In this paper efficiency of three methods of geoelectric field calculation is compared. Method 1 of geoelectric field calculation is based on 1-D conductivity model. In Method 2 impedance tensor is calculated based on the least squares method with use of magnetic and electric data for the time intervals and locations which are selected by the author. In Method 3 the author use existing pre-computed impedance tensors which were estimated with use of a robust regression method and auxiliary remote reference measurements. The author concludes that Methods 2 and 3 are more efficient comparing to Method 1, and that Method 2 is slightly more effective than Method 3.

The problem is clearly formulated, and the results which are discussed in this paper are important for GIC community, so I would recommend the paper publishing after minor revision. But I think some details in the paper should be clarified, and I am not satisfied by answers to some questions from previous reviewers (see below).

In my opinion, the main problematic point of the author's approach is in the time interval which was used for methods comparison. In Method 2 a transfer function is calculated based on two days of a four-days interval and this transfer function is used to calculate electric fields for other two days of the same four-days interval. It is not clear, if this method could provide reasonable results at some other moment, far from these four days. Difference in efficiency of Methods 2 and 3 is not significant and it is not clear if method 2 is better or equally efficient to Method 3 'in general'. I would suggest to include this discussion in the conclusion/discussion section. My concerns are close to concerns of Reviewer 1 who wrote: 'Egbert & Booker (1986; Geophys. J. R. astr Soc. 87, 173-194) provide a thorough discussion on the advantages of robust estimation vs LS estimates. Indeed, under certain simple circumstances (such as the relatively very clean time series segments selected for analysis in this manuscript) the LS estimate can work well. In other cases, especially when noise is not obvious in the time series, they can provide entirely misleading results. It should be noted that clear and unbiased electric time series is an extremely rare occurrence and it often takes an expert to identify noise and bias, including cultural, tidal and temperature effects.' In my opinion, the author did not meet this comment properly in the new version of the manuscript.

In the conclusions, I have modified the sentence

> "We have shown that a conventional least squares frequency domain method can give reliable and accurate out-of-sample estimates of the geoelectric field."

To read (removed "reliable and")

> "We have shown that a conventional least squares frequency domain method can give accurate out-of-sample estimates of the geoelectric field"

And the sentence “Method 3 produces in general reliable but slightly lower quality estimates than Method 2 for the time intervals and locations considered.” was modified to omit “in general”.

In the conclusions we have also modified the sentence

> “We have shown that a conventional least squares frequency domain method can give reliable and accurate out-of-sample estimates of the geoelectric field.”

to be

> “We have shown that a conventional least squares frequency domain method can give reliable and accurate out-of-sample estimates of the geoelectric field for data of the type considered in this work.”

In addition, the statement

> “Although the conventional least squares method has been shown to be flawed with respect to transfer function estimation for the purpose of ground conductivity estimation (Egbert and Booker [1986]), we have shown here that it can produce equal or improved out-of-sample predictions of the electric field on data segments without many defects.”

Has been modified to

> “Although the conventional least squares method has been shown to be flawed with respect to transfer function estimation for the purpose of ground conductivity estimation (Egbert and Booker [1986]), we have shown here that it can produce equal or improved out-of-sample predictions of the electric field on data segments without many defects *and when the prediction transfer function is used to predict data near in time to the interval used to derive the transfer function.*”

Both reviewers suggest that the transfer functions for all three methods should be plotted and I agree with that. Even though the author’s objective is to determine the performance of electric field calculations (not a conductivity structure), it would be useful to analyse what are the difference in spectra of transfer functions obtained by different methods.

I have decided to leave this figure out for the reasons discussed in my response to the previous reviews.

Reviewer 1: ‘Finally, p 6 lines 119-123 and p 4 line 156 make a strong statement that periods outside of the TF frequency range are predictable. As far as I am aware, this is mathematically impossible in general. Much more evidence is needed to substantiate this claim.’

I strongly agree with this comment. The new version of the paper has the same problem, Lines 175-179 on page X-8 are very confusing.

The following sentence was removed from the conclusions:

> “The pre-computed transfer functions for Method 3 do not include values for periods below 9 s; this may result in estimates of $GIC(t)$ that are less than that possible if a transfer function was computed that included lower periods.”

To address lines 175-179, I have modified the sentence to now read

> “The difference in prediction efficiency between Method 2 and Method 3 can be dependent on the zeroing of periods outside of the range of $9.1-18,725$ s, with the separation sometimes becoming larger when this constraint is removed. As an example, for RET54, the training/testing prediction efficiencies for E_x for Method 2 slightly decrease from 0.96/0.93 to 0.94/0.90 and for Method 3 they decrease from 0.89/0.92 to 0.79/0.77.”

I have removed the sentence starting with “This small improvement …” because the numbers for when periods outside of the range of 9.1-18,725 s have changed slightly, which was due to an error that was corrected in the program. Although it is possible that the PE could actually increase when a larger band of frequencies is considered, in this particular case it did not. I have computed the correlation coefficient of the prediction signal against the measure signal as a function of period and find that there are non-zero correlations outside of the range 9.1-18,725. Thus data outside of the range can be said to be predictable in the sense that positive correlations are obtained; however, in terms of the prediction efficiency, the PEs tend to be negative outside of the range 9.1-18,725. I have not included the above discussion in the manuscript because addressing the reviewers claim that it is mathematically impossible for data outside of the range 9.1-18,725 to be predictable would take a significant amount of discussion that is beyond the scope of this work.

Other comments:

Page 1, lines 17-19: ‘Method 3 produces in general reliable but slightly lower quality estimates than Method 2 for the time intervals and locations considered.’ Is it ‘in general’ or ‘for the time and locations considered’? It is the main point of the paper really, but it is not clear.

I have removed “in general” from the sentence.

Page 1, lines 15-17: ‘Method 1 produces average out-of-sample electric field estimation errors that can be equal to or larger than that measured (due to under- or over-estimation, respectively)’ Looks confusing for me, ‘produces electric filed estimation errors ... equal or larger than that (errors?) measured’?

The sentence has been modified to be:

“It is shown that with respect to these quality metrics, Method 1 produces average out-of-sample electric field estimation errors with a variance that can be equal to or larger than the average measured variance (due to under- or over-estimation, respectively)”

page X-8, lines 171-175, the author compares values of efficiency of Methods 2 and 3 for the cases when the periods outside of the range of 9.1-18,725 s are zeroing and not zeroing. It is not clear, how the author could get different values of efficiency for Method 3 if the transfer function for this method is initially bounded by 9.1-18,725 s period range (according to line122 on page X-6 and lines 136-137 on page X-7)

This was addressed in a response earlier in this document.

Page X-8, line 183. ‘The first and last 18,725 s were omitted in the computation of correlations and prediction efficiency’ It is almost 5 hours of two (or four?) considered days. I would suggest to explain why these data were omitted.

The text now indicates that this was done to exclude edge effects from the fourier inversion. These edge effects appear as large values in error at the start and end of the plots in Figures 2-5.

Page X-9, Line 205-206 and Figure 3: ‘The overestimation is visible in Figure 3.’ To be honest, I do not see that Method 1 (Figure 3, upper panel) provides overestimated values for the electric field, the error plot has both positive and negative values. Additionally, what does overestimating mean in this manuscript, E 1 -E 2 > 0 or abs(E 1) – abs(E 2) >0?

I have modified the manuscript to indicate that the magnitude is overestimated.

Page X-2, Line 28-29. ‘an effective approximation that may not reflect conductivity structures but produces reasonable GIC estimates’ (Boteler , 2015). I would rephrase in the form: ‘approximation that may not reflect detailed conductivity structures’

The suggested change has been made.

Figures 2-5

Check, please, the error plots.

For example, Figure 4, Method 2. The error plot looks suspicious around time 2.4-2.5. Black curve has two peaks, green curve does not, but the error plot (the difference) is smooth.

When I zoom in on the figures, I see two peaks for the green curve. I have checked the plots and the error curve is being directly generated from the difference in the time series that are also plotted.

Table 2 and Figures 2-5.

Efficiency coefficients PE x for Method 1 in (a)-(d) for training sets are not the same as in Figures 2-5. PE y values should be checked as well.

This has been corrected. (The entries for PEx for the training set in the table were a copy of PEy.)

Small comments:

Page X-4, line 85: Simpson and Bahr 2005 should be Simpson and Bahr [2005]?

This has been corrected.

Page X-4, eqn (2): should be division by /μ 0 in the second line.

This has been corrected.

Page X-8, line 154: σ t should be defined.

I have added the definition in the same sentence of its appearance.

Table 2: I would add description of notations ‘cc x ’ and ‘cc y ’ to the Table or to the text.

The definition of these has been added to the sentence that introduces Table 2.

Page X-4, line 85: Wait [1954] is absent in the reference list

The reference has been added.