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Reply on RC2

Mirjam Kellinsalmi et al.

Author 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-AC3, 2022

We greatly appreciate these comments. They are extremely helpful in stimulating further discussion in the manuscript. We understand a few chapters in our manuscript need more elaborating, especially we should focus on explaining the significance of the magnetic field separation, and dH/dt relation to GIC. Here are our specific responses (in bold) to the referee comments:

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).

Belakhovsky et al. applied the RB parameter, which describes the variability of a vector in time and space. No characteristic time scales are determined through this analysis. As previously replied to Ref-1, the time scale that Weygand et al. discuss is different from ours. They study the persistence of large time derivative values, whereas we study persistence in magnetic field directions. However, their study provides useful insight, which is relevant also to our manuscript. In the study by Pulkkinen et al., one difference in the methodology is that they consider the magnetometer network as a whole through a structure function, while we study explicitly H and dH/dt at single stations. Overall, they have a similar result using an entirely different method.

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.

Solar wind-magnetosphere-ionosphere interaction creates electric currents in the near-Earth space. The temporally varying "external" magnetic field of these currents drives induction in the conducting ground. The "internal" magnetic field of the induced electric currents is superposed to the external magnetic field, creating the measurable geomagnetic variations. If the driving external geomagnetic variations and a detailed model of the ground conductivity are given, 3D induction modeling can estimate the internal geomagnetic field and the

geoelectric field (e.g., Marshalko et al., 2021). The geoelectric field is the driver of GIC in a conductor system, but the amplitude of the GIC is also affected by the parameters of the system.

Study of the characteristics of internal geomagnetic variations provides information on the complicated process of geomagnetic induction. Study of the characteristics of external geomagnetic variations, on the other hand, provides information on the solar wind-magnetosphere-ionosphere interaction. Global simulations typically estimate only this part of the geomagnetic field, and assessment of their abilities in this regard requires understanding of the characteristics of the external geomagnetic variations. Thus, study of the characteristics of both internal and external geomagnetic field can improve our ability to forecast space weather events that cause large GIC. The reliability of the method is discussed more in the specific comments below.

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.

We thank the referee for stimulating a more detailed discussion on the relation between GIC and dB/dt, and of forecasting GIC (and dB/dt). As shown in many previous studies, the temporal behavior of GIC typically follows dB/dt at a nearby location. So, dB/dt is a good proxy. A full modeling requires determination of the geoelectric field and including a model of the conductor system in question, but this is beyond the scope of our manuscript.

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 dH/dt?

When talking about a typical lifetime of GIC, some care is needed. An obvious choice is to consider the length of events when GIC exceeds a given threshold. The larger the threshold the longer the event. This was noted by Viljanen et al. (2014, Figs. 9-10) who considered the GIC sum in a large grid. Due to the close relation of GIC and dB/dt, we could as well consider the persistence of large dB/dt values. As expected, durations of large dB/dt events are short (e.g., Weygand et al., 2021, cf. comments by Ref-1). The same was also shown by Juusola et al. (2015) in terms of the time derivatives of equivalent currents.

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)?

There seems to be no specific direction to which a conductor system is most sensitive to GIC, at least at higher latitudes such as the IMAGE magnetometer network. Since the directional distribution of the total dH/dt (ext+int) is very scattered then the same holds true for the geoelectric field.

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?

Ironically, our result does not evidently help in deterministic forecasting! We can only agree with Pulkkinen et al. (2006) that dB/dt "fluctuations are not even in principle predictable in a deterministic way; nature sets boundaries for the accuracy with which we can forecast the future"

There is a possibly interesting spin-off concerning simulations. A simulated ground magnetic field and its time derivative should show similar features as the measured field. So we could repeat our analysis for simulated fields and especially check whether the same time scale for dB/dt appears. If not then some fundamental physics is missing in the simulation. As a side note, simulations provide primarily only the external part of the ground field. So a proper reference from measurements is the separated external contribution.

Perhaps they refer to “evaluating a potential GIC risk level by means of the dH/dt proxy” rather than “forecasting GIC”?

Actually not. GIC risk level is obviously very much related to the magnitude of dH/dt (and the geoelectric field). This is a different aspect than trying to understand why dH/dt has a very complex behavior. Citing Pulkkinen et al. (2006, paragraph 42): "The most dramatic change in the observed dynamics occurred in the dBx/dt and dBy/dt fluctuations at temporal scales between 80 < t < 100 s. These scales are naturally linked to corresponding scales in the dynamics of the ionosphere-magnetosphere system. However, the link is all but self-evident and we postpone further speculations to forthcoming investigations." It seems that "the link" is still quite much unsolved.

- The present study is entirely based on the reliance on the external/internal separation of the geomagnetic field provided by the SECS technique;

Even if there are uncertainties in the field separation, the main result concerning the time scale of dH/dt does not change. It could also be determined without the field separation. E.g. Δθ timescale is visible in both, external and internal dH/dt, so it is also visible in the total dH/dt. See attached figure.

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.

In our application of the 2D SECS method, the cutoff parameter for singular values of the singular values decomposition is zero. As a consequence, all components of the observed geomagnetic field are perfectly reproduced at all stations used in the analysis. In this study we do not use interpolated values between stations, so considering the reliability of the solution away from the stations, as is in done in the thesis suggested by the reviewer, would not help.

Measurement errors of the magnetometers are also very small. Of course, the separation of the geomagnetic field is not perfect and is affected by the density of the magnetometers as well as the boundary conditions, as discussed by Juusola et al. (2020). Unfortunately, estimating this uncertainty is not at all simple. Implementing another separation method does not affect these sources of error.

The internal part of the separated field has been shown to follow well the known structure of the ground conductivity (Juusola et al., 2020) and correlation between the electrojet currents derived simultaneously from IMAGE and low-orbit satellite have been shown to significantly improve when the separation is carried out (Juusola et al., 2016). These results indicate that the separation should be fairly reliable.

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 …"

Will be reformulated

- L33: Faraday's induction law.

Will be reformulated

- 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(H) and, if possible, that of Δθ(dH/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.

We will consider changing this figure.

- 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)?

We will add a slightly longer description of the 2D SECS method in the manuscript to clarify this point.

- 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.

Yes, this is correct, and we will define it more precisely in the text.

- 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 ΔH (where, e.g., ΔH = H – Hb, with Hb the baseline value). I strongly recommend using the deltas before these quantities everywhere.

Ignoring Δ is a common practice within space physics community, and makes notations a little simpler. We will add a mention of this in the text.

- Table 1, in 2nd line replace H with |ΔH|; and in 4th line replace dH/dt with |dH/dt|.

Will be revised.

- L96: Bx and By should not be in bold face. Idem for caption of Figure 3.

Will be revised.

- L98: Replace dH/dt with |dH/dt|.

Will be revised.

- 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

Will be reformulated.

- 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?

The Svalbard stations are included in our analysis, e.g. Fig. 13. They were not included in the maps (Fig. 4 & 5) to make the plots easier to read.

- Caption of Table 2. Say the stations are ordered by latitude.

Will be added.

- L124: "… over the years"?

Will be revised.

- Figure 6: The number of data points in SOD for 2017 shows 32443 against the 32436 of Table 2. Isn't that an inconsistency?

Will be checked and corrected.

- Figure 7 and others: I would use "dH/dt" instead of "dH", as in the text

Will be revised.

- Draw the line corresponding to the even distribution of Δθ in figure 10a.

Will be added.

- L149: Figures 10 and 11 show the standard deviation of …

Will be revised.

- What is the meaning of the last sentence in the paragraph L149 – L153?

Will be explained more clearly.

- L154: Figure 13 is referred before Figure 12. I would recommend following the logical ordering.

Will be revised.

- 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.

Will be revised.

- Figure 9: y-axis is missing a "mean θ" (or equivalent) followed by the station name, e.g., <θ> KIL.

Will be revised.

- Figures 10, 11 and 13: Likewise, y-axis is missing a "Δθ".

Will be revised.

- Figure 14: y-axis is missing an "R". Caption: Specify that the bars indicate the std of R.

Will be revised.

- Paragraph L185-189: Only Figure 8 is mentioned, but the fact that the magnetic field is predominantly southward is shown in Figure 9.

Text will be revised to also mention Fig. 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.

Attached is Fig. 5 of the manuscript, with MAS and IVA station locations indicated (right). MAS has a more narrow 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

This is a good point, mention of this will be added.

- 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.

Will be modified.

- L234: Figure 3, panel 4)

Will be modified.

- 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"?

Yes, this is what our results indicate. See also previous comments. We thank the referee for opening the discussion. We will add some further discussion on this in section 4.5.

- 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.

Major additions and modifications will be made in this section. See also previous comments.

References

Juusola, L., Kauristie, K., Vanhamäki, H., Aikio, A., and van de Kamp, M. (2016), Comparison of auroral ionospheric and field-aligned currents derived from Swarm and ground magnetic field measurements, J. Geophys. Res. Space Physics, 121, 9256– 9283, doi:10.1002/2016JA022961.

Please also note the supplement to this comment:
https://angeo.copernicus.org/preprints/angeo-2022-4/angeo-2022-4-AC3-supplement.pdf