

Earth Syst. Sci. Data Discuss., referee comment RC2
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Comment on **essd-2022-88**

Anonymous Referee #2

Referee comment on "World Wide Lightning Location Network (WWLLN) Global Lightning Climatology (WGLC) and time series, 2022 update" by Jed O. Kaplan and Katie Hong-Kiu Lau, Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2022-88-RC2>, 2022

This paper describes the addition of 2021 data to the ongoing, multiyear WGLC global gridded lightning climatology, which is based on the detection-efficiency-corrected Worldwide Lightning Location Network (WWLN) stroke-level measurements. I reviewed the files discussed by the paper, as well as the paper and supplementary information. The files are accessible, readable, and mostly Climate & Forecasting (CF) compliant. (There were a couple errors when using an online CF checker, but the errors appear to be related to CF's known deficiencies with respect to lightning datasets.)

I have the following comments about the paper:

- WGLC as an acronym is evidently not defined in the paper.

- Figure 4 does not provide convincing evidence of a relationship between total solar irradiance (TSI) and WGLC stroke power. I recommend striking this figure and related discussion unless the authors are prepared to present quantitative statistical analysis to

back up their inferences.

- Lines 70-75 discusses some studies that “suggested that these new fuel standards would lead to significant reductions in marine PM2.5 emissions”. The cited Zhang study simply predicted reduced PM2.5 but did not demonstrate it. The cited Sofiev study again simply makes PM2.5 predictions and does not demonstrate that they actually occurred. The cited Wang study claimed a ~30% reduction in PM2.5 emissions during 2016-2019. This is prior to the claimed 2021 decrease in lightning. Do the authors see reduce lightning in shipping lanes between 2016 and 2019? If not, their claim in this section is a bit dubious.

- Along the lines of the above two comments, the paper is quick to make suggestions about TSI and pollution modulating lightning, but mostly elides discussion of notable interannual changes in precipitation patterns or major storms. For example, one could imagine comparing to the Global Precipitation Measurement (GPM) climatology for 2021 v. previous years in order to gain further insight into why lightning changed between 2021 and previous years, but the paper doesn't do this. Why not?

- Despite the focus on pollution in shipping lanes, apart from a citation and brief description of the Liu study (which has not been peer-reviewed), the paper does not really take a stand on whether lightning changed during the COVID lockdowns. That's fine, but why not, if the paper was willing to suggest that lightning decreased due to reduced ship emissions?

Ultimately, the updated dataset (which is the most important thing) is fine. The associated paper is not rigorous enough to support the many inferences about pollution and TSI modulation of lightning.

