

We thank the reviewers for their constructive feedback. In the responses below, the reviewers' comments are presented in black, while our replies are provided in blue. Text added to the revised manuscript is indicated in *italics*, with a red colour for emphasis.

Reviewer #1

Review of "Lightning in global chemistry-climate models: A review of parameterizations, evaluations, and future projections" by Krause et al.

This paper reviews the lightning parametrisations used in chemistry-climate models, along with the trends in their changing use, evaluations of skill, and the climate projections made in the literature.

I like this review. I think it is fairly straightforward but plenty of work has gone into it and it is valuable. More importantly, it is timely. Much development and evaluation of lightning parametrisations has happened over the low-earth orbit observation era that we've just had. Now we are moving into a geostationary satellite observation era for lightning, and this review takes stock of where we got to with those datasets. There is also increasing use of machine learning models, and this model takes stock of where (largely) traditional parametrisation techniques got to, given the data available. Finally, storm-resolving models on global and regional scales are now becoming useful for climate change projections. Whilst, they're unlikely to be used in chemistry-climate models soon, they can contribute to our understanding of how lightning will change in future.

All my comments are minor, even the major one. Once the authors have considered them, I consider this paper is ready for publication.

Yours sincerely,

Declan Finney

We are happy the reviewer likes our paper and are grateful for the detailed review.

(sort of) Major comment

Possible ways forward (or lack of) – This work is a useful stock-take, I just wonder if the authors have ideas for how to push its value further by proposing recommendations to the community of how to make progress? The article touches on machine-learning approaches, and on the new geostationary satellites, but doesn't provide any specific directions for how to overcome the large uncertainties in, not only projecting lightning changes, but even parameterising it in the present-day. I provide a few examples of the kinds of things that come to my mind, but I do not consider necessary to include any of these. If the authors don't have their own thoughts, then the paper can be published without enhancing the recommendations.

- It is great that Cecil2026 has developed a new OTD/TRMM-LIS/ISS-LIS dataset, which kind of is the final thing from LEO satellites for now. Great progress has been made because of the ease of use (i.e. gridded, well characterised, netcds) of the LIS/OTD data. I would say we need the same coherent datasets from the lightning-observing GEO satellites now. Something that merges the datasets into something that's easy to pick up and evaluate or create parametrisations with. It's ok if there's missing data – we now have the opportunity to get to relevant storm timescales (daily or 3-hourly). Evaluating at these scales allows us to properly start questioning the physics connecting the input variables and the lightning diagnostic.

We thank the reviewer for these great suggestions and agree that the development of coherent, well-characterized gridded datasets from geostationary lightning imagers represents an important next step for lightning parameterization development and evaluation. We have therefore substantially revised Section 3.1 to provide a broader overview of ground-based, LEO, and GEO lightning observations and to discuss their respective advantages and limitations. In particular, we now highlight the substantially higher temporal sampling of GEO instruments and the resulting opportunity to evaluate parameterizations at daily and sub-daily timescales relevant to convective systems. We also identify the development of coherent, multi-satellite GEO datasets as an important direction for future work. The revised text states:

“In contrast to LEO instruments, GEO lightning imagers continuously monitor a fixed portion of the Earth's surface, providing substantially higher temporal sampling and enabling observations at timescales relevant to individual convective systems. Examples include the Geostationary Lightning Mapper (GLM) on the GOES-R series over the Americas (Rudlosky et al., 2019) and the Lightning Imager (LI) on Meteosat Third Generation (MTG) over Europe, Africa, and adjacent regions (Bliznák and Sokol, 2026). Like LEO optical sensors, these instruments detect total lightning without directly distinguishing between CG and IC flashes, although efforts have been made to separate the two (Ringhausen et al., 2021). Their continuous observations provide new opportunities for evaluating lightning parameterizations beyond climatological spatial patterns, including the diurnal cycle and the timing and evolution of lightning associated with individual convective systems. GEO lightning observations are already being used for regional evaluations; for example, GLM observations have revealed biases in the spatial distribution and diurnal cycle of lightning simulated by CTH (Dang et al., 2025). However, the DE of GEO instruments is not spatially uniform across the GEO field of view and can decrease substantially at large viewing angles and toward its edges (Murphy and Said, 2020; Wu et al., 2023; Bliznák and Sokol, 2026). In addition, lightning detection is affected by variations in solar illumination associated with the day–night cycle and by the optical thickness of clouds above the lightning flashes. Differences in viewing geometry, instrument characteristics, DE, and data processing therefore need to be considered when combining observations from different geostationary sensors.

Both LEO and GEO optical lightning sensors primarily detect emissions near the 777 nm atomic oxygen line. However, observations at this wavelength do not capture all forms of electrical activity within thunderstorms. In particular, corona discharges, which comprise large numbers of streamers, produce little or no detectable emission at 777 nm and may therefore be missed by conventional space-based lightning imagers (Soler et al., 2020). Instead, their optical emission is concentrated near 337 nm and is accompanied by bursts of Very High Frequency (VHF) radio emission. Space-borne observations from the Fast On-orbit Recording of Transient Events (FORTE, Jacobson and Heavner, 2005) LEO satellite and the Atmosphere–Space Interactions Monitor (ASIM, Soler et al., 2020) have shown that such discharges can be detected from orbit through these optical or radio signatures. The planned Constellation of Radio Frequency Sensors (CubeSpark, Gatlin et al., 2023) aims to combine optical measurements at 777 and 337 nm with VHF observations, potentially providing additional information on thunderstorm evolution beyond conventional lightning detection (Soler et al., 2024).

All lightning observation systems thus have measurement and detection characteristics that can influence both the development and evaluation of parameterizations. These include differences in DE, sensor type and viewing geometry, temporal and spatial sampling, and the algorithms used to identify and group lightning signals into events, strokes, or flashes. For optical satellite sensors, detection is affected by background illumination, cloud optical thickness, and thresholds used to distinguish lightning signals from noise or cosmic-ray events, whereas LLSs are influenced by network geometry, signal propagation, and location algorithms. Consequently, observations from different

systems may differ in their spatial and temporal coverage, the types of lightning signals they detect, and their sensitivity to individual events or flashes, and are therefore not necessarily directly comparable across products. While the availability of coherent, well-characterized gridded LEO datasets has greatly facilitated the evaluation and development of lightning parameterizations, comparable multi-satellite datasets from GEO observations are an important remaining opportunity. In particular, their ability to resolve lightning at daily or sub-daily timescales could enable more direct evaluation of the physical relationships between environmental predictors and lightning activity and thus provide a stronger basis for developing and testing physically based parameterizations.”

- Something that I think can benefit projections is to break apart lightning intensity from storm occurrence. Finney2020 fig2 provides an example of this – I haven’t seen it very often in other climate-lightning studies. This can be done fairly easily in models by outputting one more diagnostic of lightning-days, or lightning-hours, as well as total monthly lightning. Then you can talk about lightning-days, lightning intensity (fl/day), and total monthly lightning. Again this gets down closer to the timescale that storms actually exist upon, even if not the process timescales. I think we’ll find lightning intensity more reliably increases with climate change across the world, and is tied to thermodynamic profiles and boundary layer conditions. Lightning-days is probably the complicated one, as it will depend more on synoptic patterns and global circulation changes. But pulling the metrics apart is a useful starting point for knowing how to build confidence in projections.

We thank the reviewer for this valuable suggestion and agree that separating lightning occurrence from lightning intensity could provide a useful framework for assessing projected changes in lightning. Following the approach illustrated by Finney *et al.* (2020), we have added this distinction to the discussion of future research directions. Specifically, we now highlight the potential value of evaluating the frequency of lightning-producing days or hours separately from the amount of lightning produced during those events. This would allow projected changes in total lightning to be decomposed into changes in storm occurrence and changes in lightning intensity, potentially providing a more physically interpretable basis for assessing projection uncertainty.

We agree that this decomposition could also help distinguish processes operating at different timescales. As the reviewer suggested, lightning intensity during individual events may be more directly related to local thermodynamic and boundary-layer conditions, whereas the frequency of lightning-producing events may additionally depend on synoptic-scale weather patterns and changes in atmospheric circulation. We have therefore added this approach as a promising direction for future model evaluation and projection studies, while noting that the relative contributions of these factors remain to be investigated systematically. We added this text to the Conclusions and outlook:

“In the meantime, extending existing lightning observations by new products from GEO satellites and ground-based LLSs will provide new opportunities to evaluate lightning parameterizations. The higher temporal resolution of these new products could also allow changes in total lightning to be separated into changes in the frequency of lightning-producing events and changes in lightning intensity, for example by considering the number of lightning-producing days or hours and the amount of lightning produced during these events (Finney et al., 2020). Such a decomposition could provide a more physically interpretable basis for assessing projected changes, as lightning intensity may be more directly related to thermodynamic and boundary-layer conditions, whereas the frequency of lightning-producing events may additionally depend on synoptic weather patterns and changes in atmospheric circulation. Together, these observational constraints could help identify the physical mechanisms underlying projected changes and thereby provide a basis for assessing confidence in different approaches to predicting lightning under future climate conditions.”

(If the reviewer did not intend these suggestions to be incorporated into the review, we would of course be happy to remove the respective passages.)

Minor comments

L9 – I see no requirement for chemistry to be present in a climate model in order to project lightning activity. I suggest you change this to “Climate models...” and add a sentence or two to explain that chemistry is one of the main motivations for inclusion of lightning params in climate models, and that much of the evaluation is done from that perspective. Therefore it’s useful to look at chemistry-climate models.

We appreciate the reviewer comment and agree that while lightning is often studied because of its effects on atmospheric chemistry, it is not a requirement. We changed the wording to “Climate models...”. We added the following sentence to section 2:

“While lightning itself does not require atmospheric chemistry to be simulated, lightning is often included in chemical transport models because of its impacts on atmospheric composition and chemistry.”

L13 – “times” seems a bit colloquial. I think either just list the two variables, or say the “the product of..”

We changed the wording to “the product of”.

L43 – Is “strong influence on...” “..climate” justified? The radiative effects resulting from lightning effects on ozone and methane are opposing, so I’m not sure how certain we are of the climate effect. Certainly atmospheric chemistry is strongly influenced.

The reviewer is right that the net climate impact may be quite small. We changed the sentence to:

“Lightning is an important natural source of nitrogen oxides (NO_x) in the middle and upper troposphere, an important precursor of ozone (O₃) and the hydroxyl radical (OH), thereby reducing the lifetime of methane (CH₄), and exerting strong influences on atmospheric chemistry, with implications for climate (Schumann and Huntrieser, 2007; Pickering et al., 2026).”

L57 – I find the “, while....” part of this sentence to be odd. As I understand it, this is not a caveat to the charge separation statement, but just one of the papers in the debate mentioned at the beginning of the sentence. Why just mention one paper in the debate? If you want to get into it, then I suggest you’ll need to give a couple of sentences to it. Or you could just include a number of citations at the point of “debate”.

Agreed. We decided to not go into the details and changed the sentence to:

“Although the precise mechanism responsible for lightning initiation remains an open question (e.g., Pasko et al., 2025), it is well established that lightning requires strong electric fields generated by charge separation within clouds (Lopez, 2016; Saunders, 1993).”

L59 – Whilst I am happy with the term “electrification”, I wonder if you would keep better connection to the earlier part of the paragraph to say “explanation for charge separation”

We changed the wording accordingly.

L127 – Need to check nothing missed from “parametrization” and “parametrisation”. I imagine WoS is smart enough to return them anyway, but you should check.

<https://trends.google.com/trends/explore?date=all&q=parameterisation,parameterization,parametrization,parametrisation> and

<https://english.stackexchange.com/questions/79847/parametrise-or-parameterise-a-curve>

Thanks for pointing this out. We added these search terms in our analysis and revised the text accordingly.

L132 - There is no need for them to be global. You can use regional models to project regional lightning changes. I think you might need to give a paragraph to tighten up your discussion of models. All kinds of climate model can be used to study this including GCMs and ESMs, and they may or may not include chemistry. Convection-permitting or cloud-resolving models on global or regional domains could be used (e.g. Romps2019 or Finney2020). So please have a think about how you want to describe what tools are used for projecting lightning.

While the reviewer is right that different models, including convection-permitting and regional climate models, can be used to simulate and project lightning, our review focuses on global lightning parameterizations and projections (see also line 296 in the original manuscript). We have now clarified this distinction and the chemistry issue in this paragraph:

“Global climate models, Earth System Models, and chemical transport models are our main tools for estimating future changes in global lightning activity. While lightning itself does not require atmospheric chemistry to be simulated, lightning is often included in chemical transport models because of its impacts on atmospheric composition and chemistry.”

L237 – Finney2018 supplementary fig S1 also shows that they attempted to account for shifting isotherms under warming climate.

Thank you for pointing this out. We revised the text:

“Finney et al. (2018) subsequently adjusted the sampling level from 440 hPa to 390 hPa for future climate simulations, to account for the expected rise of the tropopause and associated shift in the vertical temperature profile under warming. Romps (2019) later modified this scheme further by using the upward cloud ice flux at the 260 K isotherm (IFLUX_T), which lies within the mixed-phase region, instead of a fixed pressure level, arguing that a temperature-based reference is more physically appropriate for climate change simulations, where the height of cloud charge separation can shift with warming.”

L300 – I’d say this statement is already out of date but certainly won’t stand the test of time well. I think you’d be best acknowledging that convection-permitting models are becoming more feasible for use in climate projection (there is no reason that they have to be applied globally anyway). E.G. I don’t know if the ICON simulations had a lightning parameterisation but it would be lightweight so there’s no reason they couldn’t <https://link.springer.com/article/10.1007/s00382-026-08235-8> I don’t think you have to change your scope, just define it well. You could say that you are focusing on studies that have included a lightning param and a chemistry model. Then you just need to acknowledge that there could be higher resolution simulations in the near future, but that this is the state-of-the-art in CCMs.

We thank the reviewer for pointing this out. We changed the text to:

“However, while computational power has increased significantly in recent years and large-scale models are beginning to explicitly represent storm-scale convective circulations (Uribe et al., 2026),

running these models is still computationally expensive, especially for global, long-term climate simulations (Field et al., 2018). An alternative is so-called superparameterization, in which a two-dimensional cloud-resolving model is embedded within each grid column of a conventional climate model (Charn and Parishani, 2021). Nevertheless, summarizing all existing lightning parameterizations is beyond the scope of this review. Instead, we focus here on schemes that have been applied and evaluated in global-scale modelling studies using models that do not explicitly resolve convection.”

Section 2 – this is lengthy, I think dividing it into 2-3 subsections would be pleasant. Perhaps one for CTH-related params, one for other non-microphysical params, and one for microphysics-based params. But whatever is fine, just break it up a bit.

We agree. We have substantially reorganized Section 2, also taking into account feedback from reviewer #2. Following these questions and suggestions, we divided parameterizations into four subsections based on the physical basis of the parameterizations rather than their chronological development:

- Cloud-depth-based parameterizations, covering CTH, its modifications, and CCD
- Convective intensity-based parameterizations, covering approaches based on precipitation, mass flux, and CAPE
- Microphysical parameterizations, covering approaches that explicitly represent ice-phase processes
- Multi-predictor statistical and machine-learning parameterizations, covering approaches that use multiple environmental variables and machine-learning approaches. Here, we now also describe two own parameterizations (ENV_XGB and ENV_Linear) which were introduced in a recently accepted paper (Krause *et al.*, accepted).

This restructuring reduces the impression of a chronological order and provides a clearer progression from empirical proxies of convective development toward parameterizations that represent increasingly specific physical controls on lightning.

L335 – “...though it doesn’t distinguish between whether a flash is CG or IC” or something like that? Current sentence might imply the satellite measurement sees them as different

We made this clearer in the revised text:

“Because the 777-nm emission is produced by both CG and IC lightning, these instruments observe total lightning activity without directly discriminating between the two flash types.”

and in the GEO paragraph

“Like LEO optical sensors, these instruments detect total lightning without directly distinguishing between CG and IC flashes, although efforts have been made to separate the two (Ringhausen et al., 2021).”

L350 – I feel like this sentence is leading to a “..and therefore...” there is not high confidence in their ability to simulate such trends?

We agree with the reviewer and added the following statement to the end of the sentence:

“... leaving the ability of models to reproduce such trends poorly constrained.”

L369 – “Related to...” sounds odd here. The extratropical underestimate introduced in the previous sentence is not really to do with this intra-tropical differences. I think you mean “The tropical

underestimation occurs primarily over central Africa, with compensating overestimation from the Amazon and maritime continent”?

We meant that the tropical overestimation is primarily driven by the overestimation over the Amazon and the Maritime Continent. In contrast, lightning over Central Africa is underestimated by all parameterizations and therefore does not contribute to the tropical overestimation. We made this clearer in the revised text:

“A common bias across many schemes, including CTH, is the overestimation of lightning in the tropics and underestimation in sub- and extratropical regions (Finney et al., 2014; He et al., 2022; Ghosh et al., 2025). Within the tropics, the overestimation is particularly pronounced over the Amazon and the Maritime Continent, whereas lightning over Central Africa is underestimated by all parameterizations.”

L551 – I don’t know if you want to comment on potential reason for this insensitivity but I would hypothesise that it is heavily determined by the tropopause shift, which is reasonably well understood radiative response. Given it is a fairly direct response (at least in the tropics) to GHGs/water vapour, it probably is fairly similar across models. I suspect it’s something that is checked a lot for whether it sensible, and may well be part of the model tuning process. The cloud top height baseline may also be something that’s tuned to observations across models in the present-day simulations – that well- established baseline might also help achieve similar projected changes in CTH/lightning.

We thank the reviewer for these thoughts. We agree that providing a potential explanation for the relatively similar sensitivity across climate models and scenarios is desirable given the strong nonlinear dependence on CTH. We have now added a brief discussion of a possible physical explanation:

“This apparent consistency may partly reflect the relatively robust response of tropopause height to warming (Santer et al., 2003), which may contribute to similarities in projected changes in CTH, particularly in the tropics.”

L600 – “presumably” – If you don’t feel confident to make a robust statement, then I would change this to “for a more complex, 2-moment scheme”.

We changed the wording accordingly.

Technical comments

L107 – “sensitivities” typo?

Indeed, fixed.

L377 – “intermediate”?

Good catch! Fixed.

L534 – “sensitive” typo?

We changed the sentence to:

“Whether CG lightning responds more strongly to climate change than total lightning is uncertain as the few studies that reported changes for both lightning types (all computing CG lightning from CTH-derived total lightning multiplied by the CCD-derived CG fraction) came to different conclusions.”

Literature

- Finney D L, Marsham J H, Wilkinson J M, Field P R, Blyth A M, Jackson L S, Kendon E J, Tucker S O and Stratton R A 2020 African Lightning and its Relation to Rainfall and Climate Change in a Convection-Permitting Model *Geophys Res Lett* **47**
- Krause A, Gregor K, Qu Y, Veraverbeke S and Rammig A accepted Large Projected Increase in Cloud-to-Ground Lightning Under Future Climate *Earth's Future*