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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.

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To ease the reading we will provide below first the remarks of the reviewer in *italics* and indented, following by our response in normal font typeface.

> *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.*

A good reason why PAR is not included directly is that there are no direct estimations of surface in-coming PAR derived from MODIS products that are ready to be used for downscaling. In a way, the model actually does include indirectly a proxy for PAR to some extent by the intermediary of the LST, which should be highly correlated to PAR. But we agree that including PAR is some way would be a nice improvement that we will discuss.

Regarding the homogeneity of the cloud cover within the window, we could argue that in principle all processing is based mostly on cloud-free observations, as that is when satellite instruments can sample the ground. However, we know that the products we use have different capacities in detecting (and filtering) clouds for various reasons: MODIS has a finer resolution and thus can see smaller clouds, the SIF retrieval is less sensitive to cloud cover, the platforms have different orbit passing times, and thus are sensitive to different clouds. As suggested by the reviewer, we will discuss all of this in the revised manuscript.

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.

We agree entirely with the reviewer: ET and NDWI are strongly affected by vegetation status, and the low values in Green-up do not have the same meaning as the same low values during senescence for instance. But it is precisely because of this that the downscaling model is calibrated at every time step separately and independently, based on locally adjusted constraints. As a result, the NDWI and ET are effectively normalized as suggested by the reviewer. We will ensure that we stress this better in the revised manuscript.

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.

The eigen decomposition is actually not part of the original downscaling approach, but rather part of the use of the index of agreement. This is a very technical procedure that

would considerably overload the text and that is not necessary for the actual downscaling that is reported in the present data descriptor. As this decomposition is fully explained in the supplementary material of the dedicated paper (Duveiller et al. (2016) Sci. Reports.), which is in full open access, we think it is best that we redirect readers specifically to that document (i.e. section 5 of the supplementary information of that paper) instead of repeating everything here.

Regarding the operations specifically related to the downscaling, we will revise the text to ensure all necessary information is there. However, mostly everything should already be there. For instance, regarding the spatial window mentioned by the reviewer, this is already specified on page 6 line 4: "... using an adaptable spatial moving window containing the 40 nearest observations around the central pixel".

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.

We already dedicate a paragraph on discussing how the gaps could be filled in the current version of the manuscript (see page 11, lines 5 to 11).

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.

The trend mentioned by Zhang et al. (2018) should affect both JJ and PK datasets in the same way. We will mention this in the revised manuscript. Regarding how our algorithm deals with it, basically, the way our downscaling is parametrised (i.e.

individually at every separate time step), the trends in the input SIF data should be reflected in the downscaled SIF data. Therefore, this is a problem of the GOME2 data in general, not specifically of our downscaled SIF product. A warning about this will be added in the revised manuscript.

Below are some detailed comments:

P3 L2 “land science”-> “Earth science”?

Changed

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.

We will add some more details on how these parameters can be interpreted.

P8 L4, do you have any references to support this? The two instruments should be exactly the same.

The instruments are the same but they are on different platforms (Terra vs Aqua) that are on different orbits (descending vs ascending) that have been in space for different amounts of time (since 2000 and 2002 respectively) and thus differently exposed to sensor degradation. All these differences can be reflected in the data. Regarding the specific point of degradation we will add the following reference:

Sayer, A. M., et al. "Effect of MODIS Terra radiometric calibration improvements on Collection 6 Deep Blue aerosol products: Validation and Terra/Aqua consistency." Journal of Geophysical Research: Atmospheres 120.23 (2015): 12-157.

P8 L5, why only on PKdata? What about JJ data?

These tests are relatively expensive from the computational side, as the entire

dataset needs to be downscaled for three years everytime that a new combination if parameters is tested. We decided that, given the likelihood that the choice of the LST may be so relevant to improve the downscaling procedure, and that the TERRA observations may be of lower quality, we would look at this issue only for one of the two datasets. We will reclarify this in the text.

P10 L30: I don’t think so, this is just a high-resolution SIFdataset, 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.

If the actual change on the ground has an noticeable effect of the downscaling variables used (NIRv, NDWI or LST), we would expect to be able to see some change. A strong land cover change would probably be reflected for example. However, we agree that the downscaled SIF cannot fully replace a TROPOMI SIF retrieval. We will add a phrase to warn users about this point.

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