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Reply on RC2

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Author 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-AC2, 2022

In the reply below, the reviewer's comments are in *italics* **and our reply is in** plain text**.**

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.)

We thank the reviewer for their detailed investigation of our manuscript and data files.

I have the following comments about the paper:

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

We apologize for this oversight and will fix in the revised version.

- *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.*

In our 2021 paper presenting the original WGLC dataset (Kaplan and Lau, *ESSD*, 2021), we discussed interannual variability in both lightning occurrence and stroke power. We noted then that previous studies suggested that lightning occurrence could be related to climate variability (e.g., ENSO, QBO) but that we could not see any relationship between the WGLC and indices of climate variabity. On the other hand, we noted that the interannual variation in stroke power was qualitatively similar to total solar irradiance (TSI; see Fig. S12 in Kaplan and Lau, 2021), and cited several papers that explained how the two quantities (stroke power and TSI) could be related. We noted in the 2021 paper that it would require a longer timeseries of lightning observations to confirm any possible link between stroke power and TSI. With our extended dataset described in the current manuscript, we felt that it was helpful to follow up on our original analysis. While we feel that an extensive quantitative investigation of the relationship between stroke power and

TSI is beyond the scope of this short communication and probably still precluded because the lightning record is still too short, we are also hesitant to remove the figure and short discussion, because we feel that this is a helpful follow up and extension from our previous publication. To respond to this comment, we suggest that we provide a slightly longer discussion on the power:TSI relationship and include several references that support the idea that these quantities could be linked. A more detailed investigation should be the subject of a separate study, and we will also note this in our revision.

- *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.*

To address this comment we suggest that we present a short repeat of the analysis of Thornton et al. (2017) who first proposed the relationship between pollution and lightning in South and Southeast Asia and see if there is any apparent reduction in lightning in the important shipping lanes after 2016.

- *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?*

As our current manuscript is just a summary of an update to an existing dataset, we feel that a detailed investigation of the drivers of interannual variability in lightning is beyond the scope of the paper. Furthermore, precipitation by itself is not a driver of lightning, but rather a covariate under certain meteorological circumstances. Still, it would be valuable to investigate these relationships between atmospheric circulation, precipitation, and lightning in future studies, and we hope that our publication of a free, global lightning dataset may stimulate just that. We do include the short discussions on TSI and pollution because these follow up on statements we made in our previous paper introducing the dataset and so we feel that revisiting these ideas with our extended dataset is a good continuation of our previous observations.

- *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?*

We did not observe any immediately obvious evidence of COVID lockdowns influencing lightning, but will highlight that the WGLC would be an interesting starting point for a more detailed study.

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.

We appreciate the reviwer's detailed evaluation and recognition that our dataset is sound and that publishing this update manuscript is therefore justified. While we don't

completely agree with their assessment that including some supplementary observations on TSI and pollution and their link with lightning is not valuable, we will try to better explain in this short communication why we want to include these, principially as a continuation of ideas that we raised in the parent paper that presented the original dataset.