Title: **Estimation of the 3-D geoelectric field at the Earth's surface using spherical elementary current systems**

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Recommendation: Minor Revision

1 General Comments

This is a very interesting paper which presents comparisons of many different methods for estimating geoelectric fields, including a novel SECS-based method. I have no concerns about the scientific content of the paper and thus my suggestions are primarily meant to improve readability for the intended audience.

The paper as currently written presents the different methods in a confusing way and often introduces new methods and ideas midway through the paper. As a result, the paper feels scattered and disjointed and I think several sections could be removed. To me, it would be much more logical to have a structure like this:

> Introduction: "In this paper, we are going to develop a new method for modelling geoelectric fields using SECS, and then we are going to compare that method to other existing methods such as Method A, B, C."
>
> Methods: Develop new SECS method and summarize all other methods in short sub-sections.
>
> Results: Show comparisons of novel method with Methods A, B, C in coherent figures.
>
> Discussion: Based on the results, modify SECS to include SECS+CF correction and decide that SECS+CF is preferred method. Discuss SECS+CF in relation to dH/dt, geoelectric polarization, and ionospheric fields.

Figures

It is difficult to make visual comparisons to the different modelling methods investigated. Here are some suggestions to make this section more readable:

(1) Only include one epoch in the main text. There is nothing in the text that requires showing more than one epoch. For completeness, you could place the second epoch in a supplementary material.

(2) Re-arrange figures and panels in a more logical fashion. The way the figures are arranged makes the comparison very difficult. Consider this sentence: "*The difference can be seen by comparing the respective DF electric fields in the bottom right panel of Fig. 3 and top right panel of Fig. 7 or the bottom right panel of Fig. 4 and top right panel of Fig. 8*." This is just unnecessarily confusing for the reader to keep track of the figures on different mismatched panels for multiple epochs.

(3) Even if you rearrange the figures, it is still often difficult to visually see the differences between the methods. Consider including additional figures which subtract one method from the other.

Minor Comments

Lines 29-53: This section should be moved to the SECS theory section, with the remaining introduction modified appropriately.

Line 145: What is R' variable?

Line 187: The σ value here is not a constant value but varies with position. Is there some way that you can denote this location dependence (e.g. $\sigma(\theta, \phi, r)$?

Line 197: The units of Q_{CF} are Volts? You provide an equation for the DF SECS (Equation 9), but do not provide an equation for the CF SECS. At the very least, a reference here would be helpful.

Line 203: This is the first time you have referenced grid cells. The entire Section 2 is only relevant for a single SECS pole, yes? But in reality, you are constructing model from a superposition of multiple poles. This should be clarified somewhere in Section 2.

Figure 3: How much are the internal components influenced by the local geology around a single magnetometer? For example, the ICK station has a very strong positive internal dB/dt at 23:16 which appears like a bullseye around that location. This corresponds to a very strong negative internal E_{DF}. The total E_{DF} seems to rotate around this single station. Could this apparent structure be due to a relatively small-scale geological feature local to ICK?

Figure 5: Is this the integrated conductance of the upper 0-10 km? Or is the first layer of the SMAP model 10 km thick? For clarity, I would recommend plotting several slices of the model which includes the true surface layer (i.e. the Gulf of Bothnia should have conductivity near that of sea water), as well as some additional intermediary slices at e.g. 0.5 km, 1 km, 5 km, etc.

Line 243: The conductance should be the sum of the conductivity in each vertical layer multiplied by the thickness of the layer. The way you have written Equation 25 does not make sense unless the first layer of the SMAP model is 10 km thick.

Figure 6: Are you interpolating the 0.5° x 1° DF SECS poles onto the 5' x 5' CF SECS poles grid prior to adding them together to get the total field?

Line 263: The total electric field is generally higher in areas of lower conductivity and lower in areas of higher conductivity because of the inductive (DF internal) component of the electric field regardless of whether there are any nearby conductivity gradients.

Line 274: Is the top of the PGIEM2G model space set at the SECS altitude, or is there padding above the SECS altitude?

Line 280: The terminology here is confusing. You are using PGIEM2G to estimate SECS amplitudes and then comparing them to SECS amplitudes?

Line 294: "SECS method reconstructs it from ground-based geomagnetic measurements"

Line 315-324: I am a bit confused by this section because you are using the *total* (internal+external) DF electric field structure. Even if you satisfy the plane-wave external source, you could still have small-scale aberrations in the internal DF electric field due to variations in ground structure (see previous comment on Figure 3 at ICK).

Line 328-329: "original PGIEM2G-modelled"

Line 342: “SECS-modelling based on PGIEM2G magnetic field”. Which method is this referring to?

Line 343-346: The MSTF method should be mentioned earlier and explained in more detail.

Line 350-351: Figure 15 shows the geoelectric field modelled with the original SECS only, or the SECS with the CF correction applied?

Line 355-360: What is causing the temporal variation in the DF and CF behavior? It is unintuitive to me that sometimes the internal DF would dominate while other times the external DF would dominate since the ratio of internal to external should be mostly determined by the fixed conductivity structure.

Section 3.5.1: This section should be removed. As far as I can tell, this section makes no reference to the new SECS method. You also specifically draw attention to the fact that your SECS and PGIEM2G geoelectric estimates are not possible: “the magnetometer network … was very sparse and the conductivity model in this region is known to be inaccurate”.

Section 3.5.2: This section should be removed as well, for similar reasons as above. Once again, as far as I can tell, you aren’t actually using any of the new methods to estimate the geoelectric field. You are just approximating the electric field using a simple relation to dB/dt at MEK (Equations 39, 40). It completely undermines the point of your paper to develop this SECS method and then say, “we can’t use this new method, so we are going to use a simpler method instead”.

Section 4.1: This section is confusing and unexpected. Based on the paper thus far (excluding Section 3.5), I thought the entire purpose of the paper was to compare different methods of geoelectric field estimation. So why would you not compare the different methods’ peak values? To me, that is the fundamental quantity for space weather hazards: *how big is the largest estimated geoelectric field and when and where does it occur*? Do the different SECS, PGIEM2G, MSTF, or 1-D methods result in different peak total electric fields? Are they different by a large amount (e.g. an order of magnitude) or a very tiny amount (e.g. 2%)? Do the peaks occur at different times or different locations?

Section 4.1: It is a bit perplexing that you only discuss the time of the peak, without ever mentioning in the text that the peak total geoelectric field is *72 V/km*. Pulkkinen et al. (2012) cite 20 V/km as an extreme 1-in-100 year geoelectric field event, and Lanabere et al. (2024) cite a value of 8.5 V/km as extreme for Sweden. Do you think that your >70 V/km value is realistic? Over what spatial and temporal scale does that peak value occur? More discussion of this is required.

Line 415-422: Continuing from the previous comment: from a space weather hazards perspective, it isn’t very relevant when the DF or CF peaks occurred. The only thing that really matters is when the *total* peak occurs, and how different modelling methods compare in this regard. The DF and CF peaks are mathematical abstractions that don’t actually occur in reality.

Line 425: The discussion about dH/dt is interesting and definitely relevant to the scientific community. You could improve this by computing the correlation coefficient between dH/dt versus E field. You could also specifically compare DF, CF and total (DF+CF) fields, with the expectation that the DF component would have the strongest correlation with dH/dt based on Equations 42 and 43.

Section 4.2: Line 435-440 could be mentioned elsewhere as a single sentence in the conclusion around Line 542. If you remove Line 435-440, then this section should be renamed "directionality of the E field" or something similar. Your discussion of polarization is very interesting since it represents a way of deriving E-field polarizations independent of standard MT techniques (e.g. polarization ellipses, Love et al., 2022). The importance of E-polarization in GIC studies has been highlighted in several papers (Cordell et al., 2021; Love et al., 2019, 2022; Malone-Leigh et al., 2024; Murphy et al., 2021). It would be interesting to explore this in more detail to discern if your polarizations match standard MT polarizations.

Section 4.3: This section should be removed, but I do think that a comparison to a 1-D model in the results section could be a useful addition since it is still common in the power industry. Showing how and why the 1-D method fails is useful.

Section 4.4: This is a very interesting section which could probably be it's own paper. Including the effects of Earth induction have been mostly ignored by the space physics community, especially those trying to run large MHD models to predict ground magnetic field behaviour (e.g. SWMF, MAGE, etc.).

Section 4.5: I am not sure if this needs to be its own section. You could probably just include a small statement about the permittivity when you introduce Equation 24.

References

Cordell, D., Unsworth, M. J., Lee, B., Hanneson, C., Milling, D. K., & Mann, I. R. (2021). Estimating the Geoelectric Field and Electric Power Transmission Line Voltage During a Geomagnetic Storm in Alberta, Canada Using Measured Magnetotelluric Impedance Data: The Influence of Three-Dimensional Electrical Structures in the Lithosphere. *Space Weather*, *19*(10), e2021SW002803. https://doi.org/10.1029/2021SW002803

Love, J. J., Lucas, G. M., Bedrosian, P. A., & Kelbert, A. (2019). Extreme-Value Geoelectric Amplitude and Polarization Across the Northeast United States. *Space Weather*, *17*(3), 379–395. https://doi.org/10.1029/2018SW002068

Love, J. J., Lucas, G. M., Rigler, E. J., Murphy, B. S., Kelbert, A., & Bedrosian, P. A. (2022). Mapping a Magnetic Superstorm: March 1989 Geoelectric Hazards and Impacts on United States Power Systems. *Space Weather*, *20*(5), e2021SW003030. https://doi.org/10.1029/2021SW003030

Malone-Leigh, J., Campanyà, J., Gallagher, P. T., Hodgson, J., & Hogg, C. (2024). Mapping Geoelectric Field Hazards in Ireland. *Space Weather*, *22*(2), e2023SW003638. https://doi.org/10.1029/2023SW003638

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