The geomagnetic superstorm of 10 May 2024: Citizen science observations

by M. Grandin et al. (Egusphere-2024-2174)

Response to Reviewer 1

We thank Dr. Jaynes for her highly positive assessment of our manuscript and for providing suggestions for improvement. In our response, we reproduce the Reviewer's comments in italics and give our replies in blue. Unless specified otherwise, the line numbers we refer to in our replies correspond to those in the **manuscript with highlighted changes**.

This publication details the collection and results of a citizen science effort to study the May 2024 superstorm. The results show compelling observations that are not possible through the traditional means due to the latitude extent of such a large geomagnetic disturbance. And the results also show detailed scientific characteristics of the storm, as exhibited by the particle energy content estimates as well as the reports of technological interruptions. Overall, this is an impressive overview of the May 2024 superstorm from the citizen science data collection perspective and offers tangible leads for further scientific research from a simple and elegantly-executed system of volunteer reporting.

The paper is in publishable form as it is. However, I have some suggestions (mostly that the authors could take or leave) so I submitted it as "accepted subject to minor revisions". The suggestions are listed below.

Thank you so much for this extremely positive and encouraging introductory statement!

Line 26: I very much suggest naming this as the Gannon Storm for a couple reasons. (1) There will not ever be a consensus given by an authoritative source; the name that sticks is the one that most people use in the literature, so you are part of that decision-making here. (2) I think the Gannon Storm is the most appropriate name, both in the memory of Jenn but also in that the Mother's Day name is not meaningful in much of the world that does not celebrate that holiday. Naming it for an impactful scientist is more genuine, I think. Also, there is a delegation to the US Space Weather Action Policy group that will recommend to name this storm for her officially (I think it happens later this month or next month). This is, of course, just suggestion and I'm not fiercely adamant about the change.

Thank you for providing those arguments in favour of choosing the Gannon Storm terminology. We appreciate these, and we decided to adopt this name in the revision of our paper (though we kept the brief discussion on the "Mother's Day Storm" alternative in the introduction, l. 29–33). We have also reflected this choice at the beginning of the abstract (l. 1).

Line 41: Might be worth mentioning the 'CME that missed' study from 2013. Baker, D. N., Li, X., Pulkkinen, A., Ngwira, C. M., Mays, M. L., Galvin, A. B., & Simunac, K. D. C. (2013). A major solar eruptive event in July 2012: Defining extreme space weather scenarios. Space Weather, 11(10), 585-591.

Thank you for suggesting this reference, we have added it in the revision (l. 48–50).

Line 108: Are you only using Wind data? I think if you are using OMNI data products, you are using a combination of Wind, ACE and DSCOVR.

Thank you for pointing this out; we have corrected our statement in the revision (l. 121).

Line 118: Have you reached out to anyone to see if Artemis could provide solar wind data during the gap? They sometimes are in the right place and have reliable data to fill in the OMNI data. Not sure it matters enough to be worth the effort for this, though.

We have looked at the position of the Artemis spacecraft during the storm. It seems that, while the satellites were indeed located on the Sunward side of the Earth (waxing crescent Moon phase), they were quite significantly off the the Sun–Earth axis ($X_{GSE} \sim 50\ R_E$; $Y_{GSE} \sim 40\ R_E$), meaning that it would not be very straightforward to propagate their observations to the Earth's bow shock to obtain data comparable to OMNI and possibly bridge the gap. Since this is not a central aspect of our paper, we prefer to leave this kind of analysis out.

Nevertheless, it is good to know about this possible approach when in need to fill in gaps in solar wind data in any case, thank you for bringing it up!

Lines 127-145: While this detailed description of the storm characteristics and phases is worthwhile, I was left wondering how this compares to the rest of the solar cycle. It might be a nice addition to add some comparisons such as the last time Dst was below -400 nT or how often the SME reached similar or greater values throughout the previous decade. This should be fairly easy with the historical OMNI data. I think it would add a very nice context to all the numbers quoted in these paragraphs.

Great suggestion! We have added such elements of discussion for selected thresholds of SME and SMR (since these are the indices we are using in our study) since the Halloween events of 2003 to put things in perspective (l. 150–154 and l. 161–163). We also mention how this compares to the events during the previous solar cycle (SC24).

Lines 203-204 and Figure 2: It is nearly impossible to distinguish the blue dots from the black dots so I didn't even realize there were Skywarden reports from Canada, New Zealand, etc until reading these lines and I can't pick them out even knowing they are there. Would it be possible to make them X's or some other symbol and also maybe a different color to help differentiate?

Thank you for pointing this out, we have changed the markers for Skywarden data by making them bigger and as triangles. This enables seeing the Skywarden observations from Canada and New Zealand amid the small black dots from the survey.

Figure 5: Is it possible to add horizontal grid lines for the major ticks? I was wanting to compare the exact number of different colors at different locations but the format makes it difficult to do so. It sort of looks like pink/magenta outpaces green at all latitudes(!) but it's a bit hard to tell.

Good suggestion, we have added the grid lines in the revised version of the figure.

Figure 6 and Lines 320-325: I think it would be very valuable to show the histogram of SZA for the green reports. I suspect you'll see the opposite distribution - most of the green reports occurring at the higher SZAs. It would be interesting to see the comparison. I don't know if it's better to overplot the data (maybe with open, unfilled bars?) or to make a new panel.

Thank you for the suggestion; we have added the SZA distribution for green aurora reports in Fig. 6a as unfilled bars, and it looks good. Interestingly, the distribution is actually very similar to the blue and purple/pink/magenta one! This suggests that most observations were made in conditions when the Sun was not very low below the horizon, which can be

explained by the geographic distribution of the reports (predominantly Europe and North America, getting close to summer) and their local time (predominantly early night), as discussed in the interpretation of Fig. 4a. However, this does not change the conclusions concerning a possible reason for the numerous reports of pink/purple/magenta aurora being the resonance scattering mechanism.

In addition to adding the green-aurora SZA distribution in Fig. 6a as suggested, we have added a few lines summarising these thoughts (l. 344–353).

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Response to Reviewer 2

We thank the Reviewer for their positive assessment of our manuscript and for providing suggestions for improvement. In our response, we reproduce the Reviewer's comments in italics and give our replies in blue. Unless specified otherwise, the line numbers we refer to in our replies correspond to those in the **manuscript with highlighted changes**.

Summary

This is an excellent manuscript that has gathered citizen science contributions for reports of the auroral witnesses and disruptions of the human-made infrastructure. These results are showing how useful citizen science approach could be. In this sense, the authors' manuscript is a welcome contribution to the scientific community. However, some of their discussions do not look so new. This is especially the case with Section 2, Section 4.1.3, and Section 5.4. The authors should remove these sections to minimize their overlap with the previous studies and clarify their own novelty for further consideration on their potential publication. The authors need to appropriately cite the existing studies and clarify whether their results support these studies or not.

Thank you for this encouraging overall statement. We have critically pruned the text to shorten a bit these sections where appropriate and referred to the studies which have been published since the initial submission of our manuscript.

Major Comments

1. Section 2 gave me an impression of déjà-vu without much novelty, as such analyses have been already posted elsewhere (DOI: 10.4401/ag-9117; DOI: 10.48550/arXiv.2408.14799; DOI: 10.48550/arXiv.2407.07665; DOI: 10.5140/JASS.2024.41.3.171). This section does not bring something significant new against these previous studies. This section neither helps their overall discussions. Therefore, the authors should remove this section. If some of their information is essential, the authors should include their minimal summaries in the introduction rather than their main body.

We thank the Reviewer for pointing to these studies which also present and discuss the driving conditions which led to the 10 May 2024 geomagnetic storm. While we appreciate that the drivers are not the main focus of our study, we however believe that it is always important to provide the context in which observations are made, especially minding that our readership in *Geoscience Communication* is not necessarily from the space physics community. It is also easier for the reader to be able to find a plot of the driving conditions during this event without needing to look for it in other publications, and since we do discuss some of our results in relation to the geophysical context, it makes sense to have such a figure and a brief discussion of it in Section 2.

Moreover, we wish to underline that two of the suggested references are currently available as preprints only, and a third one became available after we submitted our manuscript in July. The fourth reference is hence the only one which was available at the time when we wrote the manuscript, and it is already cited.

We have, whenever possible, referred to the accepted and published articles on the Gannon Storm in our revision. In particular, we have referred to Kwak et al. (2024,

doi:*10.5140/JASS.2024.41.3.171*) several times in the revised manuscript, in particular when discussing the driving conditions (l. 118–119).

2. The authors emphasize their data significance, based on the lack of midlatitude observations. However, in reality, the midlatitude auroral activity has been monitored with the Super Dual Auroral Radar Network (DOI: 10.1186/s40645-019-0270-5). The authors need to compare their results with the Super Dual Auroral Radar Network (DOI: 10.1186/s40645-019-0270-5), to substantiate their statement.

Thank you for mentioning the fact that SuperDARN radars provide observations of the mid-latitudes. However, we believe that our statement underlining the scarcity of midlatitude observations of the aurora still holds, for two main reasons. First, SuperDARN is a network of radars, and as such, it does not provide optical data that would enable inferring information on the aurora that we could directly compare with our data. The mid-latitude radar echoes are often in the subauroral region like SAPS. Auroral echoes can also be seen, but they are usually in the high-latitude portion of the field of view. Second, while part of the mid-latitudes are indeed covered by SuperDARN, many of the regions from which citizen scientists reported seeing the aurora are outside of the radar coverage: most of Australia, the north of New Zealand, almost all of Europe, the southern USA and Mexico, as well as the regions from which we received a few reports (South America, Namibia, China, India).

When revising the paper, we have added a mention to SuperDARN and refer to the suggested article (l. 56–58).

3. Their statement on the auroral oval expansion (P11-12) contradicts what DOI: 10.4401/ag-9117 stated. This article concludes that the actual auroral oval was not that wide (only down to 45-50 degree) and what was witnessed in the lower latitude should have been SAR arcs. The authors need to revise their statement based on DOI: 10.4401/ag-9117 or show enough evidence to overturn what DOI: 10.4401/ag-9117 stated.

What Spogli et al. (2024) state is that “It appears highly improbable that auroras could have been visible as far South as Italy or even Africa.” Their statement is based on the fact that the OVATION Prime model output did not produce an extension of the auroral oval to such low latitudes. The authors suggest that the reports from those regions might actually be SAR arc observations, but their phrasing remains careful on the matter. In fact, looking at the picture taken from Stintino (Italy) by Fabrizio Perra and shown in their Fig. 11, there is very clear evidence of field-aligned structuring in the optical emission, hinting at aurora rather than SAR arcs which are generally diffuse. This is also consistent with the report of “red and violet glows” in their paper: SAR arcs are monochromatic (red emission). Besides, we can note that the Spogli et al. (2024) study focuses on the European-sector observations, whereas our work also considers observations from North America which was near midnight at the peak of the storm main phase.

We therefore do not think that our statement on the auroral oval extent during the geomagnetic storm, based on our data, needs to be revised.

4. Their Section 4.1.3 and Figure 5 are not new, as similar results have been already published elsewhere (e.g., Figure 18 of DOI: 10.48550/arXiv.2407.07665). The authors should either remove this section. Otherwise, the authors need to minimize their description (down to 30% of the current amount) and clarify whether their results support these precious studies or not to clarify their novelty.

The paper to which the Reviewer is referring (by Hayakawa et al.) is a preprint which was uploaded to arXiv only two days before we submitted our manuscript to Geoscience Communications. It is therefore incorrect to refer to this paper as “published”.

In any case, Fig. 18 of Hayakawa et al., while also focusing on reported colours of auroral emissions during the 10 May 2024 geomagnetic storm, was based on a different data set than ours (76 reports from a different online survey). Therefore, it will be very interesting to discuss the differences and similarities between their results and ours, but it would seem quite inappropriate to not show the auroral colour distribution in our paper and discuss it.

We have added a mention to the Hayakawa et al. preprint when introducing our survey at the beginning of Sect. 3 (l. 176–178).

5. The satellite drags (Section 5.4) have been already discussed in detail in DOI: 10.2514/1.A36164. It is not immediately clear how their citizen science approach can benefit discussions on the satellite drags either. The authors need to remove Section 5.4. Otherwise, the authors need to minimize their description (down to 30% of the current amount) and clarify whether their results support these precious studies or not to clarify their novelty.

We agree that the discussion on storm effects on satellite drag is a bit deviating from the main focus of our study. However, we think that the quantitative estimates of LEO satellite altitude loss imputable to the storm that we provide with Fig. 9 are new compared to the results presented in Parker and Linares (2024), which is already cited in our manuscript. While Parker and Linares (2024) carried out an extensive analysis on atmospheric density increase inferred from satellite drag and analysed the manoeuvres by satellite operators following their satellites' altitude loss, we look at four individual LEO objects and quantify for each of them the altitude loss imputable to the storm. Our purpose here is to add to the discussion that publicly available data can also be used for doing science.

After careful consideration, we realised that we cannot easily shorten the section. Indeed, the first part consists of explanations needed for the reader to understand which data we are showing in Fig. 9 and how we have processed them. This is followed by a statement quantifying the results we obtain, and a discussion putting our results in perspective with other recent papers, which the Reviewer invited us to do more in their earlier and below comments. We have therefore only been able to remove a sentence which was not essential.

6. The authors should appropriately show what has been revealed in the previous studies for this storm and whether their results support the previous studies or deny their results. Even for the geophysical contexts, auroral activity, and satellite drags, we can easily raise the following studies (DOI: 10.4401/ag-9117; DOI: 10.48550/arXiv.2408.14799; DOI: 10.48550/arXiv.2407.07665; DOI: 10.5140/JASS.2024.41.3.171; DOI: 10.2514/1.A36164). There are many more. The authors should contextualise their manuscript to these previous studies.

As mentioned in our response to point #2, most of the studies the Reviewer refers to were not yet available at the time when we submitted our manuscript, and several of them are also at the stage of preprints – they hence cannot be qualified as "previous studies" compared to ours.

In the revision, we now refer to the following recent papers on aspects of the Gannon Storm directly comparable to our results: Spogli et al. (2024), Kwak et al. (2024), Parker and Linares (2024), Evans et al. (2024), Foster et al. (2024).

7. The manuscript is extremely long. This reduces the readability. The authors should compress the length of the manuscript to 50% to 60%. For this, the authors should remove their sections overlapping with the previous studies.

While we acknowledge the fact that this is a long manuscript (the revised version contains ~11,000 words and 9 figures), we do not understand the reasoning behind the need for reducing its length to 50% to 60%. These numbers seem a bit arbitrary, and it is unreasonable to remove entire sections on the basis that other studies, which were submitted after our manuscript or at the same time, address similar aspects. In any case, our data set and methods are largely different from those presented in the other studies the Reviewer has referred to, so we believe that there is room for everyone to contribute to the scientific discussion in a constructive rather than competitive way.

Given the quantity of data that we are showing, and since we believe that discussing the results with respect to other publications is valuable, we cannot significantly reduce the manuscript's length without compromising its scientific integrity. We have nonetheless done our best to remove the few sentences that we found superfluous throughout the manuscript.

8. It is not immediately clear what the authors have revealed in this study, in comparison with what has been already revealed in the Aurorasaurus Project. The authors' conclusions 1 and 2 look evident from the Aurorasaurus Project. The authors should clarify their own novelty against the Aurorasaurus Projects.

We appreciate the importance of Aurorasaurus and would have included a discussion of results on the 10 May 2024 storm as yielded by the Aurorasaurus project if there were any published work presented these. However, at the time when we are submitting our revision, we did not manage to find any Aurorasaurus paper showing results from the Gannon storm. It will be very interesting to compare the results yielded by the reports from our survey and from Aurorasaurus in future, once both teams have managed to publish their data.

Minor Comments

P1 L4 The authors have stated that cameras are not from traditional scientific instruments. This is slightly different from the reality. Cameras have also been used for auroral observations for more than decades. The authors can easily reference several scientific achievements from the early auroral photographs even within recent highlights such as those in Sodankylä (DOI: 10.1007/978-3-642-31457-5; DOI: 10.5194/hgss-15-17-2024).

Thank you for underlining that our phrasing ("Hence, many people not only witnessed but also photographed the aurora during this event. These observations, although not from traditional scientific instruments...") may be considered ambiguous. We have rephrased this statement to make it clear to the reader that we are referring to citizen science observations as opposed to observations by instruments maintained and operated by researchers or engineers for the sake of doing science (l. 4–5).

P1 L11 The authors have considered their main finding as "the aurora was widely seen from locations at geomagnetic latitudes ranging between 30◦ and 60◦, with a few reports from even lower latitudes". However, similar finding has been already reported in multiple articles (DOI: 10.4401/ag-9117; DOI: 10.48550/arXiv.2407.07665).

This comment seems to partly contradict the Reviewer's major comment #3 underlining that Spogli et al. (2024) suggested that the auroral oval might not have reached latitudes as low as Italy or the southern tip of Africa. And as explained in earlier replies, Hayakawa et al. is still at the stage of preprint. In our revision, we have added references to compare and discuss our results where relevant – for instance Evans et al. (2024) on thermospheric heating during the Gannon Storm (l. 504–505) and Foster et al. (2024) on total electron content enhancement associated with low-energy precipitation (l. 351–353).

P2 L20 The aurorae were seen even before Babylonians. (DOI: 10.1007/s11214-023-01018-1). This statement needs to be corrected.

Thank you for pointing to this very interesting paper! We have amended our statement accordingly (l. 24–25).

P2 L23 It might be worth citing statistical studies to support this statement here (DOI:10.1029/2020JA028284).

Thank you, we have added a citation to this paper here in the revision (l. 27).

P2 L25 The latest storm before this took place in November 2003 rather than in the Halloween of 2003 (DOI: 10.1029/2018JA026425).

We have added a precision that the “Halloween storms” of 2003 took place in October and November of that year (l. 28).

P2 L28 For this storm, the source active regions and coronal mass ejections have been already documented in the previous studies (DOI: 10.4401/ag-9117; DOI: 10.48550/arXiv.2407.07665). They needed to be mentioned here.

We have included references to Spogli et al. (2024) and Kwak et al. (2024) on this line (l. 35).

P2 L32 Some articles have been published for the space-weather impact of this events such as the satellite drags (DOI: 10.2514/1.A36164).

The statement on P2 L32 is a general statement, not specific to the 10 May 2024 event. The references we have already cited in this paragraph are therefore more relevant than Parker and Linares (2024), which we cite later in the manuscript.

P2 L41 Their statements on the Carrington storm is somewhat outdated. A lot have been studied for the Carrington event in the last two decades after Siscoe et al. (2006).

Siscoe et al. (2006) is the study which estimated the Dst index associated with the Carrington storm, so it feels like a very appropriate reference to back up the statement that “the September 1859 ‘Carrington’ event is one of the largest storms ever documented for which geomagnetic indices have been estimated.”

P3 L51 The Kp index for this storm has been better defined in the recent studies along with the Hpo indices (DOI: 10.22541/essoar.171838396.68563140/v2).

Thank you for pointing to this interesting preprint. Since, to our knowledge, it has not yet been accepted for publication at the time we are resubmitting our paper, and given that we already cite Spogli et al. (2024) to justify our statement that Kp was estimated to have reached 9 during the Gannon Storm, we feel that referring to this paper would not bring any new insight.

P3 L53 the authors need to cite representative references for the AI-based forecasts of the geomagnetic conditions of the superstorms.

Thank you for pointing this out. We have marginally rephrased the statement and added a reference to Oliveira et al. (2021) to justify the statement that AI tools are not performing well

when being presented with inputs that are not representative of the conditions present in their training data set (l. 63–64).

P6 L150 supplementing optical observations from scientific instruments which are relatively sparse at mid-latitudes => supplementing optical observations from scientific instruments which mainly concentrate in the polar regions

Thank you for this suggestion, we have adopted a similar phrasing in the revision (l. 169).

P6 L150 We need to think twice about this statement. Midlatitude auroral activity has been efficiently monitored with networks such as the Super Dual Auroral Radar Network (DOI: 10.1186/s40645-019-0270-5). The authors should appreciate such networks too.

We certainly appreciate the SuperDARN network! However, the statement is here quite unambiguously referring to optical instruments: “supplementing optical observations from scientific instruments which are relatively sparse at mid-latitudes.”

P7 L154 Similar online survey has been taken in the previous studies too (DOI: 10.48550/arXiv.2407.07665). Such existing attempts should be mentioned.

As pointed out in earlier replies, Hayakawa et al. is a preprint which (to our current knowledge) has not been accepted for publication yet. We however added a mention to it in the revision (l. 176–178), hoping that by the time of publication of our paper it will have a final-version DOI.

P8 L189 Can the authors clearly state that almost all the contributors reported their timestamps in their local standard time? I would not be surprised even if some of them have reported their timestamps in the Universal Time

We have added the number of contributors (109) who reported their observations in UTC (l. 184–185).

P9 L200 A total of 688 citizen scientists (out of the 696 respondents) indicated that they saw the aurora => A total of 688 citizen scientists (out of the 696 respondents) indicated that they photographed the aurora; If the authors mean that these citizen scientists managed to see the aurora by their own eye without instruments, the authors should maintain their original statement.

This number includes the reports from citizen scientists who saw and/or photographed the aurora; we have clarified this in the revision (l. 222).

P9 L208-L217 The authors need to clarify that these issues have been pointed in the previous studies (DOI: 10.48550/arXiv.2407.07665).

Same as above: we would happily refer to Hayakawa et al. here but cannot consider it a peer-reviewed publication at this point in time.

P9 L220 The authors need to cite these news pieces with appropriate references rather than just their URLs. For the auroral visibility in Korea and Japan, the authors should cite DOI: 10.5140/JASS.2024.41.3.171 and DOI: 10.48550/arXiv.2407.07665.

Thank you for providing us with the reference to the recently published Kwak et al. (2024) paper about observations of aurora in Korea. We have replaced the news website URL with this peer-reviewed publication (l. 242). Since there are at this time no peer-reviewed

publications available with observations from Japan and China (that we are aware of), we have left the news website URLs for these two cases.

P11 L253 The "sighting" should be replaced to "photographing" or "imaging", unless otherwise this is a naked-eye observation.

We have replaced it with “observation”, which encompasses both naked-eye and imaging (l. 274).