

Peer Review File

Earth's atmosphere and geomagnetic field protect the biosphere from nearby supernovae



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2nd Jan 24

Dear Dr Christoudias,

Your manuscript titled "Impact of nearby supernovae on Earth's atmosphere and climate" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. In particular, please

- * ensure the provided details on the model experiments are complete and allow for reproducibility;
- * justify the chosen model experiments in more detail and;
- * present and discuss in more detail the chemical mechanisms, different scenarios and the role of CCNs.

If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

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EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

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For your information, you can find some guidance regarding format requirements summarized on the following checklist: (<https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf>) and formatting guide (<https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf>).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Please see attached pdf

Reviewer #2 (Remarks to the Author):

The paper by Christoudias considers a hypothetical scenario of a nearby (~100 pc) supernova focusing on its possible impact on the Earth's atmosphere. The topic is interesting for a detailed investigation but the level of preparedness of this work is insufficient for a high-impact publication. The paper needs to be significantly improved before it can be acceptable. More details are given below.

The paper fails to meet the condition of being reproducible. The details of the model are not properly described (see below), and the composition of the model experiments is not convincing.

1. A list or a Table of the considered scenarios should be clearly presented, including all the used parameters, e.g., geomagnetic field, heliospheric modulation, to the level where any reader can reproduce the input parameters.

2. Many details are not available to the reader.

- a. The assumption of a simple 100x increase in GCR is unjustified and must be explained. The spectrum of GCR from a nearby supernova is not just a 100x enhancement of the present-day spectrum. The latter is an 'equilibrium' spectrum of a large number of sources over a huge volume that cannot be reached for a nearby transient event (supernova). The SN-related spectrum would be first harder and then softer than the 'standard' GCR spectrum. Moreover, the most geoeffective part of the GCR spectrum (a few GeV) is strongly modulated in the heliosphere, thus, the exact heliospheric conditions must be specified.
- b. The model run is too short – one year is too short even for the gamma-ray scenario, because of the longer-lasting atmospheric effects. A longer model run is even more important for the cosmic-ray effect which can be accumulated over years leading to a new equilibrium state and the related effects.
- c. The model based on the CERN CLOUD experiment needs to be fully described. There is a reference to Dunne et al. (2016) but that reference does not provide the full model description. Is it the same as the MESSy NAN model?
- d. The description of chemistry (page 3) is very basic and doesn't need to be here in full.
- e. Fig.2 needs to be explained in more detail. Relative to what is the effect shown?
- f. It needs to be explained how the ionization was computed for the 100x increase.
- g. The statement in lines 244-248 RHS (on the recombination times) needs to be explained.
- h. The statement in lines 256-259 LHS needs to be shown.
- i. Page 8: for what epoch were the IGRF parameters considered, and how exactly was the method by Frazer-Smith applied? A reader must be able to repeat the computation precisely.

3. The considered scenarios contain many simplified assumptions, which is fine but the authors should clearly state that this is not a fully realistic modelling of a nearby SN but a conservative simplistic estimate of the effect.

Other comments:

- Line 42: "increased aerosol particles" – increased in size or number of?
- The sentence in lines 65–69 needs a reference – where has it been postulated?
- Reference (Fields et al., 2019) is to an ArXiv preprint of 2019 which is unacceptable. Has it been published as a peer-review paper? If not, the reference should be replaced or removed.
- Line 137 RHS (right-hand side) column: a reference or an explanation is needed for why it is unlikely.
- Page 4, lines 186-187, RHS column: please provide a reference for the gamma-ray pulse from a SN.
- The roles of GB, ES and MK authors are not specified in section 5.5.
- Line 192, RHS column: please explain the statement of "one billion chest X-rays" since the conversion is not straightforward – I was not able to reproduce this number even approximately. It would be better to remove this statement as not directly related to the focus of the paper.
- Fig. 3 is difficult to read as the contours/numbers seem unrelated to the colour code. Is it correct to interpret that, e.g., the columnar O₃ is reduced to ~60 DU (120 DU x 50%) in May at 80° N? This needs to be better explained.
- Line ~277 RHS: when discussing the cosmic-ray-related dose please specify the location.

Reviewer #3 (Remarks to the Author):

This manuscript presents the authors' study of the impact on Earth's stratospheric ozone layer and climate of a nearby supernovae, which exposed the Earth to an intense cosmic-ray radiation with two orders of magnitude stronger than the present intensity. The authors evaluate the impacts using an Earth system model (ESM) that incorporates some selected atmospheric chemistry and aerosol processes. Their simulated results mainly show that the effects of intense cosmic radiation through (NO, NO₂) and (OH, HO₂) catalytic cycles cause moderate stratospheric ozone depletion, and that the enhancement in aerosol and clouds from the elevated cosmic ray flux leads to a negative radiative forcing comparable in magnitude to current anthropogenic positive climate forcing. They therefore conclude that Earth's atmosphere and geomagnetic field are effective at shielding the biosphere from nearby supernovae. Reading this manuscript, I have two major concerns:

(1) The citing of the relevant references is very selective or even incorrect/misleading, especially on the link between galactic cosmic rays (GCR) and cloud formation and on (NO, NO₂) and (OH, HO₂) catalytic cycles for ozone depletion. The credits to H Svensmark, P Crutzen, H. Johnston, D. Bates and N. Nicolet are not properly given.

(2) The study considers only the GCR-driven (NO, NO₂) and (OH, HO₂) catalytic cycles for ozone depletion but ignores the cosmic-ray driven (Cl, ClO) catalytic cycles. The latter process is well documented in the literature, such as Q.-B. Lu, Fingerprints of the cosmic ray driven mechanism of the ozone hole. *AIP Advances* 11, 115307 (2021); Formulation of the cosmic ray-driven electron-induced reaction mechanism for quantitative understanding of global ozone depletion. *Proc. Natl. Acad. Sci. USA* 120, e2303048120 (2023). If the authors want to exclude the latter process, they must state clearly that their conclusions are drawn only from the results of ozone depletion simulated from the GCR-caused (NO, NO₂) and (OH, HO₂) catalytic cycles but no CR-driven (Cl, ClO) cycles for stratospheric ozone depletion.

Therefore, major revisions are required before a recommendation for publication in *Communications Earth & Environment* can be made.

Review of “Impact of nearby supernovae on Earth’s atmosphere and climate ”

BY THEODOROS CHRISTOUDIAS ET AL.

General

This paper addresses the impact of a nearby supernova on the Earth. The paper discusses in particular two consequences of a nearby supernova, namely stratospheric ozone loss and increase in cloud cover.

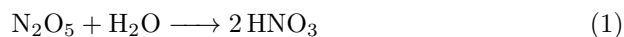
I suggest to not consider these two aspects in isolation. What counts for life on earth is the spectrum of UV-radiation at the ground – I think we agree here. The UV radiation at the ground depends on both ozone column and cloud cover, so that a decrease in total ozone does not necessarily mean more UV at the ground when there is an increase in cloud cover as well. (Other things might be relevant as well.) Perhaps something like an UV index at the ground could be calculated with the EMAC model? I know there are substantial uncertainties, but some discussion is warranted.

Overall, I suggest a bit more focus on processes – i.e., why are the findings as they are. Sometimes it is not clear if model results from this study are discussed or if processes known from the literature are presented.

In general, the study addresses an interesting topic (the effect of nearby supernova) and uses a sophisticated (three dimensional) model that is more advanced than many models used in the past. The paper is overall well written and the topic is certainly of interest to the readership of Comm. in Earth & Environ. I have some comments and suggestions; I am convinced that a revised version will be a great contribution to Comm. in Earth & Environ.

Comments

On page 3, left column there is a discussion on nitrogen chemistry. It is not clear how these arguments are derived. Is this discussion based on model results? For example, the importance of the reaction $N + NO \longrightarrow N_2 + O$ was pointed out earlier (Crutzen and Brühl, 1996); is the argument in line 125 based on the earlier study? Further, the NO_x is dependent on the fact whether the tropics or the polar latitudes are in question. This should be mentioned. The paper also states here that odd nitrogen is often present as compounds other than NO_x . Here the heterogeneous reaction



could be important – is this the case? Should this be mentioned? This reaction should be much stronger under volcanically perturbed conditions. These arguments apply in a similar way to the HO_x discussion below.

In the discussion of ozone depletion the implicit assumption is that low ozone means a higher UV-index. And it is the UV-index (or similar) that counts when the impact of terrestrial life is in question. But ozone depletion and UV-index is not the same thing, especially when the cloud cover changes as well. Suggest more discussion here. Is there a change in UV index available from the EMAC model (or could it be easily computed)?

It is pointed out in the paper that the impact of GCRs is not uniform over the globe. The impact is particularly strong at polar latitudes. Further the impact will be particularly strong in polar night, as GCRs producing HO_x and NO_x constitute the only source of radicals in the dark atmosphere (no photolysis). In earlier work (Müller and Crutzen, 1993, which is only partly relevant, as the work focuses on HCl), the point was made that certain compounds (e.g., $\text{CH}_3\text{O}_2\text{NO}_2$) could accumulate in polar night and act as a source of radicals shortly after polar sunrise. Could such processes be important here?

On Page 3 (bottom right) there is a discussion of a low-oxygen atmosphere. This is good. But about a million of years ago, many other things were different on Earth than today. For example, sea-level was lower, there are possible dust storms and global temperatures are estimated to be lower than today by 5-10 degrees. Could any of that be important for the arguments provided here? Perhaps this is most important for the CCN issue (cloud radiative forcing). Comparisons are made with conditions ‘today’ – but is this the most appropriate comparison?

There is a recent paper (Wang et al., 2022) that finds that “new particle formation in the upper free troposphere is a major global source of cloud condensation nuclei (CCN). However, the precursor vapours that drive the process are not well understood”. This finding supports in principle the arguments presented in the present manuscript, but points to the issue of precursor vapours that are briefly discussed in the manuscript. However, it is not clear that the model uses an appropriate set of precursor vapours for the supernova simulations. Further, Wang et al. (2022) point to the importance of the monsoon and dust – such aspects could be rather different say a million years ago. Further, the simulation of aerosol particle precursor vapours may be very different in different models (Wang et al., 2022; Kirkby et al., 2023). For example Kirkby et al. (2023) state that “human activities have considerably increased aerosols and cloudiness since preindustrial times, but they remain persistently uncertain and underrepresented in global climate models” – only one particular model is used here. I understand that many things are not known, but I suggest a bit more discussion here; perhaps the papers by Wang et al. (2022) and Kirkby et al. (2023) could also be helpful.

Details

- l. 042: zone $\longrightarrow$ Ozone
- l. 087: ‘Earth system models are needed’ – give an argument why
- l. 093: Is there a citation for the ion-induced processes?
- l. 109: citation for ‘previous estimates’?
- l. 123: the most recent report is WMO (2022)
- l. 123: Is there today a significant ozone loss over the tropics? If there is any loss at all it is very small (WMO, 2022). Thus a supernova induced ozone loss might have an impact on the tropics. In any case I suggest separating tropics and mid-latitudes here.

- l. 188: ‘factor of three less than today’: explain briefly why this is the case?
- l. 195: Why is it negligible in the tropics?
- l. 253: the increase from ‘pristine’ to ‘polluted’ is dramatic isn’t it?
- l. 236: shorter than what?
- l. 245: why a factor of 10? Is this obvious?
- l. 247: citation/explanation for the 57 s?
- l. 254: citation/explanation for the 500 s?
- l. 261: the ‘changes’ described here are SN driven, yes?
- l. 303: I think this argument needs a bit more discussion: first, why are the cloud top heights in the tropics at higher altitude (is this visible in one of the figures)? Second, why is a raised cloud top height equivalent with a positive climate forcing?
- Fig. 4: the bottom panel uses a linear x-axis. However the tropics cover a much larger area (for the same latitude range) than the polar region. Consider using an area weighted x-axis.
- the number of -2.5 W/m^2 is indeed comparable to the anthropogenic forcing since preindustrial (one could add a number and a citation here, perhaps Arias et al. (2021)), but the timescales in question are very different. I think it is worth pointing that out.
- lines 270-292 (conclusions) it is worth making this point, but this is not a conclusion of this study.
- l. 294: maximum of 30% – is this value for a particular season or is this an annual average?
- l. 397: units should not be in italics – use `\mathrm{cm}` etc.
- l. 432: give url for the Zendo archive
- Citation Workman $\longrightarrow$ check ‘Others’

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- WMO: Scientific assessment of ozone depletion: 2022, GAW Report No. 278, Geneva, Switzerland, 2022.

Review of “Impact of nearby supernovae on Earth’s atmosphere and climate”

We would like to thank the referees for their careful evaluation of our manuscript. We are most grateful for the comments, constructive criticism and useful suggestions received on improving the paper. We have edited and revised the paper to address the points raised by the reviewers. Our point-by-point replies are given below. In our replies, the line numbers refer to the revised manuscript.

Reviewer #1 (Remarks to the Author):

General

This paper addresses the impact of a nearby supernova on the Earth. The paper discusses in particular two consequences of a nearby supernova, namely stratospheric ozone loss and increase in cloud cover.

I suggest to not consider these two aspects in isolation. What counts for life on earth is the spectrum of UV-radiation at the ground – I think we agree here. The UV radiation at the ground depends on both ozone column and cloud cover, so that a decrease in total ozone does not necessarily mean more UV at the ground when there is an increase in cloud cover as well. (Other things might be relevant as well.) Perhaps something like an UV index at the ground could be calculated with the EMAC model? I know there are substantial uncertainties, but some discussion is warranted.

We agree with the reviewer that the detrimental effect of UV radiation is primarily of interest regarding the potential to significantly affect life on earth beyond abrupt climate change. The EMAC model encompasses a detailed shortwave radiation component that calculates the spectral actinic flux, but does not explicitly calculate a UV index considering the erythral weighing function. To meet the referee comment, we have used the EMAC model simulation output to drive the detailed Tropospheric Ultraviolet and Visible (TUV) Radiation Model to calculate the differences in UV-index at the surface based on both ozone and aerosols columns. We have added an additional panel in Fig. 4e to reflect the change in solar radiation at the surface, and report the results in more detail in the specific comment below.

Overall, I suggest a bit more focus on processes – i.e., why are the findings as they are. Sometimes it is not clear if model results from this study are discussed or if processes known from the literature are presented.

As it may not have been entirely clear which of our findings are based on our simulations and which on previous work in the literature, in particular relating to the processes, we have now added more elaborate discussion and highlight, using references, all the processes that were previously identified as relevant. More details are given in the comments below.

In general, the study addresses an interesting topic (the effect of nearby supernova) and uses a sophisticated (three dimensional) model that is more advanced than many models used in the past. The paper is overall well written and the topic is certainly of interest to the readership of Comm. in Earth & Environ. I have some comments and suggestions; I am convinced that a revised version will be a great contribution to Comm. in Earth & Environ.

We thank the reviewer for their consideration and comments.

Comments

On page 3, left column there is a discussion on nitrogen chemistry. It is not clear how these arguments are derived. Is this discussion based on model results? For example, the importance of the reaction $N + NO \rightarrow N_2 + O$ was pointed out earlier (Crutzen and Bruhl, 1996); is the argument in line 125 based on the earlier study? Further, the NO_x is dependent on the fact whether the tropics or the polar latitudes are in question. This should

be mentioned. The paper also states here that odd nitrogen is often present as compounds other than NO_x. Here the heterogeneous reaction



could be important – is this the case? Should this be mentioned? This reaction should be much stronger under volcanically perturbed conditions. These arguments apply in a similar way to the HO_x discussion below.

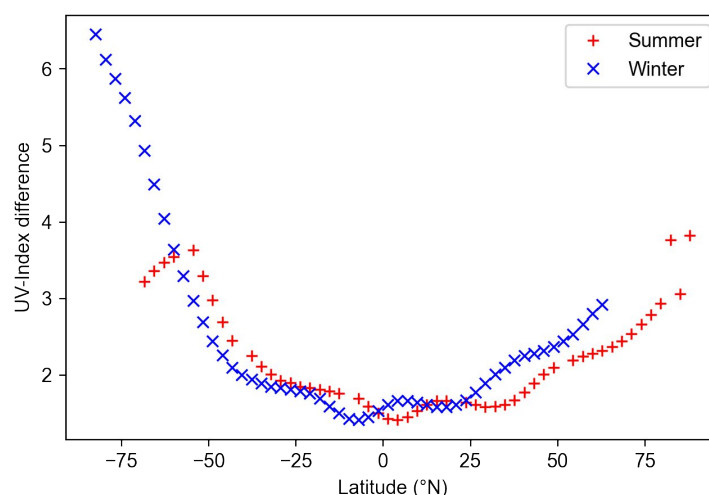
These insights are primarily based on the literature. We have highlighted the relevant citation to the previous study by Crutzen and Brühl (1996) regarding the nitrogen chemistry reactions and the HNO₃ reservoir, and to the works by H Svensmark, P Crutzen, H. Johnston, D. Bates and N. Nicolet regarding the HO_x reactions cycles, as also requested by Reviewer #3.

The reaction N₂O₅+H₂O in the gas phase is slow, but much more efficient on aqueous particles, as highlighted by the referee. Both mechanisms are included in the model. The stratospheric background aerosol and polar stratospheric clouds are accounted for in our simulations using the MSBM submodel (lines 401-405), but not volcanically enhanced aerosols.

In the discussion of ozone depletion the implicit assumption is that low ozone means a higher UV-index. And it is the UV-index (or similar) that counts when the impact of terrestrial life is in question. But ozone depletion and UV-index is not the same thing, especially when the cloud cover changes as well. Suggest more discussion here. Is there a change in UV index available from the EMAC model (or could it be easily computed)?

As the reviewer points out, the risk stems from increased UV radiation and not directly from ozone depletion. A very detailed analysis of the effect of reduced ozone levels on surface UV exposure and impacts on biota is given in the paper by D. Galante and J.E. Horvath (doi:10.1017/S1473550406003545). We have included this reference in our manuscript (line 378).

Following the comment by the reviewer, we have used the Tropospheric Ultraviolet and Visible (TUV) Radiation Model to calculate the difference in UV-index during clear-sky (no cloud) conditions (during maximum UV impact), taking into account the ozone layer and aerosol optical thickness differences between the SN and nominal GCR simulations. The average difference in boreal summer and winter as a function of latitude is shown below:



The average difference near the equator and in middle latitudes, where the most of life resides and has evolved is within a maximum difference of 2. There are more pronounced differences in polar latitudes, with greater ozone depletion. This has been added to the text (lines 371-378). However, the SN aerosol effects also lead to increased cloudiness and thus further limit the UV exposure. The overall change in solar radiation reaching the

surface of the Earth is only a few percent, and is higher only in the polar regions. We have added a panel in Fig. 4e to show this, and would like to thank the reviewer for this excellent suggestion.

It is pointed out in the paper that the impact of GCRs is not uniform over the globe. The impact is particularly strong at polar latitudes. Further the impact will be particularly strong in polar night, as GCRs producing HOx and NOx constitute the only source of radicals in the dark atmosphere (no photolysis). In earlier work (Müller and Crutzen, 1993, which is only partly relevant, as the work focuses on HCl), the point was made that certain compounds (e.g., CH₃O₂NO₂) could accumulate in polar night and act as a source of radicals shortly after polar sunrise. Could such processes be important here?

Our model includes a full atmospheric chemistry mechanism (encompassing CH₃O₂NO₂), and captures all cascade reactions and subsequent effects after the introduction of radical by the SN GCR throughout the atmosphere. We also simulate the photochemistry changes occurring during sunrise and sunset over the poles. Our model also includes chlorine nitrate (ClONO₂), which accumulates during polar night. This is of low relevance to our study, as for most of geological time (except the period with CFCs until about 2060 when the ozone hole will disappear), chlorine levels are low.

On Page 3 (bottom right) there is a discussion of a low-oxygen atmosphere. This is good. But about a million of years ago, many other things were different on Earth than today. For example, sea-level was lower, there are possible dust storms and global temperatures are estimated to be lower than today by 5-10 degrees. Could any of that be important for the arguments provided here? Perhaps this is most important for the CCN issue (cloud radiative forcing). Comparisons are made with conditions 'today' – but is this the most appropriate comparison?

Indeed, some conditions may have been different in the distant past, although the oxygen content is expected to be the key parameter. Unfortunately, it is not feasible to run a paleoclimate simulation capturing all prevalent conditions (dust storms, sea level, etc.) in the level of detail allowed by our Earth system model, but we have conducted a sensitivity test with reduced atmospheric oxygen levels to be directly comparable to the reference simulation. This does not purport to be a full paleoclimate assessment, but altering just the oxygen content of the atmosphere allows us to estimate the effects on the ozone layer. This has been added to the text (lines 183-191).

To avoid the impression that we performed full paleoclimate conditions, we added a section heading “Low-oxygen atmosphere”. To highlight our result that the 2% oxygen atmosphere provides protection to life on the planet, we added the following remark in the conclusions: “Interestingly, in an atmosphere with ten times less oxygen than today, the ozone layer is only three times thinner.”

There is a recent paper (Wang et al., 2022) that finds that “new particle formation in the upper free troposphere is a major global source of cloud condensation nuclei (CCN). However, the precursor vapours that drive the process are not well understood”. This finding supports in principle the arguments presented in the present manuscript, but points to the issue of precursor vapours that are briefly discussed in the manuscript. However, it is not clear that the model uses an appropriate set of precursor vapours for the supernova simulations. Further, Wang et al. (2022) point to the importance of the monsoon and dust – such aspects could be rather different say a million years ago. Further, the simulation of aerosol particle precursor vapours may be very different in different models (Wang et al., 2022; Kirkby et al., 2023). For example Kirkby et al. (2023) state that “human activities have considerably increased aerosols and cloudiness since preindustrial times, but they remain persistently uncertain and underrepresented in global climate models” – only one particular model is used here. I understand that many things are not known, but I suggest a bit more discussion here; perhaps the papers by Wang et al. (2022) and Kirkby et al. (2023) could also be helpful.

Our simulations rely on the binary sulfuric acid-water nucleation schemes as measured by CLOUD and published in Dunne et al. (2016). Vapours of natural origin that reach high into the stratosphere are the most relevant vapours for nucleation in the pristine (no anthropogenic influence) atmosphere for the past Millions of years, the cosmic timescales relevant for potential SN extinction events. The paper by Wang et al. refers to synergistic nucleation that requires the presence of NH₃ that is indeed associated with high uncertainty. However, the recently observed high NH₃ concentrations in the South Asian summer monsoon upper troposphere are linked to anthropogenic activities, and are only relevant to the recently increasing pollution levels in the region. Further, NH₃ is emitted at the surface (not through volcanic injections as is the origin for H₂SO₄) and thus has limited

impact on the upper troposphere and none in the stratosphere. Dust is similarly emitted at the surface and, being coarse-size particles, is not directly influenced by the SN-enhanced irradiance. None of these processes are directly affected by SN, even if it would occur today. They would actually compete with and, therefore, moderate the impact of ion-induced nucleation.

Details

- [I. 042: zone → Ozone](#) Corrected. Thank you.
- [I. 087: 'Earth system models are needed' – give an argument why](#)
ESMs can uniquely capture the complex circulation dynamics and process feedbacks. Added to the text.
- [I. 093: Is there a citation for the ion-induced processes?](#)
Yes, we have added citations to Dunne et al., 2016; Ehrart et al., 2018.
- [I. 109: citation for 'previous estimates'?](#) Citation added: Thomas et al 2005 (line 120).
- [I. 123: the most recent report is WMO \(2022\)](#) : Cited the most recent Report. We would like to thank the reviewer for the remark.
- [I. 123: Is there today a significant ozone loss over the tropics? If there is any loss at all it is very small \(WMO, 2022\). Thus a supernova induced ozone loss might have an impact on the tropics. In any case I suggest separating tropics and mid-latitudes here.](#)
We agree with the reviewer that a general statement may be misleading. The text was rephrased to improve the statement.
- [I. 188: 'factor of three less than today': explain briefly why this is the case?](#)
Added remark: "only a factor of three less than today's atmospheric average of roughly 300DU, as observed by the WMO Global Ozone Observing System". We added the following explanation: "A reduction of stratospheric ozone allows deeper penetration of short-wave UV radiation that photodissociates oxygen ($h\nu < 242\text{nm}$). Therefore, O₃ formation relocates to lower altitudes with higher air and oxygen density, causing a vertical redistribution of ozone, which moderates ozone reduction in the vertical column."
- [I. 195: Why is it negligible in the tropics?](#)
This was changed to "an order of magnitude lower" as pointed out by the reviewer.
- [I. 253: the increase from 'pristine' to 'polluted' is dramatic isn't it?](#)
Indeed, the CCN concentrations change are sizeable and climatically relevant, as stated in our manuscript, but have not been implicated to cause a mass extinction event.
- [I. 236: shorter than what?](#) "than present-day" added to the text. Thank you for the detailed observation.
- [I. 245: why a factor of 10? Is this obvious?](#) ↓
- [I. 247: citation/explanation for the 57 s?](#) ↓
- [I. 254: citation/explanation for the 500 s?](#)

We agree with the referee that these numbers in line 245-254 need to be explained more clearly. We have therefore changed the text as follows:

Line 241 onwards:

"The effect of the shorter ion lifetimes is as follows. At high ionisation rates, Q (i.p. $\text{cm}^{-3}\text{s}^{-1}$), the ion lifetime is dominated by ion-ion recombination, so $Q = \alpha \cdot n_{\text{ip}}^2$, where α is the ion-ion recombination coefficient ($1.6\text{E-}6 \text{ cm}^3\text{s}^{-1}$ throughout the troposphere [Zauner-Wieczorek et al, ACP, 2022]) and n_{ip} (cm^{-3}) is the ion-pair concentration. The ion lifetime is $\tau (\text{s}) = 1/(\alpha \cdot n_{\text{ip}}) = 1/(\alpha \cdot Q)^{0.5}$. So, at 100 times present ionisation rates, ion lifetimes are a factor 10 shorter than in the present-day atmosphere. During the supernova, the ion lifetime is $\tau = 56 \text{ s}$ at ground level ($Q = 200 \text{ i.p. cm}^{-3}\text{s}^{-1}$), and 18 s at 10 km ($Q = 4000 \text{ i.p. cm}^{-3}\text{s}^{-1}$).

These lifetimes are much shorter than the mean time for a primary ion to form a bisulfate (HSO_4^-) ion, marking the initial step for ion-induced nucleation to take place. The collision rate of each negative ion with an H_2SO_4 molecule is $k (\text{s}^{-1}) = k^0 \cdot [\text{H}_2\text{SO}_4]$, where k^0 is the charged-neutral collision rate coefficient ($2\text{E-}9 \text{ cm}^3\text{s}^{-1}$ [He et al, Science, 2021]) and $[\text{H}_2\text{SO}_4]$ (cm^{-3}) is the sulfuric acid vapour concentration. For example, at a representative sulfuric acid concentration of 10^6 cm^{-3} , the collision rate of each negative ion with a sulfuric acid molecule is $k = 2\text{E-}3 \text{ s}^{-1}$. i.e. a mean time of 500 s . This implies that only around 10% or less of the ions will form an HSO_4^- ion before they are neutralised. Moreover, a

further 500 s is required to nucleate the first molecular cluster, $\text{H}_2\text{SO}_4\cdot\text{HSO}_4^-$, during which time the HSO_4^- ion is undergoing neutralization by ion-ion recombination. Consequently, only a small fraction of the ions deposited by GCR from a nearby SN are available to drive ion-induced nucleation of sulfuric acid particles.”

- I. 261: the ‘changes’ described here are SN driven, yes? Yes, clarified in the text.
- I. 303: I think this argument needs a bit more discussion: first, why are the cloud top heights in the tropics at higher altitude (is this visible in one of the figures)? Second, why is a raised cloud top height equivalent with a positive climate forcing? Answer: 1) When more particles are available to form cloud droplets the latter are smaller and take longer to form ice and rain, so they penetrate deeper into the atmosphere¹ 2) The higher clouds are colder, hence the infrared cooling to space is less.
- Fig. 4: the bottom panel uses a linear x-axis. However the tropics cover a much larger area (for the same latitude range) than the polar region. Consider using an area weighted x-axis. Changed to an area weighted x-axis.
- the number of -2.5 W/m^2 is indeed comparable to the anthropogenic forcing since preindustrial (one could add a number and a citation here, perhaps Arias et al. (2021)), but the timescales in question are very different. I think it is worth pointing that out. Added. Thank you for the suggestion.
- lines 270-292 (conclusions) it is worth making this point, but this is not a conclusion of this study. This was now moved to previous section. Thank you.
- I. 294: maximum of 30% – is this value for a particular season or is this an annual average? This is local seasonal max – Clarified in the text.
- I. 397: units should not be in italics – use cm etc. This is now done. Thank you for the suggestion that improved the presentation.
- I. 432: give url for the Zendo archive This will be added once the manuscript is finalized after review in order to correspond to the exact version published.
- Citation Workman ---→ check ‘Others’ Corrected. We would like to thank the reviewer for their diligence.

Reviewer #2 (Remarks to the Author):

The paper by Christoudias considers a hypothetical scenario of a nearby ($\sim 100 \text{ pc}$) supernova focusing on its possible impact on the Earth's atmosphere. The topic is interesting for a detailed investigation but the level of preparedness of this work is insufficient for a high-impact publication. The paper needs to be significantly improved before it can be acceptable. More details are given below.

The Reviewer raised some very important points. We have strived to respond to all comments and enhance the model description to reach the required standards for reproducibility.

The paper fails to meet the condition of being reproducible. The details of the model are not properly described (see below), and the composition of the model experiments is not convincing.

The inherent complexity of global climate-chemistry Earth System models, often the result of decades-long development effort requires to resort to references of previous documentation for all the different components. The available references that describe the model have been added to the manuscript in the Methods section to detail the model code and experiments:

- Jockel et al (2010) and Roeckner et al (2003, 2004, 2006) document the model code, and dynamics.
- Pringle et al (2010) presents and evaluates the model aerosol scheme.
- The full chemistry mechanism is presented in Poschl et al (2000), and evaluated in Pozzer et al (2007).
- The CCN activation and cloud formation is after Karydis et al (2017) and Bacer et al (2018).
- Our model accounts for the formation of polar stratospheric clouds following Kirner et al, (2011, 2015).
- Ions and new particle formation are detailed in Ehrhart et al. (2018)

Moreover, the model is publicly available after signing a user protocol, hence the results are reproducible and documented. The precise version of the code used is given in Sec. 5.4. To enhance the ease to reproduce the

¹ “Improving our fundamental understanding of the role of aerosol–cloud interactions in the climate system”, J Seinfeld et al., PNAS 113 (21) 5781-5790, <https://doi.org/10.1073/pnas.1514043113>

study, we have improved the description in the Methods section, included additional references, and have added a Table with the simulation input parameters, following the suggestion of the reviewer. Finally, the full dataset shown in the figures will be publicly available at Zenodo once the manuscript is finalized after the peer review process.

1. A list or a Table of the considered scenarios should be clearly presented, including all the used parameters, e.g., geomagnetic field, heliospheric modulation, to the level where any reader can reproduce the input parameters.

We thank the reviewer for this suggestion to enhance the ease to reproduce our simulations. A table detailing the relevant parameters has been added to the manuscript.

2. Many details are not available to the reader.

a. The assumption of a simple 100x increase in GCR is unjustified and must be explained. The spectrum of GCR from a nearby supernova is not just a 100x enhancement of the present-day spectrum. The latter is an 'equilibrium' spectrum of a large number of sources over a huge volume that cannot be reached for a nearby transient event (supernova). The SN-related spectrum would be first harder and then softer than the 'standard' GCR spectrum. Moreover, the most geoeffective part of the GCR spectrum (a few GeV) is strongly modulated in the heliosphere, thus, the exact heliospheric conditions must be specified.

We agree with the reviewer that the energy spectrum of the incident GCRs from a nearby incident supernova will change with time after the event, and this will affect the ionisation rate in the atmosphere versus altitude and latitude. The GCR fluxes will also be modulated by the heliospheric magnetic field (solar wind) and the geomagnetic field. However, these influences on the GCR flux are relatively minor in comparison with the major sources of modulation, namely: a) the proximity of the supernova, b) the alignment or otherwise of the galactic magnetic field between the supernova and Earth and c) the time since the SN event occurred. It is beyond the scope of the present paper to consider all these effects and so we have studied the influence of a factor 100 increase of GCR flux, with the same relative ionisation profile versus altitude and latitude as the present day. We have chosen a factor 100 to represent the peak GCR flux estimated by Melott et al (2017) for a SN at 50 pc and with a turbulent galactic magnetic field. From Figs. 1B and 2B in Melott et al (2017), we consider this well represents the maximum expected flux over the first ~1 ky after the SN event, and our approximation of an ionisation vs altitude similar to the present day is reasonable.

To clarify this assumption, we have now stated it explicitly in the revised manuscript (lines 100-111).

b. The model run is too short – one year is too short even for the gamma-ray scenario, because of the longer-lasting atmospheric effects. A longer model run is even more important for the cosmic-ray effect which can be accumulated over years leading to a new equilibrium state and the related effects.

The complexity of our model increases the computational demand and prohibits decadal runs. We have performed year-long runs after a model spin-up period of an additional year. We have further extended the 100-times GCR simulation for 5 years and observed a stable Ozone atmospheric cycle with no overall trends. From experience, we know that a one-year spin-up period is sufficient as the chemical lifetimes of the species involved are much shorter.

c. The model based on the CERN CLOUD experiment needs to be fully described. There is a reference to Dunne et al. (2016) but that reference does not provide the full model description. Is it the same as the MESSy NAN model?

The model is given in full detail in the Supplementary Materials (Sec. 8) of the Dunne et al. 2016 Science publication, and is implemented by the NAN submodel in our model code. This is now clarified in the text (lines 409-415).

d. The description of chemistry (page 3) is very basic and doesn't need to be here in full.

The major reaction pathways are presented here to provide insight to the reader and explain the perhaps counter-intuitive effect of the null cycles of the radicals that limits the overall impact. We feel that insight into these processes is of value to readers from a broader audience, as also highlighted by Reviewer #1.

e. Fig.2 needs to be explained in more detail. Relative to what is the effect shown?

We have added details in the caption to aid the explanation of the Figure. The effect is shown relative to present-day GCR intensity, and with the same simulations but with the atmospheric Oxygen content at 2%.

f. It needs to be explained how the ionization was computed for the 100x increase.

The ion production rate was scaled globally by a factor 100 over present-day (year 2010) rates modelled by the IONS submodel (Ehrhart et al., 2018) throughout the atmosphere. This is now clarified in the text (lines 109-112, 429-436).

g. The statement in lines 244-248 RHS (on the recombination times) needs to be explained.

h. The statement in lines 256-259 LHS needs to be shown.

g,h: We agree with the referee that the statements and numbers in lines 245-254 needed to be explained more clearly. This was also raised by Reviewer #1:

Line 241 onwards:

“The effect of the shorter ion lifetimes is as follows. At high ionisation rates, Q (i.p. $\text{cm}^{-3}\text{s}^{-1}$), the ion lifetime is dominated by ion-ion recombination, so $Q = \alpha \cdot n_{ip}^2$, where α is the ion-ion recombination coefficient ($1.6\text{E-}6 \text{ cm}^3\text{s}^{-1}$ throughout the troposphere [Zauner-Wieczorek et al, ACP, 2022]) and n_{ip} (cm^{-3}) is the ion-pair concentration. The ion lifetime is τ (s) = $1/(\alpha \cdot n_{ip}) = 1/(\alpha \cdot Q)^{0.5}$. So, at 100 times present ionisation rates, ion lifetimes are a factor 10 shorter than in the present-day atmosphere. During the supernova, the ion lifetime is $\tau = 56$ s at ground level ($Q = 200$ i.p. $\text{cm}^{-3}\text{s}^{-1}$), and 18 s at 10 km ($Q = 4000$ i.p. $\text{cm}^{-3}\text{s}^{-1}$).

These lifetimes are much shorter than the mean time for a primary ion to form a bisulfate (HSO_4^-) ion, marking the initial step for ion-induced nucleation to take place. The collision rate of each negative ion with an H_2SO_4 molecule is k (s^{-1}) = $k^0 \cdot [\text{H}_2\text{SO}_4]$, where k^0 is the charged-neutral collision rate coefficient ($2\text{E-}9 \text{ cm}^3\text{s}^{-1}$ [He et al, Science, 2021]) and $[\text{H}_2\text{SO}_4]$ (cm^{-3}) is the sulfuric acid vapour concentration. For example, at a representative sulfuric acid concentration of 10^6 cm^{-3} , the collision rate of each negative ion with a sulfuric acid molecule is $k = 2\text{E-}3 \text{ s}^{-1}$. i.e. a mean time of 500 s. This implies that only around 10% or less of the ions will form an HSO_4^- ion before they are neutralised. Moreover, a further 500 s is required to nucleate the first molecular cluster, $\text{H}_2\text{SO}_4 \cdot \text{HSO}_4^-$, during which time the HSO_4^- ion is undergoing neutralization by ion-ion recombination. Consequently, only a small fraction of the ions deposited by GCR from a nearby SN are available to drive ion-induced nucleation of sulfuric acid particles.”

i. Page 8: for what epoch were the IGRF parameters considered, and how exactly was the method by Frazer-Smith applied? A reader must be able to repeat the computation precisely.

The IGRF parameters considered refer to the common era (year 2010). The Frazer-Smith methods was applied through the EMAC IONS submodel, extensively described in Ehrhart et al., 2018. This has been added to the text (line 421-428).

3. The considered scenarios contain many simplified assumptions, which is fine but the authors should clearly state that this is not a fully realistic modelling of a nearby SN but a conservative simplistic estimate of the effect.

Indeed, some simplifying assumptions are necessary to tackle the question that is central to our study due to the challenge posed by the multi-scale problem and the complexity of the system. However, it is the view of the authors that such a statement is unnecessary. All models rely on simplifications, tabulations and parameterisations of the physical systems simulated. This is well understood by the community and the modelling details for our study, including any underlying assumptions, are readily available to the reader in the Methods section, and have been further enhanced based on the comments of the Reviewer. Moreover, we feel that such a statement would detract from one of the main points of our study – we present marked improvement in modelling

complexity and resolution and a much larger (more realistic) number of processes compared over to the column or 2D models used to date in the past to investigate this question. This is the first time that the atmospheric dynamical, cloud and climate effects have been explicitly and interactively simulated in such simulations.

Other comments:

- Line 42: "increased aerosol particles" – increased in size or number of?

Number of particles (added in text)

- The sentence in lines 65—69 needs a reference – where has it been postulated?

Added references to relevant papers by Ellis and Schramm (1995) and Wallner et al. (2016).

- Reference (Fields et al., 2019) is to an ArXiv preprint of 2019 which is unacceptable. Has it been published as a peer-review paper? If not, the reference should be replaced or removed.

That is an Astro2020 Science White Paper submitted to the 2020 Decadal Survey on Astronomy and Astrophysics. These whitepapers have been submitted by the scientific community and are reviewed by the Decadal Survey Committee and Panels.

- Line 137 RHS (right-hand side) column: a reference or an explanation is needed for why it is unlikely.
- Page 4, lines 186-187, RHS column: please provide a reference for the gamma-ray pulse from a SN. Reference added for these points.
- The roles of GB, ES and MK authors are not specified in section 5.5.

All authors took part in the scientific discussion and provided comments on the manuscript. We have expanded Sec. 5.5 to highlight this.

- Line 192, RHS column: please explain the statement of "one billion chest X-rays" since the conversion is not straightforward – I was not able to reproduce this number even approximately. It would be better to remove this statement as not directly related to the focus of the paper.

We agree with the reviewer and have removed the statement as indicated.

- Fig. 3 is difficult to read as the contours/numbers seem unrelated to the colour code. Is it correct to interpret that, e.g., the columnar O₃ is reduced to ~60 DU (120 DU x 50%) in May at 80°N? This needs to be better explained.

That is the correct interpretation. The caption of Fig.3 has been refactored to be easier for the reader to interpret the plot.

- Line ~277 RHS: when discussing the cosmic-ray-related dose please specify the location.

Specified location in the text: "in the tropics and mid-latitudes"

Reviewer #3 (Remarks to the Author):

This manuscript presents the authors' study of the impact on Earth's stratospheric ozone layer and climate of a nearby supernovae, which exposed the Earth to an intense cosmic-ray radiation with two orders of magnitude stronger than the present intensity. The authors evaluate the impacts using an Earth system model (ESM) that incorporates some selected atmospheric chemistry and aerosol processes. Their simulated results mainly show that the effects of intense cosmic radiation through (NO, NO₂) and (OH, HO₂) catalytic cycles cause moderate stratospheric ozone depletion, and that the enhancement in aerosol and clouds from the elevated cosmic ray flux leads to a negative radiative forcing comparable in magnitude to current anthropogenic positive climate forcing. They therefore conclude that Earth's atmosphere and geomagnetic field are effective at shielding the biosphere from nearby supernovae. Reading this manuscript, I have two major concerns:

We thank the reviewer for their comments. Please find our response to the two concerns below.

(1) The citing of the relevant references is very selective or even incorrect/misleading, especially on the link between galactic cosmic rays (GCR) and cloud formation and on (NO, NO₂) and (OH, HO₂) catalytic cycles for ozone depletion. The credits to H Svensmark, P Crutzen, H. Johnston, D. Bates and N. Nicolet are not properly given.

As it may not have been clear which of our findings are based on our simulations and which rely on previous work in the literature, we now highlight, using references, the previously identified links between GCR and NO_x/HO_x, their catalytic cycles and cloud formation. We have added citations to the relevant articles by H Svensmark, P Crutzen, H. Johnston, D. Bates and N. Nicolet, as also requested by Reviewer #1.

(2) The study considers only the GCR-driven (NO, NO₂) and (OH, HO₂) catalytic cycles for ozone depletion but ignores the cosmic-ray driven (Cl, ClO) catalytic cycles. The latter process is well documented in the literature, such as Q.-B. Lu, Fingerprints of the cosmic ray driven mechanism of the ozone hole. *AIP Advances* 11, 115307 (2021); Formulation of the cosmic ray–driven electron-induced reaction mechanism for quantitative understanding of global ozone depletion. *Proc. Natl. Acad. Sci. USA* 120, e2303048120 (2023). If the authors want to exclude the latter process, they must state clearly that their conclusions are drawn only from the results of ozone depletion simulated from the GCR-caused (NO, NO₂) and (OH, HO₂) catalytic cycles but no CR-driven (Cl, ClO) cycles for stratospheric ozone depletion.

The CR–driven electron-induced reaction (CRE) leading to ozone depletion in recent years is dominated by the atmospheric abundances of CFCs and HCFCs which are of anthropogenic origin. Though these produce a significant effect at present, they were not prevalent over the cosmological timescales relevant for nearby supernova events (millions of years in the past and in the future), and with current emission control policies can be expected to end around 2060. Thus, the relevant SN-induced CRE effect is dependent on the concentrations of naturally-occurring Ozone-depleting I and Br containing substances (mainly CH₃Br) which we understand have lower reactivity (we note that the rate constant for Br loss rate is 10 times lower at 298K than for Cl).

This discussion was added to the paper (lines 140-156), with citation to Qing-Bin Lu (2023).

Our simulations employ the MSBM submodel to account for the formation of polar stratospheric clouds with detailed heterogeneous chemistry reactions of ozone-reducing compounds (Kirner et al., 2011; 2015). This, along with the relevant references, have been added to the manuscript (lines 401-405).

Finally, even though we don't explicitly account for the empirical scaling by the square of the ion-concentration of loss-rate, the CR ion production is effectively modulated by ion-recombination, as discussed in our manuscript.

15th Apr 24

Dear Dr Christoudias,

Your manuscript titled "Impact of nearby supernovae on Earth's atmosphere and climate" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

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Joe Aslin
Deputy Editor,
Communications Earth & Environment
<https://www.nature.com/commsenv/>
Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

See attached PDF.

Reviewer #2 (Remarks to the Author):

The authors have significantly improved the quality of the manuscript when addressing the criticism raised by the reviewers. I am glad to recommend it for acceptance subject to a very minor revision as specified below. In case my suggestions are straightforwardly addressed, I don't need to see the revised paper.

1) The authors are reluctant to state that the work presents a rough estimate of the SN effect, but I insist that it should be stated that this work does not pretend to be a precise model of the SN effect on the Earth's environment. The reason is that, even though the atmospheric model is precise and accurate, the background assumptions on the GCR spectra and time evolution (as discussed in my previous review) are only order-of-magnitude estimates. In addition, as argued by Reviewer 1, assumptions on the low-oxygen atmosphere are also imprecise. Accordingly, while the general result

that the overall SN effect is modest, is robust and undisputable, it is important to emphasise that this work cannot serve as a full model of the impact of a specific SN on the Earth system in its complexity. One sentence would be sufficient.

2) The issue with the reference to Fields et al. (2019) is still not satisfactorily resolved. The Arxiv reference is no good. At least, it should refer to Bull. Am. Astron. Soc., 51, 410 (2019). Interestingly, I was unable to find that paper published in Astro2020. Moreover, this is a White Paper which produces no scientific results per se but just a wrap-up. The fact that nearby SNs are expected to occur once per few Myr was not the discovery of that White paper but is further referenced in research works.

Reviewer #3 (Remarks to the Author):

After reading the introductory section (lines 1-166) of the revised manuscript, I do not think that the authors have given appropriate citations of original publications on the GCR-cloud/climate connection and ozone depletion mechanisms (HOx, NOx, Clx, CRE), some of which seems inconsistent with the literature [see, e.g., the (text)book “Atmospheric Chemistry and Global Change” edited by Guy P. Brasseur, John J. Orlando, and Geoffrey S. Tyndall (Oxford University Press, 1999)]. The authors must also cite a reference on their statement that naturally-occurring ozone depleting halogen-containing substances are mainly CH₃Br. It is true that the cited reference (Lu, 2023) investigated the CR-driven electron-induced reaction (CRE) of anthropogenic halogen-containing ozone depleting substances (CFCs, HCFCs, etc) for atmospheric ozone depletion. However, the reference did not state that naturally-occurring ozone depleting substances are mainly Br-containing molecules. Alternatively, the authors must clearly state that the CR-driven (Cl, ClO) cycle for stratospheric ozone depletion is not included in their study reported in the current manuscript. These revisions are needed before a recommendation for publication in Communications Earth & Environment can be made.

Second review of “Impact of nearby supernovae on Earth’s atmosphere and climate ”

BY THEODOROS CHRISTOUDIAS ET AL.

General

As stated in my first review, this paper addresses the impact of a nearby supernova on the Earth – this is of interest to the readership of Comm. in Earth & Environ. In particular the paper discusses two consequences of a nearby supernova, namely stratospheric ozone loss and increase in cloud cover, with a resulting impact on UV on the surface of the earth.

In response to the reviewer comments (in particular reviews one and two) the paper has seen many changes and adjustments. I think that the applied changes have improved the paper. In particular, I think an important improvement is that the authors have used the Tropospheric Ultraviolet and Visible (TUV) radiation model to calculate the difference in UV-index during clear-sky (no cloud) conditions (during maximum UV impact), taking into account the changes of the ozone layer and aerosol optical thickness. This is good.

Overall, I support publication of the paper close to its present form. Clearly, the topic is of interest to the readership of Comm. in Earth & Environ. I have some final comments and suggestions; these suggestions should be taken into account when formulating the final version of the paper.

Comments

Citations on stratospheric ozone chemistry

In the reply (to review three) it is stated that: “We have added citations to the relevant articles by H Svensmark, P Crutzen, H. Johnston, D. Bates and N. Nicolet”. (A similar comment is found in the reply to review one.)

However, citations to H. Johnston, D. Bates and N. Nicolet are not found in the revised manuscript. I think this is okay, as this paper is not addressing the entire history of stratospheric ozone science. If it were of interest to look back at that issue, citing Brasseur (2019) might be an option or the review by Portmann et al. (2012). Should the authors decide to cite Johnston (1971), I would recommend to also cite Crutzen (1970).

CR–driven electron-induced reaction...

The sentences on the CRE mechanism (starting at l. 140) were implemented (I assume) in reply to review three. However, the so-called CRE theory of ozone depletion is considered as very problematic in the community (e.g., Chipperfield et al., 2022, and references therein); it should not be considered as an independent theory of polar ozone loss.

I agree with the authors that CFCs were not prevalent over the cosmological timescales relevant here, so that I do not consider it necessary that the CRE

mechanism is discussed in this paper. But if it is discussed, it should not be considered as established science.

Details

- l. 155/156: I assume that here the reaction of CH_3Br with OH is meant; however, Lu (2023) is not a good reference for rate constants. My choice would be different (Burkholder et al., 2020), but I suggest removing the entire discussion anyway.
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- In the reply it is stated that the “model also includes chlorine nitrate (ClONO_2), which accumulates during polar night” – while I agree with the authors that this issue is of low relevance, I wonder why ClONO_2 accumulates in polar night – is it not removed by heterogeneous reaction with HCl ?
- Comments on NH_3 in the reply: I agree with the authors that any NH_3 emitted to the atmosphere due to human activities is not relevant for this study. However I am not so sure that NH_3 in principle is not relevant for the upper troposphere; large amounts of NH_3 and NH_4NO_3 have been observed in the monsoon at upper tropospheric altitudes (but of course only in recent years) (e.g. Höpfner et al., 2019).

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Second Review of “Impact of nearby supernovae on Earth’s atmosphere and climate”

Dear Editor,

We would like to thank the referees for their careful evaluation of our revised manuscript. We are most grateful for the comments and useful suggestions received to improve the paper. We have edited and revised the paper to address all suggestions by the reviewers. Our point-by-point replies are given below.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

General

As stated in my first review, this paper addresses the impact of a nearby supernova on the Earth – this is of interest to the readership of Comm. in Earth & Environ. In particular the paper discusses two consequences of a nearby supernova, namely stratospheric ozone loss and increase in cloud cover, with a resulting impact on UV on the surface of the earth.

In response to the reviewer comments (in particular reviews one and two) the paper has seen many changes and adjustments. I think that the applied changes have improved the paper. In particular, I think an important improvement is that the authors have used the Tropospheric Ultraviolet and Visible (TUV) radiation model to calculate the difference in UV-index during clear-sky (no cloud) conditions (during maximum UV impact), taking into account the changes of the ozone layer and aerosol optical thickness. This is good. Overall, I support publication of the paper close to its present form. Clearly, the topic is of interest to the readership of Comm. in Earth & Environ. I have some final comments and suggestions; these suggestions should be taken into account when formulating the final version of the paper.

We would like to thank the reviewer for their consideration and comments. We have implemented all suggestions to formulate the final version.

Citations on stratospheric ozone chemistry

In the reply (to review three) it is stated that: “We have added citations to the relevant articles by H Svensmark, P Crutzen, H. Johnston, D. Bates and N. Nicolet”. (A similar comment is found in the reply to review one.) However, citations to H. Johnston, D. Bates and N. Nicolet are not found in the revised manuscript. I think this is okay, as this paper is not addressing the entire history of stratospheric ozone science. If it were of interest to look back at that issue, citing Brasseur (2019) might be an option or the review by Portmann et al. (2012). Should the authors decide to cite Johnston (1971), I would recommend to also cite Crutzen (1970).

Following the reviewer suggestion, we have added citations to Bates and Nicolet (1950), Crutzen (1970), Johnston (1971), Portmann et al. (2012).

CR-driven electron-induced reaction. . .

The sentences on the CRE mechanism (starting at l. 140) were implemented (I assume) in reply to review three. However, the so-called CRE theory of ozone depletion is considered as very problematic in the community (e.g., Chipperfield et al., 2022, and references therein); it should not be considered as an independent theory of polar ozone loss.

I agree with the authors that CFCs were not prevalent over the cosmological timescales relevant here, so that I do not consider it necessary that the CRE mechanism is discussed in this paper. But if it is discussed, it should not be considered as established science.

We agree with the reviewer that the CRE mechanism is not relevant or necessary to discuss in this context, and have removed the discussion.

Details

- l. 155/156: I assume that here the reaction of CH₃ Br with OH is meant; however, Lu (2023) is not a good reference for rate constants. My choice would be different (Burkholder et al., 2020), but I suggest removing the entire discussion anyway.

We agree with the reviewer and have removed the entire discussion.

- Citation Kirner 2011: “emac” and “psc” should not be in small letters; (perhaps also elsewhere in the list of references).

The letter case has been corrected in this and other instances in the References.

- In the reply it is stated that the “model also includes chlorine nitrate (ClONO₂), which accumulates during polar night” – while I agree with the authors that this issue is of low relevance, I wonder why ClONO₂ accumulates in polar night – is it not removed by heterogeneous reaction with HCl?

ClONO₂ can accumulate because it does not photolyse during the polar night, but as correctly pointed out by the reviewer, it can react heterogeneously in the regions of polar stratospheric clouds. In any case, all these processes are included in our model and do not significantly influence the current results.

- Comments on NH₃ in the reply: I agree with the authors that any NH₃ emitted to the atmosphere due to human activities is not relevant for this study. However I am not so sure that NH₃ in principle is not relevant for the upper troposphere; large amounts of NH₃ and NH₄ NO₃ have been observed in the monsoon at upper tropospheric altitudes (but of course only in recent years) (e.g. Höpfner et al., 2019).

As pointed out by the reviewer, NH₃ has been observed in the upper troposphere in the Asian Monsoon region, leading to the formation of ammonium nitrate (NH₄NO₃) in the Asian Tropopause Aerosol Layer (ATAL). As stated by Höpfner et al., 2019, the origin of NH₃ is from surface sources, dominated by anthropogenic (agricultural) emissions in the recent decades. We have performed sensitivity tests with the EMAC model in which we remove the anthropogenic emissions of NH₃ on the surface, leading to a complete depletion of NH₃/NH₄⁺ in the upper troposphere in the Asian Monsoon region.

Reviewer #2 (Remarks to the Author):

The authors have significantly improved the quality of the manuscript when addressing the criticism raised by the reviewers. I am glad to recommend it for acceptance subject to a very minor revision as specified below. In case my suggestions are straightforwardly addressed, I don't need to see the revised paper.

We would like to thank the reviewer for their comments and recommendation.

1) The authors are reluctant to state that the work presents a rough estimate of the SN effect, but I insist that it should be stated that this work does not pretend to be a precise model of the SN effect on the Earth's environment. The reason is that, even though the atmospheric model is precise and accurate, the background assumptions on the GCR spectra and time evolution (as discussed in my previous review) are only order-of-magnitude estimates. In addition, as argued by Reviewer 1, assumptions on the low-oxygen atmosphere are also imprecise. Accordingly, while the general result that the overall SN effect is modest, is robust and undisputable, it is important to emphasise that this work cannot serve as a full model of the impact of a specific SN on the Earth system in its complexity. One sentence would be sufficient.

We have added a sentence in the Conclusions to emphasise this, as requested by the reviewer:

“The considered scenarios contain some simplifying, but justified assumptions, in particular regarding the low-oxygen atmosphere, which aim to represent the early Cambrian ozone layer but not the climate of the period. While the general result that the atmosphere and climate are not strongly perturbed by SN is robust, this work cannot serve as a model of specific SN impacts on the Earth system in their full complexity.”

2) The issue with the reference to Fields et al. (2019) is still not satisfactorily resolved. The Arxiv reference is no good. At least, it should refer to Bull. Am. Astron. Soc., 51, 410 (2019). Interestingly, I was unable to find that paper published in Astro2020. Moreover, this is a White Paper which produces no scientific results per se but just a wrap-up. The fact that nearby SNs are expected to occur once per few Myr was not the discovery of that White paper but is further referenced in research works.