

Comment for manuscript amt-2021-289 on behalf of one of the reviewers.

Dear authors,

one of the reviewers communicated with me and sent some follow-up comments regarding the revised version of your manuscript. I post them here in the public discussion because they seem appropriate for the manuscript's public record. I encourage you to post a point-by-point response when making edits for the final version of the paper.

Thank you,

Sebastian Schmidt (editor)

Comment by reviewer in response to the revised version, and also in response to AC3 (<https://doi.org/10.5194/amt-2021-289-AC3>)

The authors could have done more to address my initial feedback, addressing the following clarification comments would be greatly appreciated.

It is still unclear to me if the paper is a validation paper of the NOAA STAR TOA SW flux product and if so the dataset and version number should be properly cited. If the GOES SW TOA flux product is being produced by NOAA it should be cited. If it is not, then it should also be stated in the text. If this is an algorithm paper of a potential NOAA product that is in development that should be clearly stated.

3.1 Satellite data for GOES-16 and GOES17: datasets are used in papers I expect the product name, version number and location should be given. I find section 3.1 completely lacking in this regard. First of all, I searched for <https://www.bou.class.noaa.gov/> and the site could not be found. I do not know if this is the GOES L1b radiance data, since the product name was not given in the text. The text mentions that "The CODC data are not always available from CLASS". Could the authors provide the name and version of the product of the cloud retrievals used in this study. Lastly the GOES based TOA flux dataset or product promoted in this paper is not cited in the paper.

3.2 Reference data from CERES: This section is completely confusing. Some of the figures were used from CERES SSF L2 and for fig. 9 the CERES FLASHflux level 2. Again, the edition numbers were not cited. I believe it was CERES SSF L2 Edition4 and FLASHflux Version 4A. This is extremely important if someone wanted to recreate the results in the future when the CERES project may have moved on to Edition 5.

I was disappointed that only a few overpasses were validated in the paper and here is the response from the authors. "The ABI is at 5 min intervals. However, we want to compare four products simultaneously. It is hard to find cases when all of the GOES-16, GOES-17, CERES/Terra and CERES/Aqua have overlap in time and that the overlap is large enough to compare all of them." For me, there is no stipulation that they need to be validated simultaneously in order to have a robust validation matched dataset.

I looked at the ESMF re-gridding web site, there are multiple grid type options. Could the gridding algorithm just be simply detailed in the text.

The point of the paper is that the CERES and GOES surface types could be a factor. The Su et al. 2015 ADM type are more a function of NDVI over land and not strictly dependent on IGBP type and that NDVI allows for seasonal variability, whereas the GOES (this paper) has a static surface type categories not allowing for seasonal variation of interannual variability.

Line 389. The “ground truth”, namely, the CERES observations are also undergoing adjustments and recalibration, is misleading. The CERES SSF L2 TOA flux observations have been using consistent algorithms and instrument calibration across a CERES edition (not FLASHflux). That is a new edition is reprocessed from the beginning of record with consistent algorithms and calibration. That is why citing datasets is so important.

In the abstract the last sentence states: A satisfactory agreement between the fluxes was observed for both clear and cloudy conditions and possible reasons for differences have been identified.” Satisfactory agreement is a relative term. I believe that the authors need to describe who their users are and that the level of agreement is sufficient for their applications.