Comment on angeo-2022-4

Anonymous Referee #2

Referee comment on "The time derivative of the geomagnetic field has a short memory"
by Mirjam Kellinsalmi et al., Ann. Geophys. Discuss.,
https://doi.org/10.5194/angeo-2022-4-RC2, 2022

General comments:

The present manuscript describes results based on the analysis of several quantities derived from the horizontal projection of the magnetic field throughout the high-latitude IMAGE magnetometer network. The study has a clear precedent in the valuable article by Juusola et al., 2020 (which they cite). To carry out the present analysis, the authors rely on the equivalent source method of Spherical Elementary Current Systems (SECS), with nodes in the ionosphere and subsurface to perform the external/internal separation of the modeled magnetic field. Based on this separation, they analyze different aspects of the behavior of both the horizontal projection of the magnetic field and its time derivative, such as preferent directionality and associated persistence in time, change in amplitude, etc., always distinguishing between external and internal components. One of the main conclusions of their analysis -as the title immediately reveals- is that the direction of the time derivative of the horizontal magnetic field during disturbed times does not persist beyond a couple of minutes.

Even if this result is significant in itself, I have serious doubts that it is sufficiently relevant to merit an exclusive publication, especially if this subject has already been investigated by other researchers, who reached similar conclusions, though perhaps using different methods and parameters (Belakhovsky et al., 2018; Weygand et al., 2021, Pulkkinen et al., 2006). Furthermore, the present study is based entirely on reliance on the external/internal separation of the geomagnetic field provided by the SECS technique; however, no assessment is made of the uncertainty of this separation, as the authors themselves acknowledge. For these reasons, I strongly recommend including additional, substantial material based on the following

Specific comments:

- The authors argue that their findings are important to the subject of GIC (this word is repeated a number of times along the manuscript) but they do not substantiate this argument based on GIC measurements of any type. Could the authors provide evidence that their conclusions are somehow reflected in GIC measurements? For example, is it expected that the GIC (which depends on the time derivative of the horizontal magnetic field) has a typical lifetime of two minutes, comparable to the directional persistence of d$\mathbf{H}$/dt? If not, is it because the infrastructures (e.g., power network or oil/gas pipelines) where the GIC is expected to flow have not a preferent direction (e.g., N-S or E-W)? Also, the authors point that the final aim is to forecast GIC. I guess GIC can be predicted by trying to anticipate the ground magnetic variations based on IMF/solar wind observations, along with accurate models of the ground conductivity. Can they specify more clearly how is it expected that the main conclusion of the manuscript (i.e., the "short memory" of the time derivative) helps in this endeavor? Perhaps they refer to "evaluating a potential GIC risk level by means of the dH/dt proxy" rather than "forecasting GIC"?
- The present study is entirely based on the reliance on the external/internal separation of the geomagnetic field provided by the SECS technique; however, the effectiveness of this method is subject to different aspects, such as the nature of the primary/secondary sources, the density of ground magnetometers, or the election of the cutoff parameter for the singular values of the singular value decomposition (SVD) typically used in the context of SECS, among others. The authors have ample experience on this technique, so they should be able to provide an estimate of the uncertainty of the modeled magnetic field and of its external/internal separation in particular. I'm not aware of many articles where this important subject is treated, but perhaps the following thesis can help: https://open.uct.ac.za/handle/11427/35593 (see section 7.2). If this implementation is not feasible, I encourage the authors to at least apply the alternative method of field separation they refer to in Section 4.1 in order to assess how much of the separation depends on the method utilized.

Technical corrections:

- L19: I would suggest: "Space weather **events**, **eventually produced by eruptive phenomena in the Sun**, can have harmful effects on Earth, **for example** via …"
- L33: Faraday's **induction** law**.**
- Figure 1 is somewhat naïve, and in my opinion unnecessary -just consider my comment as a recommendation. In its place (though perhaps not as Figure 1), I would find more useful to illustrate the concept of Δθ($\mathbf{H}$) and, if possible, that of Δθ(d$\mathbf{H}$/dt), which is central to this manuscript. I think the horizontal projection of the geomagnetic field can be represented at times t and t + T as two arrows, and then represent the corresponding variation in θ.
- L62 "2D SECS": I guess you have used internal and external nodes for the field separation. Is there a specific designation for this modality to differentiate it from the use of external nodes only (which would be the case to model the total horizontal field when there is no need for external/internal separation)?
- L75: The IMAGE time resolution is 10 s. Does the threshold of 1 nT/s refer to a mean variation computed as 10 nT in those 10 s? If so, I think the authors should state it.
- The authors always refer to Bx, By and H whereas section 2.1 specifies that baselines are subtracted from the data using a certain automatic method. In consequence, they work with variations of those quantities. I think this point is important and the nomenclature currently used may give rise to confusion. Properly speaking, the studied quantities are ΔBx, ΔBy and Δ$\mathbf{H}$ (where, e.g., Δ$\mathbf{H}$ = $\mathbf{H}$ − $\mathbf{H}_b$, with $\mathbf{H}_b$ the baseline

value). I strongly recommend using the deltas before these quantities everywhere.
- Table 1, in 2nd line replace H with $|\Delta\mathbf{H}|$; and in 4th line replace dH/dt with $|d\mathbf{H}/dt|$.
- L96: Bx and By should not be in bold face. Idem for caption of Figure 3.
- L98: Replace d**H**/dt with |d**H**/dt|.
- Caption of Figure 3: I would recommend to state "Figure 3. Plot of different quantities related with the horizontal magnetic field at Tromsø, …". 4) **Amplitude of the** time derivative …
- L116: No mention is made of stations in Svalbard and surroundings, which do not appear in Figures 4, 5 and Table 2 (and indeed anywhere except for the map in Figure 2). Are they only used for the purpose of the SECS-based external/internal separation?
- Caption of Table 2. Say the stations are ordered by latitude.
- L124: "… **over** the years"?
- Figure 6: The number of data points in SOD for 2017 shows 32443 against the 32436 of Table 2. Isn't that an inconsistency?
- Figure 7 and others: I would use "dH/dt" instead of "dH", as in the text.
- Draw the line corresponding to the even distribution of Δθ in figure 10a.
- L149: Figures 10 and 11 show **the standard deviation of** …
- What is the meaning of the last sentence in the paragraph L149 – L153?
- L154: Figure 13 is referred before Figure 12. I would recommend following the logical ordering.
- Figure 8: Show a title for the x- and y-axis for at least one of the subplots, e.g., "MLT (h)" and "# of events". Also, MLT = 25 sounds bad. Please, place ticks at 0, 12 and 24 h.
- Figure 9: y-axis is missing a "mean θ" (or equivalent) followed by the station name, e.g., <θ> KIL.
- Figures 10, 11 and 13: Likewise, y-axis is missing a "Δθ".
- Figure 14: y-axis is missing an "R". Caption: Specify that the bars indicate the std of R.
- Paragraph L185-189: Only Figure 8 is mentioned, but the fact that the magnetic field is predominantly southward is shown in Figure 9.
- L191-193: Please, refer to a specific figure the reader should look at. Do the authors refer to Figure 5 (right) here? If so, I don't see an especially narrow distribution at MAS station (unless I get confused with nearby stations); instead, other nearby stations like IVA show a yet narrower distribution.
- In the context of the discussion of the coast effect (L193), comment that the distributions of dHint/dt at DON and RVK have a significant component perpendicular to the coast.
- Section 4.2.1: I suggest removing the discussion on how you have achieved the mean direction of dH/dt here. This has been defined in Section 2.2 (Methods section). Move the mention of the Davis (2002) method to section 2.2.
- L234: Figure 3, panel 4)
- Section 4.5: The reader is left with the idea that, despite the efforts made in this manuscript, forecasting GIC is still an equally distant undertaking. Do they really want to transmit this notion, perhaps in line with the conclusion of Pulkkinen et al., 2006, that "dBx/dt and dBy/dt fluctuations are not even in principle predictable in a deterministic way"? Moreover, please note that forecasting GIC (title) is not equivalent to forecasting dH/dt (first line). Did the authors mean "dH/dt" in the title instead of "GIC"? Also, L151-153 are especially confusing to me. For these reasons, I would recommend either rewriting this section more clearly or consider removing it.