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Interactive comment

Interactive comment on "Gas flaring activity and black carbon emissions in 2017 derived from Sentinel-3A SLSTR" *by* Alexandre Caseiro et al.

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Review of "Gas flaring activity and black carbon emissions in 2017 derived from Sentinel-3A SLSTR" The paper attempts to locate all the active gas flares of 2017 and estimate their flare gas volumes using nighttime data collected by the Sea and Land Surface Temperature Radiometer (SLSTR) instrument flown on-board the Copernicus satellite Sentinel-3A. The basic detection algorithm for the individual nights of data follows the VIIRS nightfire (VNF) method and appears to be solid. But the steps used to go from the individual nights of data to the annual summary are questionable and should be revisited: 1. The SLSTR results found 6232 flaring sites in 2017. This compares to over 10,000 flares reported by the VIIRS nightfire team for 2017 (https://eogdata.mines.edu/download_global_flare.html). 2. Many flares are intermit-

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tent. The nightly flare detection data does not contain sufficient information to account cloud and solar contamination effects that could effect the annual flared gas volume calculation. Hence the annual characterization of flared gas volume should calculate the “duty cycle” or “percent frequency of detection” for each flaring site. The VNF team makes the calculation based on flare detection numbers in the set of nighttime cloud-free observations made of the site during the year. Because the nightly VNF product only contains the detections – the annual analysis includes an inventory of the cloud state (cloudy or clear) for the nights lacking VNF collection that are free of solar contamination. The VNF method excludes both sunlit and cloudy observations in the calculation of flaring site duty cycle. The method reported in this paper (section 2.3 and Figure 2) is woefully inadequate and appears to have resulted in a drastic under-estimation of annual flared gas volume in Russia. I suspect that the method in Figure 2 does not account for solar contamination outages during summer months – as shown below with VNF for a flare in northern Siberia.

3. The paper lacks detail on the method used to discriminate clear versus cloudy observations. In addition, the paper makes several assertions that should be rechecked: 4. The paper makes several claims that the “SLSTR-based methodology is able to detect smaller gas flares”. No evidence is presented to back up this claim. 5. The paper state that the VNF product only uses a single shortwave infrared channel. This was the case for early VNF data. However, from January 2018 forward VNF from two satellites has included two SWIR bands. My recommendation is that the paper undergo major revision and a second round of peer review prior to publication. The authors should make a specific comparison against the VNF product from 2017 to better understand difference between the SLSTR and VIIRS flaring sites and flared gas volumes. Are there specific geographic regions where one system detects more flares or more flared gas volumes? Since the instruments and detection algorithms are so similar, the authors should figure out the reason behind the discrepancies. To make a direct comparison of the combustion source detection limits with VNF, the authors can follow the methods outlined in https://www.mdpi.com/2072-4292/11/4/395. This

crosschecking with VNF could lead to major improvements in the gas flaring results from SLSTR and a far better paper.

Please also note the supplement to this comment:
https://www.earth-syst-sci-data-discuss.net/essd-2019-99/essd-2019-99-SC1-supplement.zip

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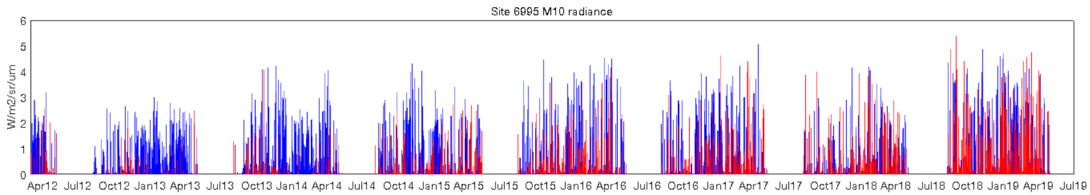

Fig. 1. Temporal profile VNF SWIR radiance for a flare in Siberia. Each year there is an outage period due to solar contamination.

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