This manuscript reviews the parameterization schemes used to represent lightning flash rates in global chemistry-climate models. It introduces 16 schemes and summarizes their evaluation against observations and their projected future changes. This work is mainly a summary of previous studies and includes little new analysis. It would be a useful paper for newcomers to learn the current status of lightning parameterization research and to find the relevant earlier work, but it lacks in-depth insight or outlook. Overall, it falls within the scope of ACP and serves as a supplement to the classic review by Schumann and Huntrieser (2007) from nearly 20 years ago. Before it can be considered for publication, the following comments need to be addressed.

Major comments:

1. Lightning flash observations play an important role in this review, as they serve as the ground truth against which the model parameterizations are evaluated (Figure 1). However, the manuscript does not describe these datasets in sufficient detail. Please provide a proper introduction to at least the OTD/LIS climatology and the FLASHMAP dataset used in Figure 1: how each dataset is constructed and validated, and what their main limitations are. In addition, please explain why these particular datasets were chosen for Figure 1 rather than the other products introduced in Section 3.1.
2. Section 2 reads as a chronological catalogue of equations rather than a synthesis, and it lacks the physical reasoning that should tie the schemes together. The section lists one parameterization after another largely in time order, but it does not explain the underlying physics of lightning occurrence or how the proxy in each scheme is connected to that physics. In particular:
    - Why was CTH chosen to represent flash rate in the first place, and what is the physical justification for the proxy variable used in each of the other schemes?
    - What drives the distinct lightning characteristics over land versus ocean? Why do some schemes capture this land–ocean contrast while others do not, and why do some schemes fail to reproduce it even when separate coefficients are used for land and ocean (see Section 3.2, Fig. 3c)?
3. Section 2: For each parameterization equation, please briefly explain how it was derived — based on what theory or dataset, and from what period. Simply stating that the authors "came up with an equation" (e.g., Line 165) is not sufficient.
4. Section 2: When introducing variants of existing schemes, please explain why

each modification was considered necessary and how much it improved on the original, ideally with quantitative numbers. For example, He et al. (2022) modified the Lopez (2016) scheme based on findings from McCaul et al. (2009): what were those findings, and how does the modified scheme perform relative to the original?

5. Section 3.1: Geostationary satellite instruments for lightning detection should also be described.
6. Section 3.3, Lines 442–444: Why does the seasonal performance of the different schemes depend on their ability to simulate the land–ocean contrast? Please explain. Isn't it rather a matter of how well the proxy variable represents lightning activity across seasons, and of whether the model can reproduce the seasonal cycle of the proxy itself?
7. Section 4.1 and Abstract: for the values 4.4±7.6% and 7.5±7.9% K^{-1}, what does the standard deviation represent? I would suggest not reporting the average over all projections, since — as the authors themselves note — this value is heavily biased toward CTH. Instead, please report a separate average for each scheme, with the standard deviation reflecting the spread across different models or scenarios.

Minor comments:

8. Abstract, Line 25: do these values represent global-mean changes?
9. Introduction: consider removing the subsection titles. They are more of a distraction than a help, and read like a ChatGPT-suggested edit.
10. Introduction, Lines 75–76: on what reference or dataset is this statement based?
11. All equations should be numbered.
12. Section 2, Lines 151–152: please provide the reference and briefly explain why this scheme is less widely used.
13. Section 2, Lines 152–153 ("To account for the large differences in hypothesized updraft velocities"): this seems to imply that updraft velocity is the more direct proxy for flash rate, and that CTH is used only as a proxy for updraft velocity. Since CTH cannot capture the land–ocean difference in updraft velocity, separate equations are then needed for land and ocean. Is this the intended reasoning? If so, please state it explicitly

14. Section 2, Lines 169–171: are the authors suggesting that the original CTH scheme is inconsistent with theoretical expectations or storm observations? Which expectations and which observations are meant here?
15. Section 2, Line 161: what kind of spatial calibration factor? Please specify.
16. Section 2, Lines 163–164: what method, and for what purpose? It is difficult to tell what Stolz et al. (2021) actually contributed here.
17. Section 2, Line 210: typo — "lightsning" should be "lightning".
18. Section 2, Lines 211–219: are the authors suggesting that Yoshida et al. (2009) consider cloud depth a better proxy for flash frequency than CTH? What is the theoretical basis for this?
19. Section 2, Line 218: what does F_MC denote?
20. Section 2, Line 275: what is meant by a "two-dimensional" deep learning framework?
21. Section 3, Line 336: detection efficiency also depends on time of day (daytime vs. nighttime observation). Please add this.
22. Section 3, Line 340: where does the estimated CG fraction come from? Please provide references and details on the assumed CG-fraction distribution and its justification.
23. Figure 3: what do the labels T1, EC, ZH, ZHW, B1, 1-m, and 2-m mean? Please clarify.
24. Section 3, Line 455: "NH South America" — this appears to be a typo. Please check.
25. Section 3: other studies have examined monthly time-series / seasonal / diurnal performance using geostationary instruments (e.g., Wu et al., 2023; Dang et al., 2025). Please consider including them.
26. Section 3, Lines 498–499: please provide quantitative numbers to support the claim of "impressive performance" by the machine-learning approaches.
27. Section 3, Lines 503–505 and Figure 5: it is difficult to follow how this analysis relates to the reliability of the schemes. Please explain the reasoning more explicitly.

References:

Dang, R., Jacob, D. J., Wang, H., Nowlan, C. R., Gonzalez Abad, G., Chong, H., . . . Liao, H.: High-resolution geostationary satellite observations of free tropospheric NO2 over North America and implications for lightning emissions, Proceedings of the National Academy of Sciences, 122, e2510535122, doi:10.1073/pnas.2510535122, 2025.

Wu, Y., Pour-Biazar, A., Koshak, W. J., and Cheng, P.: LNOx Emission Model for Air Quality and Climate Studies Using Satellite Lightning Mapper Observations, Journal of Geophysical Research: Atmospheres, 128, e2022JD037406, https://doi.org/10.1029/2022JD037406, 2023.