First Energetic Neutral Atom images of the Earth's magnetosphere from the JENI detector on board the JUICE mission during the Lunar and Earth Gravity Assist

Replies to Reviewers

We would like to sincerely thank both Reviewers for their thorough inspection of our manuscript. Their comments helped us to address all possible issues before potential publication.

REVIEWER #1

This manuscript reports a nice ENA observation of the Juice/PEP/JENI instrument during the Earth gravity assist. The observations show a clear decreasing intensity while getting farer to the Earth, furthermore the evolution of ENA observations shows some repeated intensifications possible signature of non-storm substorm activities. The main value of the manuscript is the demonstration of good performances of JENI sensor; however, the observations are interesting and the interpretation seems correct, nevertheless there are number of unclear points in the data analysis and in the figures. For this reason, I suggest a moderate revision of the manuscript. I think that, after considering my suggestions the paper deserves publication.

We sincerely thank the Reviewer for their thorough and thoughtful comments. Bellow we provide detailed descriptions of the changes we made to the revised version of our manuscript in response to those comments.

Detailed comments:

Title: I don't think that "first" is correct. Are other observations expected? Anyway, is this a relevant point for the article? Please, delete the word.

We meant that these are the first images of Earth from the JENI imager. However, we can see that this could be the source of confusion. So, we suggest to change our title to *"Energetic Neutral Atom images of the Earth's magnetosphere from the JENI detector on board the JUICE mission during the Lunar and Earth Gravity Assist*".

Line 40: I would write " and of magnetospheres of other planets", in fact, the references refer to many planets from Mercury, Venus, Mars, Jupiter and Saturn, not only outer planets. Then the authors refer to exosphere without any explanation. I suggest expanding or delete that.

We have followed the Reviewer's suggestion. The relevant sentence now reads: *Energetic neutral atom (ENA) imaging has emerged over the years as a powerful means to diagnose the global, underlying ion distributions of the terrestrial magnetosphere (e.g. Brandt et al. 2021; 2022), the magnetospheres of other planets, together with the extend and interactions of moons exospheres (e.g. Krimigis et al. 2002, Mauk et al., 2003, 2004; Mitchell et al., 2004, Barabash et al., 2006; 2007, Orsini et al., 2010, Brandt et al., 2008; 2010; 2012, Dialynas et al. 2013).*

Figure 1, 2 and 4, the lines in the images are almost invisible in the printed version. So it is hard/impossible to understand the spatial reference. Which is the MLT indication?

We understand the Reviewer's consideration, and we have made the lines more visible. As we note in the caption of Figure 1: [...] *The Earth is shown in the middle of these images, whereas its dipole L-shell lines at L=4 and L=8 at local times 00:00 (top of the image), 06:00 (left of the image), 12:00 (bottom of the image), and 18:00 (right of the image) are also included for reference. [...].*

Figure 1: the blue line in black background is not recommended. It would be useful to show in the plot on the left where the image on the right has been taken. What is “1.1.2 Equations” at the end of the caption?

We changed the color of the line to pink and we also indicated the places where the images on the right are taken. The “1.1.2 Equations” at the end of the caption is a remnant from the ANGEO template. It is now deleted.

Figure 2: labels of left plot are too small. Plots on the left are important for the paper. They should be enlarged. In the images on the right pre and during the events, the timing of the observations are required. The caption refers to a smoothing of the images: is it really needed? I think that the real observations must be shown.

Done.

Line 137: please, define also SMU.

Done.

Lines 140-142: IMF data are not shown in the manuscript. It would be useful to see them or at least the authors should include a reference to them.

We now provide a reference to Brandt et al. (2026), who provides more details regarding the IMF and we also provide a reference to the WDC for Geomagnetism, Kyoto Dst index service (https://wdc.kugi.kyoto-u.ac.jp/dstdir/), that provides interactive plots of the Dst index, which would be the indicator of a geomagnetic storm.

Line 149 and 153: acronym should be defined at first reference. Also add a reference for the LAE definition.

LAE is now defined at first occurrence. Please note that the physics of LAEs are thoroughly described in the references we include, i.e. Roelof (1997), Brandt et al. (2001), Goldstein et al. (2018).

149-159: excluding LAE would require excluding high latitudes signal close to the planet. I am not sure that the performed analysis goes in that direction. In the figure 3 caption the authors refer to the exclusion of the pixels close to Earth, but it is not clear which pixels are excluded. Some more specification on this analysis is required. Furthermore, smoothing the spectrogram “to enhance visibility” is something that should be avoided.

The Reviewer is right. As we also explained initially in our manuscript our analysis is not able to resolve all possible effects that may point to a fraction of ions being injected from the plasma sheet forming the ring current and charge-exchanging to be lost as LAEs. We further noted that such questions can be addressed via forward models or other inversion techniques. Such a task goes beyond the scope of the subject manuscript, which is essentially to show the first images that were captured from the JENI instrument, and would be performed in a future analysis. We now added to the caption of Figure 3 that we excluded all pixels that were bellow the L=4 line. Finally, we have now included contour levels in Figure 3.

I agree with the authors that the different timing of the night and day ENA associated to E1 seems to confirm a process stating from the night side (higher intensity and earlier decrease) and circulating and decaying toward the dayside. It is compatible with stretching and dipolarization in the plasma sheet, and ion circulation after injection. Is it the same for E2 and E3? The authors refer also to Oxygen while the images are not showing any mass discrimination. Most of the caption of Figure 3 should be moved in the main text. Generally, a more detailed description is needed.

This is an excellent suggestion that can be thoroughly discussed in a future analysis that would perform a forward model using these ENA images and also perhaps add a full scale ion model. The present study is primarily focused on demonstrating the ENA images and capabilities, pointing out the consistency with those events we identified. To make this clear, we now included a sentence in lines saying that a more thorough and targeted analysis should be performed in the future.

The reviewer is right that we refer to O^+, and the reason was to fully explain the effects of a geomagnetic storm, by pointing to the relevant references. However, we can see why this can be confusing and for that reason we have deleted the relevant sentence (together with the corresponding references).

Finally, some of the information from the caption of Figure 3 are now moved to the text, per the Reviewer's suggestion.

Figure 4 and discussion: the dayside ENA emission seems to contradict the spectrogram in figure 3. Here the dayside ENA remain almost constantly at low intensity. The colorbar labels are too small.

We understand the Reviewer's consideration. As Reviewer #2 correctly pointed out, Figure 4 showed high angular resolution ENA intensities, over a very broad range (15.1-85.5 keV), and also over a broad accumulation time (82.5 min). We have now included contour levels in Figure 3 and it is now clearly shown that most of the variability on the dayside (bottom panel) occurs bellow 15 keV, whereas the 15.1-85.5 keV energies are less variable (on average). Especially beyond about 25 keV, the small scale fluctuations around E1 are comparable to the slight drop in the intensities. This is consistent with what is shown in Figure 4. Furthermore, as we noted in the text, the dayside ENA emissions do not show the same variability as the nightside, but remain *nearly constant* throughout the E1 event. We should have noted that there is a small decrease in ENA intensities.

Lines 195-204 and figure 4 right: the low intensity of high-energy ENA during the E1 seems to indicate an opening of ion trajectories at these energies, as it happens during the main phase of a storm, when ring current particles, having open patterns, can populate only partial ring current in the night / dusk side. Even if there is no evidence of storm signatures during the period, the ENA spectra seem to point to the development of a partial ring current. Regarding the low energy ENA, another point to be noted here is that the ions increase the energy while approaching lower L-shells, so it could be possible that the ENA spectra of the whole night side (or dayside) mix outer and inner magnetospheric ions. The decrease of post event ENA at low energies could be due to the movement of ions from outer to inner regions.

The Reviewer is correct and we initially considered adding a sentence along the lines of the this comment. However, we concluded that without a more detailed model that would take into account exospheric neutrals and injections from outer to inner magnetosphere, we would not be in the position to support this argument.

Line 217: it should be Figure 2 not Figure 1

Done.

Lines 215- 218 "although the 2.86-6.5 keV ENA emissions increase after each nightside injection episode, they do so with a slight time lag": This sentence contradicts the spectra shown in figure 4. While in figure 4 the intensity at 3-6 keV at 15 UT is lower than the previous ones, in figure 2 the black curve shows a gradual increase (with respect to the expected trend) after E1. The time delay of the increase of low energies ENA seems visible in the plot of figure 2 only for E1. It is hard to see it for other events. This point needs some more analysis and justification.

The Reviewer is right. We agree that this sentence is misdealing and we have removed it from the text.

Lines 218-220 "between E5 and E6 we see ENA enhancements which may correspond to a very small change in the SML index": this point has not been discussed in the previous section. Some more analysis is needed. In fact, the spatial resolution is not enough at those distances to a detailed analysis, but a general increase of the integral ENA emission (exceeding the expectation after the R^-2 law) is clear and requires some discussion.

We understand the Reviewer's consideration, but we would like to make clear that our analysis is not able to address this question, as it goes beyond the scope of the current manuscript. This is why, as far as this time period is concerned, we call it an open question and we note that this could mean that the *[…] ENA variability at Earth is complex and may not be uniquely attributed to those nightside injections*.

Part of the reasons that we cannot provide a discussion about that is because our analysis is not able to address whether the SML index variability between E5 and E6 can be attributed to substorm events or not.

Line 248: As mentioned above, the Oxygen ENA are not shown in the manuscript.
We have now omitted the part of the sentence that mentioned O+.

REVIEWER #2

This paper reports hydrogen ENA observations of the Earth's magnetosphere by JUICE/JENI during the Lunar–Earth Gravity Assist. The dataset is valuable, and the observational opportunity is unique: JENI obtained continuous 7.5-minute ENA images while JUICE moved outward from approximately 17 to 150 RE. The observations are potentially useful both as a demonstration of JENI performance and as a case study of ENA variability during non-storm-time substorm activity.

However, in its present form, I do not think that the main physical interpretation is demonstrated quantitatively enough. The manuscript argues that the observed ENA enhancements are direct responses to substorm-related energetic ion activity and that the morphology is consistent with repeated nightside injections. These interpretations are plausible, but the analysis remains largely descriptive. In particular, the effects of viewing geometry, low-altitude emissions, background/foreground contamination, and energy-dependent transport require further clarification.

I therefore recommend major revision. This recommendation reflects the need for a more quantitative and reproducible analysis, rather than a lack of interest in the dataset. I believe the paper could become publishable after substantial improvement of the analysis, figures, and discussion.

We thank the reviewer for the thorough inspection of our manuscript and we very much appreciate the detailed comments they provided.

We think that there may be a misunderstanding regarding the scope of the current manuscript and perhaps we may have accidentally mislead the Reviewer: The scope of the current manuscript is to demonstrate some of the capabilities of the JENI instrument on the JUICE spacecraft. As most of the instruments on JUICE, JENI was turned on during the LEGA, only to provide some first images of Earth to determine whether the instrument is working properly. As Reviewer #1 noted *"The main value of the manuscript is the demonstration of good performances of JENI sensor"*. This is why the subject manuscript is included in ANGEO's special issue that would host all "instrument manuscripts" during the LEGA.

Our manuscript shows that despite the fact that JENI imaged Earth in a time period where there was no intense "global geomagnetic activity", the instrument was still capable of imaging ENA structures over several R_E away from Earth, revealing that the ENA variability may be associated to "non-storm" time injection events, which are identified by the use of the SML-index. The main point is that these images give confidence that the JENI observations will also allow us to identify and disentangle variability components also in a complex system like Jupiter (which is where JUICE will operate in full capacity).

The Reviewer provided wonderful ideas, which -unfortunately- go beyond the scope of the current manuscript. We -nevertheless- tried to make specific changes to the revised version of our manuscript toward addressing most of these comments. We hope our explanations are adequate.

1. The link between SML dips and ENA enhancements needs to be demonstrated quantitatively. What time delay is expected between the SML onset/minimum and the ENA response, considering ENA travel time, ion injection/drift timescales, and the 7.5-minute accumulation time? The authors should clarify whether the observed timing is physically consistent with substorm-related injections.

We appreciate the Reviewer's consideration, but we would like to kindly note that we do not aim to provide a model that would explain the details regarding the correlation between the ENA enhancements and the SML decreases, i.e. taking into account ion injection/drift timescales and we do not aim to provide a quantitative analysis, as this goes far beyond the scope of the present manuscript.

In Figure 2 we identify an undeniable connection between the SML index and the ENA enhancements, i.e. all identified peaks in SML are associated with the onsets of ENA enhancements. This is as far as our current manuscript goes.

Certainly, the travel time of keV ENAs from Earth to the spacecraft is really small and should not be considered as a limiting factor. For example, a ~10 keV ENA travelling with 1400 km/s requires ~2.6 min to reach the spacecraft at ~35 R_E. A 45 keV ENA travelling with ~2,800 km/s requires ~1.3 min. As the spacecraft moves outward, this travel time increases, but it is still comparable to the ~7.5-min accumulation time. For example at ~100 R_E the travel time of 10 keV ENAs is ~7.6 min whereas 45 keV ions require ~3.8 min. We now include this note in our manuscript.

In fact, to avoid any unnecessarily confusion around our results we now replace all mentions of the term "*are a response*" (regarding the ENA countrates vs SML index) to "*most likely associated*". For example, instead of writing that *"we conclude that these increased ENA emissions are a direct response to the increased energetic ion activity during those substorm events"* we now write *"we conclude that these increased ENA emissions are most likely associated to the increased energetic ion activity during those substorm events*".

2. The manuscript identifies eight ENA enhancements, E1–E8, that "roughly coincide" with minima in the SML index. However, the paper then concludes that these are direct responses to substorm-related energetic ion activity. This conclusion seems too strong without a more quantitative analysis.

We do not disagree with the Reviewer. Surely, future analyses should provide a model that would address this process, but we would like to highlight that we do in-fact show sufficient evidence to draw this conclusion.

In brief, the SML index and the ENA counts vs R plots show good correlation. Furthermore, the ENA images during the events show a clear intensification of ENAs on the nightside magnetosphere. Furthermore, given that there were no indications of any global geomagnetic storms, these ENA intensifications cannot be associated/related to any global storm process, but to non-storm time substorm injections. This conclusion is fully supported by past ENA observations, showing similar structures as the ones we show here at similar ENA energies (e.g. Brandt et al. 2021).

Nevertheless, to follow the Reviewer's comment we now include a less conclusive statement, clarifying that a more precise analysis needs to be performed in the future: *[...] these emissions seem to be associated to the increased energetic ion activity [...]. A future, rigorous analysis that would take into consideration the physics of injections is required to prove causation.*

3. The authors should provide an objective definition of E1–E8, timing of ENA enhancement onset, peak, and decay relative to SML onset/minimum, uncertainty estimates on ENA counts or fluxes, residuals after removal of the expected distance trend, a statistical measure of the ENA–SML relationship.

Although we are not sure what an "objective definition" means, we would like to note that E1-E8 are defined in this study as the times where the SML index is minimum (where "E" stands for "Event"). We refer the Reviewer to our reply at their comment #1 above.

To follow the Reviewer's comment we now provide the "ENA counts vs distance" figure after removing the expected ($1/R^2$) distance trend and our conclusion (as expected) remains the same. This is also done in past studies (e.g. Mitchell et al. 2004). Finally, we kindly ask the Reviewer to appreciate that the measured (statistical) uncertainties for the ENA counts were already included in the figure (and they still do).

4. The total ENA counts decrease approximately as spacecraft distance increases, and the authors compare this with R^{-2} reference lines. However, JENI is observing an extended and structured source, not a point source. The spacecraft distance, boresight direction, angular resolution, source filling factor, and selected pixels all evolve during the flyby.

The Reviewer is correct: we note that the overall ENA signal strength falls off as the spacecraft moves away from the ENA source roughly following a R^{-2} line. This is shown in Figure 2 and it is also shown in the new figure when removing the R^{-2} trend (please see above). It is also true that Earth is not a point source, and we do not assume Earth to be a point source.

We kindly ask the Reviewer to appreciate that comparing the ENA emissions as a function of distance is not an uncommon practice. To support this point we had provided the Mitchell et al. (2004) reference where the authors showed that the measured ENA emission decreased as a function of radial distance from Jupiter following <u>roughly</u> an $1/R^2$ trend. We now also include the Paranicas et al. (2005) analysis doing the same at Saturn. Neither Jupiter nor Saturn are point sources, while both include intense ENA activity within their magnetospheres and both studies find a rough $1/R^2$ dependence of ENAs as a function of distance. The Reviewer is correct that there are variations from the $1/R^2$ line in all cases and this is true in our case as well. We now include more details regarding the ENA variability.

5. The authors state that low-altitude emissions can substantially dominate ENA emissions relative to the plasma sheet and that plasma-sheet/ring-current studies require excluding LAE-contaminated pixels. However, the procedure used to exclude LAEs is not described in enough detail. Please elaborate more.

The Reviewer is correct and this is also a request from Reviewer #1. We now added to the caption of Figure 3 that we excluded all pixels that were bellow the L=4 line.

6. The paper argues that nightside ENA emissions broaden and intensify during substorm events, whereas dayside emissions do not show comparable variability. This is potentially interesting, but the interpretation seems not sufficiently developed. If the authors want to argue for nightside injections and subsequent drift/circulation, then corresponding energy channels for enhancement should be discussed if they are consistent with previous observations. This might help interpreting this event either plasma-sheet injection, partial ring-current development, and LAE contributions.

The Reviewer is correct and this is the reason why we initially noted in our manuscript that *"[...] These characteristics are very consistent to other "non-storm" time injection events identified previously by the IMAGE/HENA at Earth (e.g. Brandt P. C. 2021). [...]"*. Surely, the energy channels of IMAGE/HENA and JUICE/JENI instruments are not exactly the same, but they measure ENAs in consistent energy ranges and show the same overall structures, which in both cases are interpreted as "non-storm" time injection events.

7. The manuscript refers several times to oxygen ENA behavior during storm-time conditions, but the presented analysis is based on hydrogen ENAs. The summary also mentions associated O^+ enhancements in the ring current region. Since oxygen ENA measurements are not shown, the discussion should be rewritten?

The Reviewer is correct, but we kindly ask the Reviewer to appreciate that the discussion section does not refer to Oxygen and we, therefore, did not follow their comment to re-write the discussion.

We refer to Oxygen ENAs (or ions) three times in our manuscript:

- <u>Introduction Section:</u> *"[...] with a very sudden intensification of oxygen ENAs related to non-adiabatic acceleration of ions during rapid magnetic field reconfigurations (e.g. Mitchell et al. 2003). [...] Injection events that are not related to geomagnetic storms show weak, or no* *intensification of oxygen ENAs due to the lack of significant amounts of singly ionized oxygen in the plasma sheet (e.g. Nose et al. 2010)."*

- Section 3: We aimed to fully explain the effects of a geomagnetic storm and this is why we included the relevant references: *[...] to a global process, e.g. a global geomagnetic storm, where the Hydrogen ENA flux would be enhanced nearly symmetrically around Earth, with associated O^+ enhancements in the ring current region (e.g. Daglis, 1997; Daglis et al., 1999, Mitchell, et al., 2003)*
- Summary: Again we mentioned Oxygen only for completeness, to describe what happens in a global geomagentic storm: *[...] such as a global geomagnetic storm, where the Hydrogen ENA intensity would be enhanced nearly symmetrically around Earth, with associated O^+ enhancements in the ring current region.*

We think that the mention of O^+ in the introduction Section is only providing more accurate information regarding the injections (properly referenced) and, thus, it is not likely to confuse the readers. We ask the Reviewer to allow us to keep this sentence.

All mentions of Oxygen in Section 3 and the "Summary" are now omitted.

8. Please describe more explicitly how background and possible foreground ion contamination were treated, especially given the deflection-plate voltage used during this period. The authors state that selected pixels away from Earth were used to avoid LAEs, but the selected pixel regions and exclusion criteria are not shown. Since the main interpretation relies on distinguishing nightside plasma-sheet/ring-current ENAs from LAEs, the authors should provide a more explicit description of the LAE removal procedure?

We understand the Reviewer's consideration.

Foreground ion rejection

We would like to highlight that the deflection plate voltage is in fact used to reject ions from entering the instrument, thus ensuring that the particle entering the detector are ENAs. As we note in the manuscript (Section 2): *"[...] the thin conductive plates were held at a relatively low voltage about 1 kV deflecting charged particles up to about 20 keV/q"*. We further added to the manuscript that *"JENI does not have ion contamination beyond about 20 keV, because of quiet solar wind conditions"*.

Background ENA sources

We assume that by the term "background" the Reviewer asks about possible ENA background sources while imaging. We need to note that the background is fairly steady and we know that it is well bellow the ENA foreground we measure from Earth. Further, as shown in Figure 1 (and also noted in the paper), JUICE was imaging Earth from outside of Earth's magnetosphere. As there are no ENA sources around Earth that could be considered as a potential background (e.g. Jupiter or Saturn are intense ENA sources, but are very far from JUICE during the LEGA), the only remaining "background" source of ENAs is the outer heliosphere. Admittedly, the ENA emission at keV energies is optically thin (that is, ENA intensity cannot be significantly reduced between the termination shock and Earth (e.g. Dialynas et al. 2017-DOI: 10.1038/s41550-017-0115), which means that ENAs created via charge-exchange interactions in the heliosheath will eventually propagate to 1 AU. It is shown through several studies that the ENAs from the heliosheath at the keV energy range respond to the solar cycle phases within ~3 years or more (e.g. Dialynas et al. 2017; McComas et al. 2018-https://doi.org/10.3847/2041-8213/aab611). The ENA images of Earth from JENI were collected during the summer of 2024, which (given the above time-delay) corresponds to the minimum of solar cycle 25. The ENA intensities at (propagated) solar minimum from the heliosheath at ~8.4 keV is ~2-3 particles/(cm^2 s sr keV), whereas the ENA intensities at ~9.1 keV during LEGA is ~155 particles/(cm^2 s sr keV), i.e. more than a factor of 77 higher. Similar conclusions apply to higher energies as well. For example, ENA intensities at (propagated) solar minimum from the heliosheath at ~18.00 keV is ~0.1 particles/(cm^2 s sr keV), whereas the ENA intensities at ~9.1 keV during LEGA is ~15-24 particles/(cm^2 s sr keV). We, therefore, conclude that the ENA emissions from the heliosphere are entirely insignificant and not worthy to mention. We kindly ask the reviewer to allow us not to include this discussion in our manuscript, because it will only detract from the main point of the this manuscript, which is solely to demonstrate the good performances of JENI sensor and that the derived images provide confidence that JENI would operate properly at Jupiter.

Excluding LAEs: The Reviewer is right and this was also asked by Reviewer #1 and also by the same Reviewer in their point #5 above. We now added to the caption of Figure 3 that we excluded all pixels that were bellow the L=4 line.

I believe that the figures require substantial improvement. The figures are currently not adequate for a paper whose main evidence is image-based ENA morphology.

We thank the reviewer for their suggestions to improve our Figures.

Figure 1 should be remade with clearer labels, visible reference lines (for example no x-y axis for middle and right figures, Earth and magnetic field lines are difficult to see), and marked spacecraft positions corresponding to the example images. The caption also contains an apparent error text, "1.1.2 Equations," which must be removed.
Done.

Figure 2 is central to the manuscript but is too difficult to read all the numbers and small legends on the images. The timing of the "pre" and "during" images should be explicitly given (or if there is, then make it larger please).
Done.

Figure 3 should show the pixel-selection regions used for dayside and nightside averaging. Smoothing may be useful for visualization, but the quantitative conclusions should be based on unsmoothed data. I suggest that the authors either show the unsmoothed version in the main text or supplementary material, or explicitly demonstrate that the smoothing does not change the identified enhancements.
Done.

Figure 4: The use of 82.5-minute accumulation images should also be justified since this is long compared with substorm timescales?
Done. This was done toward eliminating any possible small scale variations before during or after the event. We have now updated Figure 4 with 37.5-minute accumulation time images, which are comparable to the time scales of the non-storm time substorm injections (e.g. Partamies et al. 2013-doi:10.5194/angeo-31-349-2013). The new images are -as expected- nearly identical to the previous ones.

The description of the dayside emission in Figure 4 should be reconciled with the spectrogram shown in Figure 3. In Figure 4 the dayside ENA emission appears nearly constant and weak, whereas Figure 3 suggests some energy-dependent temporal structure. It would be better if the authors could clarify whether this difference results from different energy ranges, different accumulation times, different pixel selections, or smoothing.
The Reviewer is correct. This is due to the fact that Figure 4 includes high spatial resolution data over a very broad energy range (15.1-85.5 keV), accumulated over 82.5 min, but the ENA fluxes in Figure 3 include high energy resolution data over 7.5 min accumulation time.

Minor comments

- **The title should not emphasize "First" unless the authors explain why this is scientifically relevant.** Done.
- **SMU should be defined when first introduced.** Done.
- **IMF and solar-wind conditions are mentioned but not shown. Either include the relevant data or cite the source and summarize the values.** Done.

- **Acronyms such as LAE should be defined at first use.** Done.
- **The manuscript contains typographical and grammatical errors, please correct them.** We made corrections to the best of our ability.
- **Line 217 should refer to Figure 2, not Figure 1.** Done.
- **The introduction should more clearly define the scientific gap. At present, it reads mostly as a literature list and instrument context section.** As we explained at the beginning, we feel that there may be a misunderstanding regarding our manuscript. We do not try to bridge some scientific gap. We understand that the Reviewer's comments are mostly driven by their perception that we aim to bridge some scientific gap, but this is not what we aim to do. This manuscript is simply a demonstration of the JENI instrument capabilities as it was turned on during the LEGA. So, an introduction that reads mostly as an "instrument context section" is exactly what we aimed for.

NEW FIGURES ACCORDING TO THE COMMENTS FROM REVIEWERS

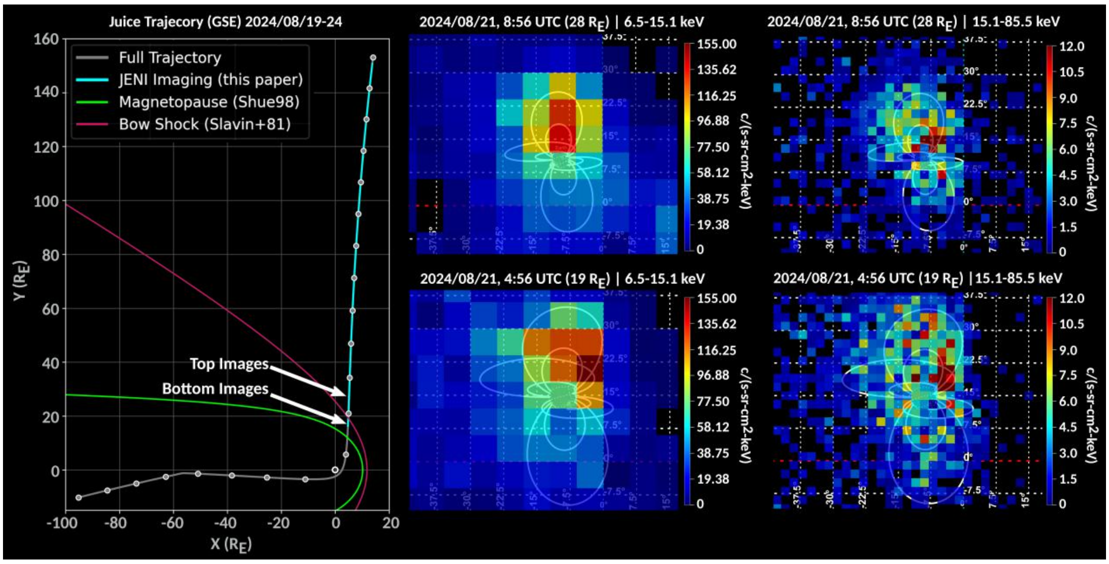

Figure 1

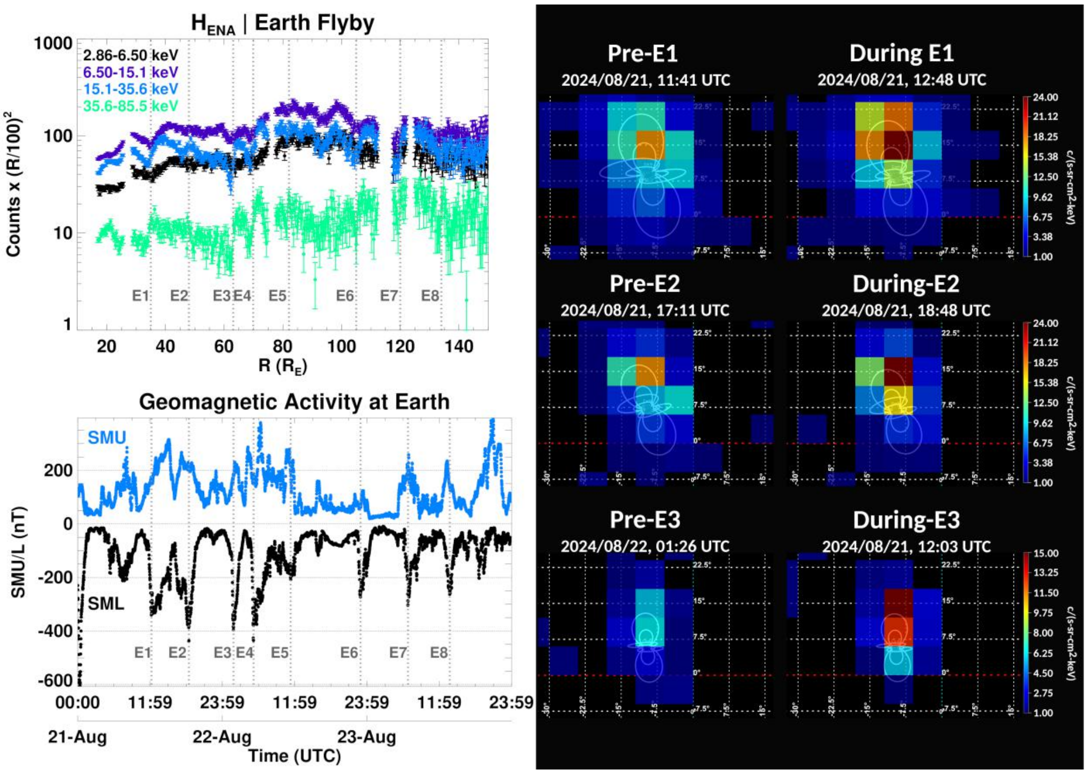

Figure 2

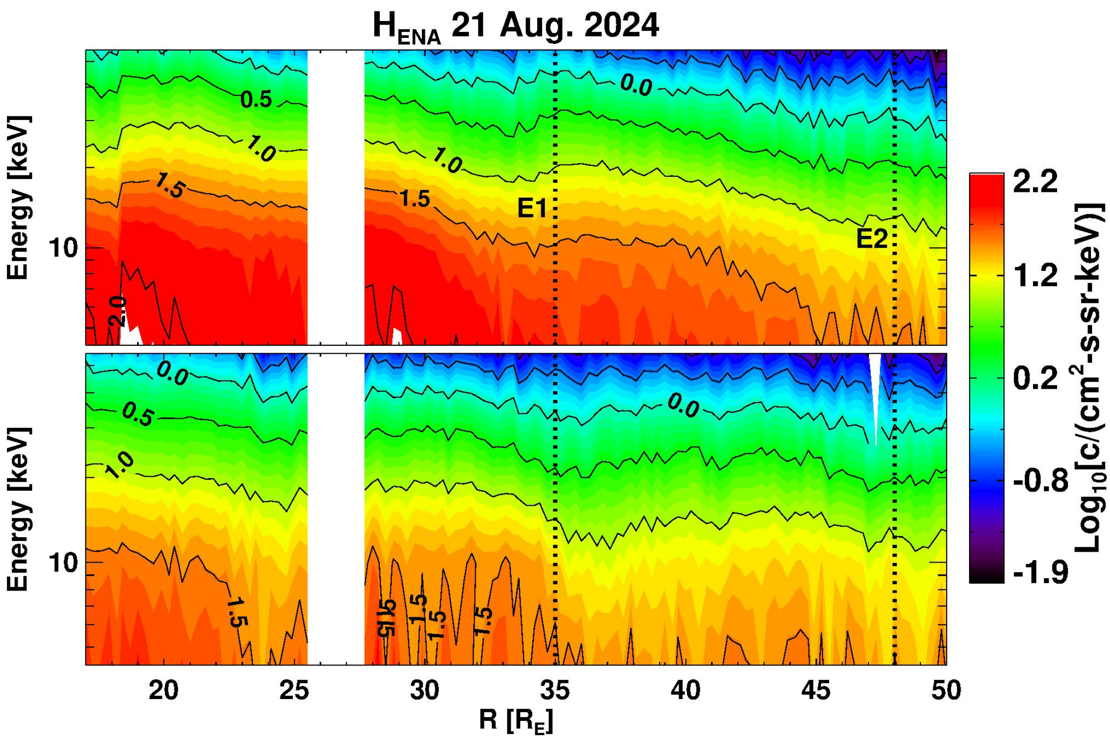

Figure 3

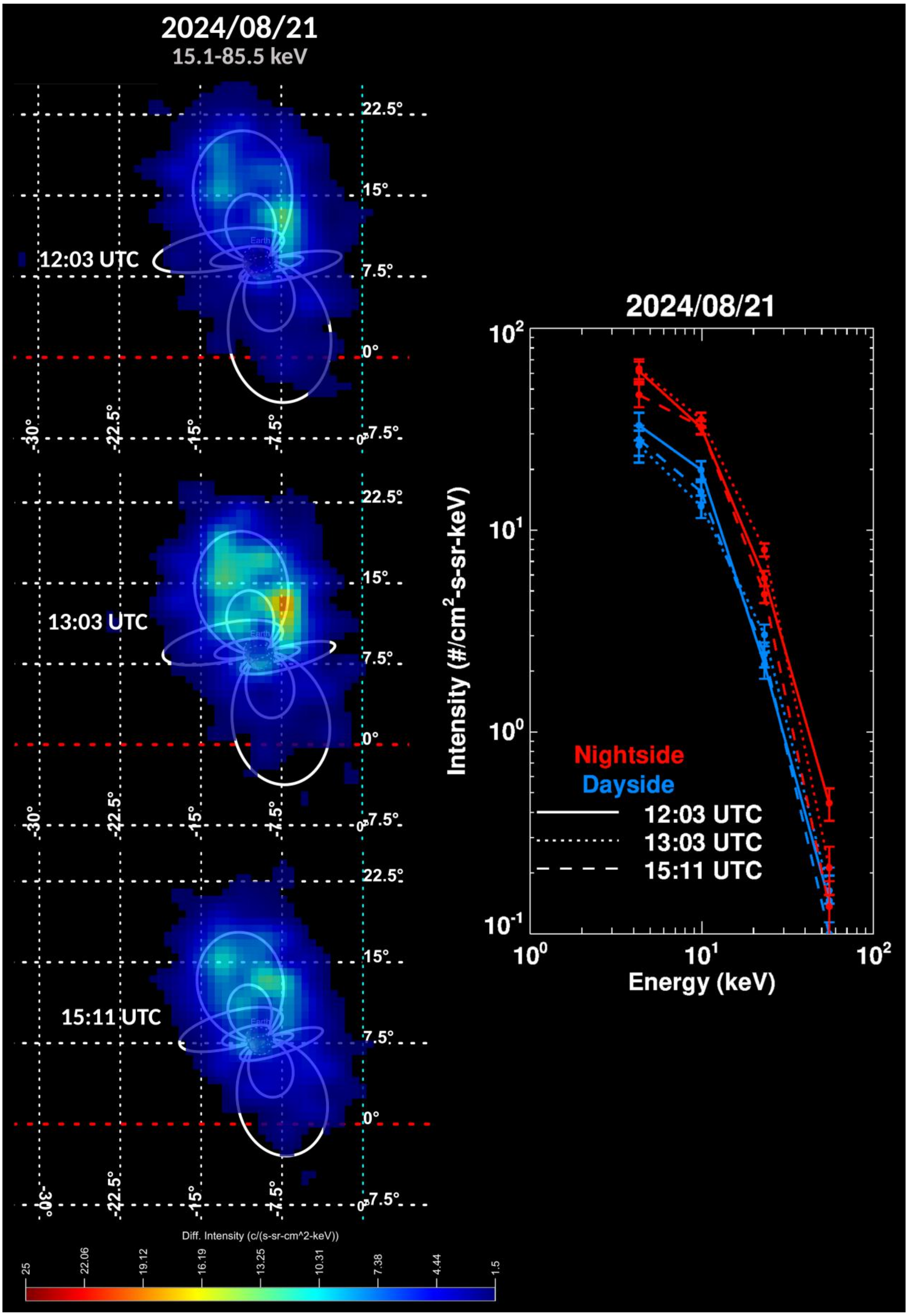
2024/08/21
15.1-85.5 keV
12:03 UTC
13:03 UTC
15:11 UTC
22.5°
15°
7.5°
0°
-7.5°
-30°
-22.5°
-15°
-7.5°
Diff. Intensity (c/(s-sr-cm^2-keV))
25
22.06
19.12
16.19
13.25
10.31
7.38
4.44
1.5
2024/08/21
Intensity (#/cm^2-s-sr-keV)
Energy (keV)
Nightside
Dayside
12:03 UTC
13:03 UTC
15:11 UTC

Figure 4