is a similar to what was already described in the Auger sounding results section and can either be removed, or combined with Lines 175-183. Ok.

- Line 241: Please change "developed in Schamper et al" to "developed by Schamper et al" Ok.

- Line 244: Please change "once the calibration done" to "once the calibration is done" Ok.

- Line 246: Please remove "Actually" at the beginning of the sentence, and start with "Despite" Ok.

- Lines 250-251: "All those non-straightforward steps…" I would suggest rewording the start of this sentence and remove "non-straightforward" The sentence will be modified accordingly.

Inversion parameters:

- Line 270: Please remove the word "clearly" Ok.

- Lines 280-281: As mentioned above, the instrument height should be mentioned earlier in the paper. Will be done.

- Lines 284-286: An equation sign is missing, e.g., RMSE = …., also there is no equation number assigned to this equation (1) on the right-hand side of the margin. Please check the journal formatting for equations. Ok

- Lines 289-290: Is this sentence meant to be a standalone paragraph? This information is also listed in the Figure 8 caption (Lines 480-482). Text will be reformatted.

EMI inversion results and discussion, General trend:

- Lines 294-295: The introductory sentence is a standalone paragraph? Is this a formatting error when Line 296 should be a continuation of the same paragraph? Also, same comment for Lines 307-308. Text will be reformatted.

Conclusion:

- Lines 341-342: Please delete "(CMD explorer from GF instruments," as this is already mentioned earlier in the paper. Will be done.

Figures:

- Figure 1, Line 441: In the bottom panel, is the study area highlighted by the small red star on the figure? It would be helpful to either enlarge location start, or show a boxed area where the surveys were performed to help the reader easily locate the study site. Additionally, for the figure caption there is a typo: "maps" should be uppercase "Maps," and add the word "bottom" after "plain" to denote the top vs. bottom panels. Ok.

- Figure 2, Line 443: Please change "studied area" to "study area". Ok.

- Figure 3: It would be helpful to show where the locations of the auger soundings were performed with respect to the geophysical surveys Will be done.

- Figure 4, Line 454: Please change "log" to uppercase "Log" to begin the sentence. Ok.

- Figure 5, Line 460: Please remove the word "clearly" Will be done.

- Figure 7, Line 473: Please change "histogram" to uppercase "Histogram" Will be done.

Best Regards

References:

De Smedt, P., Van Meirvenne, M., Meerschman, E., Saey, T., Bats, M., Court-Picon, M., De Reu, J., Zwertvaegher, A., Antrop, M., Bourgeois, J. and De Maeyer, P., 2011. Reconstructing palaeochannel morphology with a mobile multicoil electromagnetic induction sensor. Geomorphology, 130, 136-141.

Delefortrie, S., De Smedt, P., Saey, T., Van De Vijver, E., Van Meirvenne, M., 2014. An efficient calibration procedure for correction of drift in EMI survey data. Journal of Applied Geophysics, 110, 115-125.

Fitterman, D.V., Menges, C.M., Al Kamali, A.M. and Jama, F.E., 1991. Electromagnetic mapping of buried paleochannels in eastern Abu Dhabi Emirate, UAE. Geoexploration, 27, 111-133.

Huang, H., 2005. Depth of investigation for small broadband electromagnetic sensors: Geophysics, 70 (6), G135–G142.

Maillet, G.M., Rizzo, E., Revil, A. and Vella, C., 2005. High resolution electrical resistivity tomography (ERT) in a transition zone environment: application for detailed internal architecture and infilling processes study of a Rhône River paleo-channel. Marine Geophysical Researches, 26, 317-328.

Sudduth, K. A., Drummond, S. T., Kitchen, N. R., 2001. Accuracy issues in electromagnetic induction sensing of soil electrical conductivity for precision agriculture. Computers and electronics in agriculture, 31, 239-264.