Response to referee #2

We thank the Referee for his careful reading, and the valuable comments and suggestions he made that helped to improve the quality of the manuscript. We considered each of the comments and questions and provided an adequate answer. The comments of Referee #2 are written in bold and the answers in plain text. Sentences indicating a modification to the manuscript are written using italic font.

The latex diff of the revised manuscript, accounting for comments of referees #1 and #2 is provided as an attached file.

Response to the general comment

Specific questions :

Abstract:

The authors mention the "effects of electric fields" (l. 5). I assume, this study is only about ambient fields and does not include self-consistent electric fields by solving the Poisson equation.

Indeed, the study does not include self-consistent electric fields.

The abstract has been updated to indicate the range of electric field we used to avoid the necessity to have self consistent models.

Also, in line 10, it would already good to mention what kind of "stepping methodology" (line 10) is meant, i.e. of particles.

page 1, line 10 of the revised manuscript: The sentence has been updated to indicate "[...] stepping methodology of particles [...]".

The authors mention that they only tested electric fields until 3 MV/m; however, the electric field for thermal runaway, i.e. for all electrons to run away irrespective of their initial energy, is approx. 26 MV/m, why not consider fields between 3 MV/m and 26 MV/m?

We did do a range of electric fields when investigating the runaway threshold, but we were limited to classical breakdown threshold for the chosen set-up / approximations. To go above classical breakdown, lower energy physics and self consistent electric fields must be considered. And the latter is not possible for Geant4 because of the space oriented, non-synchronous particle trajectories.

The reason why we chose not use E-fields above 3 MV/m *has been has been added or clarified in three parts of paper: page 3, line 27-28; page 4, line 7-8; page 8, line 13-16*

1. Introduction:

1.1: line 4: If it is about observations of high-energy phenomena, I would suggest to cite Fishman et al., 1994 in addition to Williams, 2010.

We agree that citing (*Fishman et al.*, 1994) is relevant here.

page 2, line 9 of the revised manuscript: the suggested citation was added.

1.2: In line 14, the authors talk about the energy regime of HEAP. However, they do not define this energy regime. Please be more precise in defining the energy range.

The energy regime we are considering here is up to about 100 MeV. However we think now that it was over-stating to qualify it "the energy regime of HEAP", since, some researchers in the community may encounter higher energies, e.g. when working on cosmic rays and lightning.

page 4, line 2 of the revised manuscript: "In the energy regime of HEAP, ..." → "In the energy regime of a kilo-electronvolt to a hundred of mega-electronvolts, ...".

Futhermore, the authors write that some individual electrons do not survive. Especially, for high-energy electron beam, it would be good to name the reason for this.

Any electron has a chance to undergo "hard" collisions where a large amount of energy is transferred to the photon in a bremsstrahlung or ionisation interaction. In such interaction, if the energy drops below the runaway threshold (or the energy threshold, as both should ideally bet set to similar values), it is a process that removes a high energy electron from the considered population. We agree with the referee that it can be indicative to give this reason in the article.

page 4 line 16-17 of the revised manuscript: the reason just described here was added.

Also, please specify what values you consider "much larger than the ionisation threshold" (page 3, lines 23-24).

Such a value can be set to above a few keV.

page 4, line 19-20 of the revised manuscript: an indicative value of "a few kilo-electronvolts" is now specified.

On page 4, lines 14-15, the authors write "The minimum energy $\epsilon_2^{\min}$ that can runaway is given by the requirement $F(\epsilon_2^{\min}) > E$ [...]." But should the friction force not be smaller for runaway? Please clarify this.

The friction force is split into two parts. One part is below the electron low energy cutoff and used as a friction. The other part is considered by explicit discrete collisions. This first friction part for electrons above 10 keV is indeed a function of energy and decreases for increasing electron energy, but converges to a constant for electron energies above 1 MeV. So the lower the energy, the higher the friction and the threshold $F(\text{electron energy}) = qE$, lies orders of magnitude higher than ionization energy (i.e. ≈ 10 eV comparing to ≈ 10 keV).

Also, to be more correct, it is $q \times E$ that has the unit of the friction force F.

page 5, line 13 of the revised manuscript: "$F(\epsilon_2^{\min}) > E$" was changed to "$F(\epsilon_2^{\min}) \approx q\,E$"

On page 5, the authors discuss the angular dependency (between the electron motion and electric field direction) on the run-away process. This has already been discussed very extensively by [O. Chanrion et al., 2016. Influence of the angular scattering of electrons on the runaway threshold in air. Plasma Phys. Contr. Fus., vol. 58, 044001]. Please cite this article.

This article was already cited in another section of the non-revised manuscript (p.2, line 28). But we agree that citing it again inside this section discussing the angular effects is a good idea.

page 6, line 22 of the revised manuscript: A second citation to the article (Chanrion et al., 2016) was added.

2. Model descriptions:

2.1: The authors say that different sets of electro-magnetic cross sections are used. However, the authors do not state (neither in the main text nor in the supplementary material) which processes are actually taken into account. This is clearly missing, but crucial since simulation results strongly depend on the chosen processes and cross sections. The authors state where the cross sections come from and they make some comparison plots in section 8 of the supplementary material, but only for a few processes. Say, in the future, other researchers want to compare their results. Then, the knowledge of the used cross section data is crucial to interpret results. I would thus suggest to elaborate more on the processes and cross sections.

We agree with this point: it would be more practical for future researchers that would like to use this work to have an easy-to-read summary of the process and which cross section sets are used by the models we tested.

We added a table providing all this information (i.e. processes cross sections and/or models used by each code) as part of the supplementary material .pdf file (section 13). We also added a reference to this table in the revised manuscript (page 8, lines 4-5).

2.2: On page 8/line 29, the authors say that GRRR uses the "energy at that instant" to calculate the collision rate ν_k . However, it is not clear which energy: the energy of each individual particle, the maximum energy of all particles or the mean energy of all particles. Please clarify.

Thanks for pointing out this ambiguity. Now we state clearly that it is the energy of each particle that is used to calculate collision rates.

page 10, line 9 of the revised manuscript: "the rate ν_k is calculated using the energy at that instant" → "the rate ν_k for each particle is calculated using that particle's energy at that instant".

2.4.1: For space-oriented codes, "a single particle is simulated over its entire life-time" (page 9/line 21). However, what is the reason to lose an electron (especially in the regime above several keV). The only reason to lose an electron would be attachment to air molecules. However, for this process to occur the electron normally needs to lose more energy than down to 10 keV. Please be more precise here.

Our wording "a single particle is simulated over its entire life-time" was not accurate, since the particles are usually simulated until they go below the energy threshold, for most of the codes. Note that Geant4, by default, does simulate all primary particles down to zero energy (the "energy threshold", in Geant4, is a minimum energy for the secondary produced particles to be tracked or not). As shown in the section 3 (RREA probability study), the probability for an initial electron to produce a RREA is negligible below 10 keV. In this energy range, the electrons will slow down in a small length and time scale; and indeed it is only when they go down to energy in the eV scale that they will attach to air molecules and that we can consider that it is "really lost"; and this will happen a little bit later in their lifetime.

Page 1, line 4 of the revised manuscript : "a single particle is simulated over its entire life-time" → "is simulated until it goes below the low energy threshold ϵ_c, chosen by the user". We also repeated the precison that Geant4 follows all primary particles until zero energy (that was also indicated before in the Geant4 description section 2.1).

2.4.2:Why would "acceptable values of $\delta\ell_{max}$ depend on the electric field" (page 10/line 28)? It is very clear that it should be smaller than the electron's mean free path. But the mean free path depends on the electron energy rather than on the electric field. Please clarify the dependence on the electric field.

The gain or loss in energy by a particle due to the E-field depends on the distance it propagated along its direction, and its magnitude. The electron may move from point A to point B, and could do a collision or a null-collision at point B. The result of the collision at point B depends on the (differential) cross sections at this point, that depends on the energy, that depends on the energy it gained (or lost) when traveling from A to B, that depends on the electric field and the distance between the two points , that will be affected by $\delta\ell_{max}$ in some case. For the tracking to be accurate, the cross sections should remain approximately constant between point A and B, that is

only acceptable if the A-B distance is not too high. The mean free path is important, but so is also the distance at which the gain in energy due to the electric field is small enough so that the change in the cross sections between point A and B is small enough. Depending on the context, it could be greater or smaller than the mean free path, and one should always use a step length that is small enough compared to both quantities. In Geant4, that can be achieved by forcing the maximum acceptable step $\delta\ell_{max}$ to be small enough, or, more cleverly, by reducing the α_R parameter. However, an exact quantification of the dependence of the good values of $\delta\ell_{max}$ (or α_R) for different electric fields is not easy to perform, and would require extra simulations, that we did not have time to run.

However we think this explanation should not be included in the main text, since is bit too long and we could not precisely quantify the dependency of $\delta\ell_{max}$ with E. Thankfully, this response to the reviewer will be available in the GMD web-page of this paper.

3. Probability of generating RREA:

In Figure 1, the authors present the probability that a single electron with an initial energy of 75 keV in an ambient field of 0.8 MV/m creates an RREA. As a criterion, they use that at least 20 electrons with an energy of 1 MeV are created. Please specify which value for $\delta\ell_{max}$ is used in panel a) and which value for α_R is used in panel b).

The values are the default ones. That is $\delta\ell_{max} = 1$ km and $\alpha_R = 0.2$ for O4 and $\alpha_R = 0.8$ for O1.

page 14 of the revised manuscript: The caption of figure 1 has been updated to indicate this.

However, I am confused, though. The friction force based on [A.V. Gurevich, 1961. On the theory of runaway electrons. Sov. Phys. JETP-USSR, vol. 12, pp. 904–912] is supposed to be 0.65 MeV/m for a 75 keV electron, thus the ambient field is definitely sufficient to accelerate the electron into the run-away regime. Of course, this does not mean that 20 electrons with energies are above 1 MeV, but 12% seems low.

The RREA probability value for this case is around 10 to 12% and is given by two independent codes (GRRR and Geant4). We think that going from the (average) friction force to this probability is by no means straightforward, as it is a stochastic process, and it requires computer simulations to be evaluated (as far as we known). We encourage other researchers to evaluate this probability independently, and maybe by different methods from Monte-Carlo if possible.

But this might depend on the simulation time. How long has the shower been simulated?

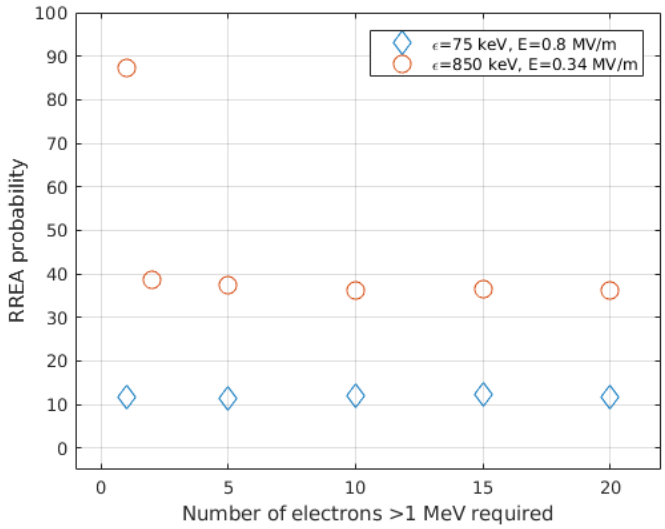
The simulation is running for as much time as needed for one of the two possibilities to happen for each single electron seed:

- [i]: it is absorbed
- [ii]: there is at least 20 electrons of more than 1 MeV in the system.

The simulation space is large enough so that the particles will never encounter the border. In pratice, for this configuration ($\epsilon = 75$ keV and $E = 0.80$ MV/m), the electron time is ≈ 2 ns (times are given from the global / laboratory fame) if [i] is reached and ≈ 230 ns on average if [ii] is reached. For information, it takes ≈ 190 ns on average if 10 electrons of more than 1 MeV are required, ≈ 130 ns for 5, and ≈ 17 ns for 1.

It would also be good to see the RREA percentage for different criteria (20 electrons above 1 MeV, 10 electrons above 1 MeV, 5 electrons and finally 1 electron above 1 MeV which should give almost 100%).

The RREA percentages as function of the number of 1 MeV electrons requested are presented here for two cases: ($\epsilon = 75\,\mathrm{keV}$, $E = 0.8\,\mathrm{MV/m}$) and ($\epsilon = 800\,\mathrm{keV}$, $E = 0.35\,\mathrm{MV/m}$). It was obtained using the Geant4 O4 model with $\alpha_R = 0.001$.



For the ($\epsilon = 75\,\mathrm{keV}$, $E = 0.8\,\mathrm{MV/m}$) case, the probability does not significantly changes if 20, 10, 5 or 1 electron of more than 1 MeV are required. This result is actually consistent with the RREA probabilities we found for higher energy electron seed with the 0.8 MV/m field (see supplementary material section 5.2, or the figure just below here). Once the seed electron has reached 1 MeV, then its probability to generate the 20 MeV electrons in the 0.8 MV/m field is 100 %. Actually, for this values of (ϵ, E), what mainly affects the final probability is what happens at the start of the simulation. i.e. if the single initial electron is able to gain enough energy, or goes below the energy threshold before (that is here set to 990 eV). However, for ($\epsilon = 75\,\mathrm{keV}$, $E = 0.8\,\mathrm{MV/m}$), the "required number of 1 MeV electrons" does play a significant role in the probability. It is coherent with what is indicated in the manuscript: "The difference [between requiring 1 or 20 MeV electrons] is mainly noticeable for low electric field (< 0.4 MV/m) and high seed energies (> 700 keV)." (page X, lines XX)

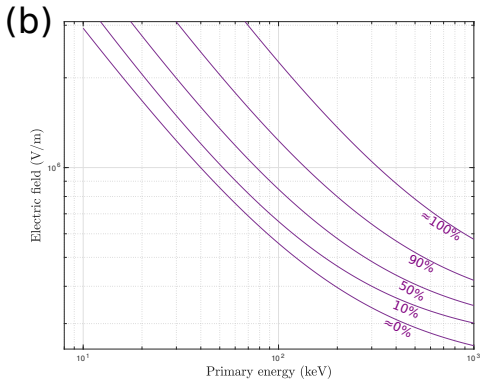
Supplementary material, section 5.3: the previous plot was added as the reader may be interested to know this information.

page 12, line 32-33 of the revised manuscript: a reference to this plot in the supplementary material was added.

In Figure 2, the authors show the avalanche probabilities (10%, 50% and 90%) as a function of initial electron energy and ambient electric field. What about the right top (high energy, high field) and bottom left (low energy, low field) part? What are the probabilities there?

Plots indicating the values for the other probabilities for the four models are presented in the supplementary material (section 5). For high field or high energy, the probability is 100%, For low field and low energy, the probability is 0%.

However, the primary objective of figure 2 was not to present the full probability domain, but to compare the codes for three probabilities in-between the 0% and the 100% case, where we expected to see most of the differences. Furthermore the 0% and 100% contour lines tend to be more noisy than the three other ones, making it more difficult to compare the models. However for the Geant4 O4 model (supposingly the most accurate model), we could build a distribution with very large statistics (>50,000 seeds per parameter set), and we could produce a noiseless plot with the 0%, 10%, 50%, 90% and 100% probabilities contour lines :



There is so much space in this figure. Why not add some curves or values for these two regimes.

As suggested by thre referee, we think it will be helpful for the reader to see the level curves corresponding these two regimes, i.e. the 0% and the 100 % levels. So we integrated this last plot in the manuscript in addition to Figure 2 (→"Figure 2.b."). And the former "Figure 2" is now "Figure 2.a."

However we don't want to add more level curves (e.g. from other codes) to Figure 2.a. because it will make it hard to read.

4. Characterisation of RREA showers:

When discussing the evolution of the self-similar state , the authors say that they used a different number of seed electrons for Geant4, REAM and GRRR (page 16, lines 12–13). For consistency, I suggest to add one more case where the same number of seed electrons is used.

For very large electric field (> 2 MV/m), Geant4 used 200 seed electrons, but GRRR only used 20 and REAM used 16, due to limitations in our computation resources. In addition for low E-fields, only a few of the 200 sampled electrons did trigger a RREA for O1 and REAM (as the RREA probability is low for $\epsilon = 100\,\mathrm{keV},\ E = 0.8\,\mathrm{MV/m}$), meaning also larger error bars for the two models. As we do not have the capability to run GRRR or REAM with larger statistics, the cases with the same number of seed electrons would mean showing Geant4 results with lower statistics. But we think this is not really necessary: it will make the figure less readable, without providing extra information. Actually, the uncertainty intervals for Geant4 simulations with reduced statistics can be easily guessed: they would expand by about a factor of 3.2 ($\approx \sqrt{\frac{200}{20}}$) for 20 seeds and 3.5 ($\approx \sqrt{\frac{200}{16}}$) for 16 seeds.

Figure 5 shows the time to reach the self-similar state. Comparing all the different models, it seems that the time to reach that is consistent within one order of magnitude. I propose to add this to line 14 on page 16.

Indicating that the models are consistent within one order of magnitude is largely over-evaluating the difference, since:

- Without considering the error bars, the relative difference between the models is never more than 200%
- for most of the electric fields, the models are consistent within the statistical error bars; that are a bit large for REAM and GRRR due to the limited amount of seed electrons we could simulate

We think the way it is is already discussed in the non-revised manuscript is good and should not be changed.

4.3.1: In Figure 6, the authors present the mean energy (of the self-similar state) as a function of the ambient field. It might be good to remind the reader in the figure caption which value for α_R was used here for O1 and O4.

The value of α_R is set to the default for O1 and O4, that is 0.8 and 0.2 respectively. In this case the maximum acceptable step of 1 millimeter ensures that we have accurate simulations.

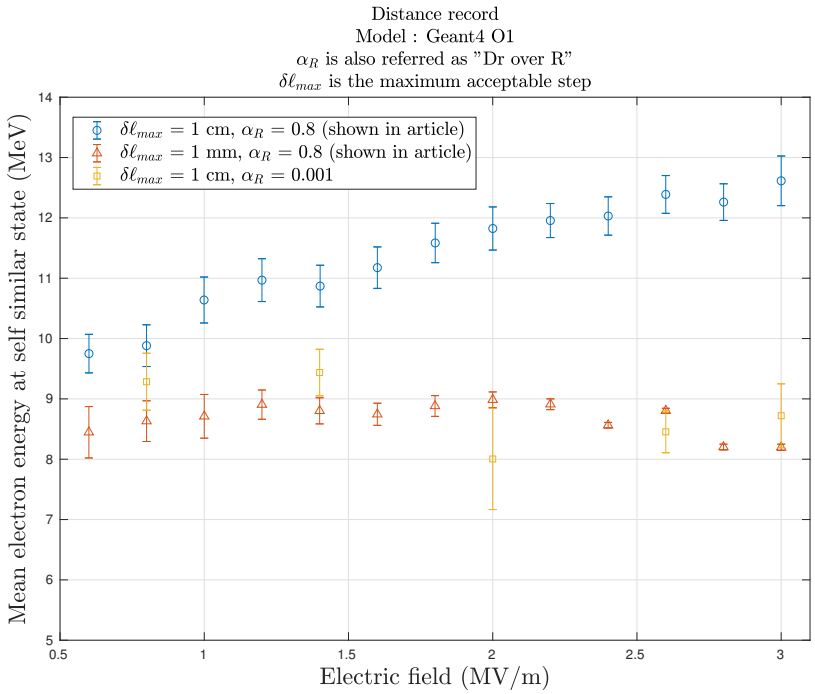
Page 21: the caption of figure 6 has been updated to give the suggested information.

I am wondering how the plots for O1 and O4 would look for α_R. It would be good to plot one case for a different α_R to show the dependence of the mean electron energy as a function of α_R .

The main point of this section (4.3) of the paper is to compare the models in the case were we think they are set-up optimally, so we do not think that adding this α_R comparison in this section is a good idea. In the original (and revised) manuscript, we just show one case where the maximum acceptable step is too large to give the reader an idea of how bad it can be, without discussing it further.

However it is informative to see if reducing α_R makes the mean energies converge towards similar values compared to reducing $\delta\ell_{max}$ to 1 mm. *We added plots of mean energy as function of α_R for O1 ($\delta\ell_{max}$ is set to 1 cm to have a common reference with figure 6), in the supplementary material (section 7), for several electric fields.* Unfortunately, we could not cover the same number of electric fields, and could not simulate the same number of seed (i.e. the new data is more noisy).

Page 21, line 12-13 of the revised manuscript: A reference to the supplementary material pointing to this curve was added. Here is a reproduction of this curve:



As expected, $\delta\ell_{max} = 1$ mm is close to $\delta\ell_{max} = 1$ cm with $\alpha_R = 0.001$

In Table 2, the authors present the fit parameters a_1 , a_2 and a_2 of Eq. (20) for different models. Please add the error bars in order to judge the quality of these fits.

Table 2 (page 22) has been updated to give confidence intervals on the fitted parameters a_1 , a_2 and a_2, and the text was also updated accordingly (page XX, line XX-XX). The best fit values have also been re-evaluated compared to the previous version of the manuscript.

We want to make clear that the confidence intervals given are not error bars. They indicate what is the range of values of the three parameter that are able to give good quality fits, assuming a 95% confidence threshold. The quality of the fits can be evaluated using the r-squared coefficient, and it is larger than 0.90 for all the fits of table 2, meaning that the model (equation 20) fits very well the data, as can be qualitatively observed in Figure 6.

In Figure 7, the authors present the electron spectra at 128 m. But does 128 m refer to the z-coordinate or to the travelled distance $r = (x^2 + y^2 + z^2)^{1/2}$. Please clarify this in the figure caption and in line 5 (page 19), line 2 (page 20) and in the caption of Figure 8.

The distance of 128 meters refers to the Z coordinates (-Z being the direction of the E-field so that electrons are accelerated towards positive Z).

page 24, figure 7, page 25 figure 8, page 22, line 19 and page 23, line 21: the manuscript were updated as requested.

4.3.2: In section 4.3.2, the authors state that a comparison with photon measurements is difficult because of the attenuation of photons in air. Whereas I agree in general, there are some issues I would like to address. The authors say that a 100 keV photon at 12 km altitude travels 1540 m in average, a 50 keV photon 671 m and a 20 keV photon 63 m. Where do these values come from? Is this a result of their simulations (if so, how did you obtain these); if not, please cite your source. I made a brief comparison with NIST data (http://physics.nist.gov/PhysRefData/Xcom/Text/XCOM.html) and obtained attenuation lengths of approx. 2000 m for 100 keV, 1600 m for 50 keV and 500 m for 20 keV. This needs to be clarified.

The given average path lengths come from Geant4 simulations. It is a simple set up where M >10,000 mono-energetic photons are sampled (in air at 12 km density) and their total path length before absorption is recorded, and then the M length values are averaged. The standard deviations around the averages are also calculated and given in the article. If requested, we would be glad share the Geant4 source code.

We agree with the attenuation lengths given by the referee. However, the attenuation length and the average path length are different quantities: the average path length is lower because a photon will not keep the same attenuation length over his full track (it will be reduced at each step) since it is losing energy along its path. These mean path lengths values also have large variations from event to event (stochastic process) that are evaluated with the standard deviations indicated in the manuscript.

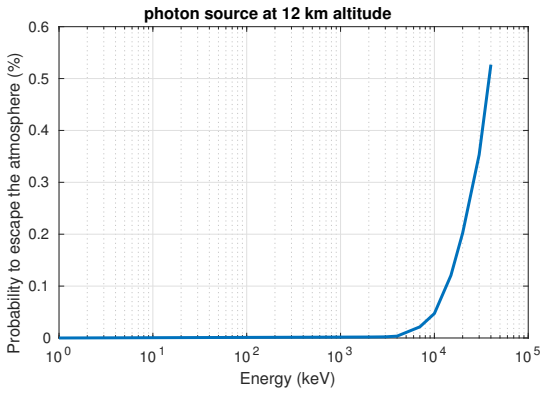
Page 24, line 2-4 of the revised manuscript : to clarify this point, a sentence was added.

Additionally, the authors say that the "photons have no chance to escape the atmosphere and to be detected by a satellite" (pages 20/21). I would like to remind the authors that the "average path travelled" (page 20/line 14) or the attenuation length is only an average. Hence, there can always be photons which escape the atmosphere and may be detected at satellite altitudes even though the probability is low. Actually, Fermi has measured photons with energies between 10 and 500 keV (see https://gammaray.nsstc.nasa.gov/gbm/science/terr_grf.html). Please be more precise here.

It is close to impossible for a photon of less than 500 keV to reach space, if it is emitted from 12 km altitude (and even more impossible for 100 keV photons). The vast majority of photons (if not all of them) observed below 500 keV by the Fermi space telescope (and also RHESSI) are actually photons that, at emission, had larger energies (likely more than 1 MeV) and lost some part of it by collisions with air, and eventually reached the satellite.

Page 24, line 6-11 of the revised manuscript : to clarify this point in the paper, three sentences was added.

For information, here is a plot giving the probability of photons to escape the atmosphere as function of energy, assuming a source at 12 km altitude:



Note that, even for 40 MeV photons, the probability to escape is less than 1%, indicating that if a TGF is produced from this altitude, it must be very strong to be detected in space. The curve was obtained from Geant4 simulations and, if requested, we would be happy to share the source code.

Supplementary material, section 14 : the previous curve was added, as it may be helpful for other researchers.

4.4: In the supplementary material, section 9.2., I cannot find any plot showing the parallel velocity $\beta_{\parallel}$ (only the mean Z speed; or is the mean Z speed meant to be $\beta_{\parallel}$). Maybe, it is there, but at least not apparent. Could the authors please point me to the correct plot?

The mean $\beta_{\parallel}$ and the mean Z speed are indeed the same, since the the electric field is on the Z direction. We only compare mean $\beta_{\parallel}$ values and do not show the full distributions of $\beta_{\parallel}$.

Page 25, line 4 of the revised manuscript : the sentence has been updated to clarify this.

page 24, lines 24/25: The authors say that single Coulomb scattering would "increase the necessary computation time". Should using a single scattering (instead of multiple scattering) not decrease the computation time?

The number of interactions experienced by an electron (or positron) before being stopped increases with its kinetic energy and so a detailed simulation becomes very demanding in computation time at high energies: that's why multiple scattering method were developed. The idea behind the multiple scattering algorithms is to avoid to explicitly simulate every "hard" collision of every single electron (i.e. avoid doing single scatterings), but to do multiple scatterings inside one step length (or one collision). It permits to use step lengths substantially larger (usually >10 times) compared to a pure single scattering strategy, and so reduces the necessary computation time to simulate the propagation of a given electron (or positron), but can be less accurate. Such algorithms usually rely on several tweakable parameters that should be optimized to shorten necessary computation time while keeping an acceptable physical accuracy. Remark also that what is maybe the biggest difference between {REAM-GRRR} and {Geant4 O1-O4} (in the set-ups used in this paper), is that the former use single (Coulomb) scattering algorithms and the later uses multiple scattering.

Page 28, lines 4-7 of the revised manuscript : the point about single scattering has been updated to clarify this.

Technical corrections:

All the suggested technical correction have been applied. We thank the referee for his very careful reading.

Others changes (not suggested by referees)

During the revision process, several extra improvements to the paper were suggested by the authors:

- *Page 16, line 26 of the revised manuscript: A citation to the article "Fundamental parameters of the relativistic runaway electrons avalanche in air" by Babich et al. (2004) was added; as it is an important study to mention in the context of this work.*

- *Page 3, lines 8-13 of the revised manuscript: A sentence was added in the introduction about x-ray emissions observed in laboratory spark experiments, that is also a potential case where the models we are comparing can be applied.*
- *Page 2, line 21 of the revised manuscript: For completeness, we added two citations to the recent gamma-ray glow observations of (Kochkin et al., 2018; Dwyer et al., 2015) in the introduction.*
- *abstract (page 1-2): Improvements in the English, added more details on the Electric field range.*
- *conclusions (page 26-27): Improvements in the English, added more details on the Electric field range, and more details.*
- *The second paragraph of the introduction (page2, line 11-13) was updated to give two more interesting citations about TGF satellite observations (one for RHESSI, one for AGILE and a more recent one for Fermi).*
- *Figure 2: The 10%, 50% and 90% probability contours for the REAM model (red curves) were added, together with a paragraph discussing how it compares with Geant4 (Page 13-14, lines 34-35 and 1-9).*
- *Page 3, lines 29-32 of the revised manuscript: We added a small paragraph clarifying the differences between the different values (between 2.36 and 3.2 MV/m) of the classical breakdown field, that can been seen in the literature.*
- *Page 13, lines 4-5 of the revised manuscript: a sentence was added to justify the use of this particular {E,ϵ} set.*
- *Page 23, lines 1-4 of the revised manuscript:* a sentence indicating why an electric field of $E = 0.80$ MV/m is used for the comparison case was added.
- *Page 13, lines 15-16 of the revised manuscript:* an indication that we provide Geant4 examples source codes with tweakale α_R and $\delta\ell_{max}$ parameters was added.
- Page 12, lines 23-25 of the revised manuscript: We added a sentence about the chosen direction of the initial electrons in the RREA probability simulation and its impact on the probability.

References

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