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Interactive comment on "A spatially downscaled sun-induced fluorescence global product for enhanced monitoring of vegetation productivity" *by* Gregory Duveiller et al.

Anonymous Referee #2

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This manuscript by Duveiller et al. presented a new SIF dataset that is developed based on a previously published method (Duveiller et al., 2016 RSE). In this manuscript, the results from combination of multiple input variables were compared, as well as training against two GOME-2 SIF dataset (PK or JJ). The authors used OCO-2 SIF and TROPOMI SIF as reference for the comparison. The manuscript is clearly written, and the updated dataset seems to improve to some extent as compared to the previous one with extended temporal coverage.

However, I do have some comments for the authors to consider and possibly improve this dataset. 1. The SIF light use efficiency model: I have two concerns for this model.

First, SIF has a unit of energy flux, and in LUE models, the energy input is also an important variable. This model developed by the authors does not include an energy flux term, e.g. PAR. This could have limited effects if the authors assume that the cloud cover is homogenous within each local spatio-temporal window, but how much confidence do we have for this prerequisite should be discussed. Second, the authors used a sigmoid function of ET or NDWI to assess the water stress on vegetation, to me, this is problematic. The changes in ET or NDWI is strongly affected by the vegetation status, i.e., vegetation coverage or vegetation index. For example, during the green-up period, both ET, GPP enhanced as a results of vegetation greening. However, lower ET or NDWI values in the earlier period does not indicate that vegetation is more water limited. Normalization is necessary to use these variables to assess water stress. 2. As a journal specifically targeted at publishing dataset, I would suggest the authors provide enough details in the method for generating this SIF dataset. For example, in the last paragraph of section 2.4, how does the eigen decomposition work is not clear. The spatial and temporal window sizes are also not informed. Although the original method is described in details in a previous publication, since this journal is a data journal, the readers should gain enough understanding of the dataset without referring to other papers. Otherwise, this paper is more like an addendum or update to the previous paper. 3. The author mentioned that the dataset has spatial and temporal gaps in some areas due to the missing values for the GOME-2 SIF or the predictor variables. Would there be a method to solve this issue? The author mentioned about the potential usage for this dataset, however, the gaps would limit these potential applications. 4. The JJ SIF dataset shows an abnormal decreasing trend due to the sensor degradation (Zhang et al., 2018), how about the PK dataset? Since the downscaling are based on these two datasets, this needs to be further discussed. How does the algorithm deal with this issue, if the problem still exists, this needs to be informed and the users should be cautious for trend analysis using this dataset.

Below are some detailed comments: P3 L2 "land science"-> "Earth science"? P6 L2, it would be good to explain the meaning of these b parameters a little bit, it will better help readers understand the ranges used in Table 1. P8 L4, do you have any references to support this? The two instruments should be exactly the same. P8 L5, why only on PK data? What about JJ data? P10 L30: I don't think so, this is just a high-resolution SIF dataset, it cannot be compared directly with TROPOMI SIF, for example, you cannot use downscaled SIF for year 2017 and compared with TROPOMI SIF for 2018 to detect changes.

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