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Geoscientific Model Development Discussions

Interactive comment on "Development of a new gas flaring emission data set 1 for southern West Africa" *by* Konrad Deetz and Bernhard Vogel

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Dear Mikhail Zhizhin (Referee, Geoscientific Model Development),

thank you for your reviewer report from 14 October 2016. We have accounted for the comments and suggestions in the revised manuscript version. Please find our replies to the particular comments in the following. We have uploaded a revised manuscript which also includes the revision regarding the comments and suggestions of the first reviewer.

Sincerely Konrad Deetz and Bernhard Vogel

Referee comments:

In the paper a new method to model emissions from gas flaring is developed and

validated on oil fields in Western Africa. The paper is a substantial contribution to the modeling science, and the approach is valid and motivating for further research. I have some comments on the presentation and details of the method which could be considered by the Authors before it is published.

0. I would recommend changing abbreviation VNP (VIIRS Nightfire Product) to commonly used VNF (simply VIIRS Nightfire) in the manuscript.

We agree on that and have changed the manuscript accordingly. For the general VIIRS Nightfire (including all combustion sources) we use the abbreviation "VNF" and for the extracted flaring information from VNF we use the abbreviation "VNFflare".

1. Formula (1) derives gas flow rate from flare radiative heat and temperature measured from satellite. It is a basis of the proposed model. However, it is taken from Appendix of regulating document by the German Environmental protection agency. This is technical, not scientific source. The derivation of the formula is not provided neither in the paper under review, nor in the cited document. The cited document has no source for the formula either. It is important to derive the formula (1) or to provide a scientific reference.

We agree on that. TA-Luft is a technical document and the equation is not well introduced there. Equation 1 of the manuscript originates from VDI 3782, 1985: Dispersion of Air Pollutants in the Atmosphere, Determination of Plume rise, Verein Deutscher Ingenieure, VDI-Richtlinien 3782 Part 3, Equation 24, https://www.vdi.de/richtlinie/vdi_3782_blatt_3-ausbreitung_von_luftverunreinigungen_in_der_ atmosphaere_berechnung_der_abgasfahne (accessed: October 17, 2016). We changed the citation accordingly. Although VDI 3782 (1985) is also a technical document, the derivation of the equation becomes clear. The heat flow M in MW is given by equation 1

M=c_p F (T_S-T_A), (1)

where F is the flow rate in m3 s-1, c_p the mean specific heat capacity of the emissions, T_S the source temperature and T_A the ambient temperature. VDI 3782 (1985) provides a value of the mean specific heat capacity of

c_p=1.36 10ˆ-3 MW s m-3 K-1

which is derived for a pit coal firing but VDI 3782 (1985) denotes, that this can be used for other flue gases as well since potential deviations are negligible. (An explicit c_p value for gas flaring is not provided in the literature.) For the ambient temperature T_A we use 298.15K as a fixed value, representative for the tropical region. Within a sensitivity study regarding the influence of T_A on F we have used the mean heat flow and the mean source temperature of all flares in TP15 and varied the ambient temperature between 293K and 303K, as a reasonable temperature range in the tropical regions. The resulting maximum difference in the heat flow is 0.0036 m3 s-1. Therefore we assume the errors using a fixed climatological value for the ambient temperature are negligible, but of course the user has to adapt the ambient temperature to the region he wants to apply the inventory. We have emphasized this in the manuscript. By using equation 1, the value for c_p and for T_A, the flow rate F in MW is given by:

F=M/((1.36 10ˆ-3 (T_S-298.15)). (2))

2. Flare temperature used in the formula (1) is taken from instantaneous satellite measurement (VNF). It has a large variance depending on atmospheric conditions etc. I would recommend using mean flare temperature averaged over all cloud-free detections.

We see your point. This leads to a further source of uncertainty, because we cannot decide whether the spatial source temperature variations really results from the sources or from the atmospheric conditions. We think that this problem does not affect the climatological approach (Eclim in the revised manuscript) because for every detected flare the source temperature already is averaged over the two-month period of TP14 or TP15 before we calculate the emissions. We assume that this is a compro-

mise between robustness and keeping the spatial variability of the flaring. To allow for consistency we now also use these temporal averages of source temperature and radiant heat for the daily resolved inventories (Eobs and Ecom in the revised manuscript). Therefore all three inventories have the same underlying emission field and the difference is just related to number of flares that are active at a certain day. For Eclim all flares are active at once, for Eobs only the actual observed flares are active and for Ecom the actual observed flares + the cloud covered flares (taken as active) are considered (com=combination). Nevertheless we have also included a further inventory in Tab. 5 that uses instantaneous input data to derive Eclim (first calculating the emissions for every single observation and then averaging the emissions temporally). This is given as “Eclim, instant. input” and should allow further insight in the sensitivity/uncertainty.

3. The number 283 used in the formula (1) I believe stands for ambient air temperature at night? Is it a proper climatological value for Wester Africa?

Yes, the 283 refers to the ambient temperature. We agree that this value is not appropriate for the tropics. We have changed this value in the manuscript to 25°C (298.15K, also described in Comment 1 of this document). Owing to the change of the ambient temperature we have repeated our analysis to be consistent with this new value. The change from T_A=283K to 298.15K lead to a slight increase in the emissions (e.g. for Fig. 9b the spatially integrated SWA emissions of TP14 increase from 651 to 658 t h-1).

4. Comments 1-3 may result in a wider variance of the proposed model output, and the model sensitivity analysis should be presented.

Regarding the ambient temperature (Comment 3) we have presented the maximum uncertainty in the heat flow as 0.0036 m3 s-1, which is also described in the manuscript. For the mean heat capacity of the emission c_p we do not have further information to assess the uncertainty. For considering the uncertainty in using temporal averages of source temperature and radiant heat instead of the instantaneous satellite observations, we have added a further emission inventory in Tab. 5 (“Eclim, instant. input”, for TP14 and TP15).

5. The Authors have made a considerable effort to take into account cloud conditions which can mask flare observations from space. Why not to use only cloud-free observation days, and to count detected/not detected flare cases to derive mean radiative heat?

By using the postprocessed flaring data (VNFflare in the revised manuscript which includes also a cloud mask) instead of the “Flaring only” product, it is straightforward to separate the flares into the categories (a) “cloud-covered”, (b) “cloud-free and inactive” and (c) “cloud-free and active”. By assuming that the cloud-covered flares are active with their mean emission strength, we can estimate the daily emissions via the sum of (a) and (c). To use only the cloud-free observation days would be problematic because SWA is a region with very extensive cloud cover (on average approx. 70% in the flaring area).

I would like to acknowledge that the Authors provide software sources and input data used in the study as the paper supplement. It is helpful for reproduction and reuse of their science and model.

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