Review of Kohler et al, “A linear method for the retrieval of sun-induced chlorophyll fluorescence from GOME-2 and SCIAMACHY data”.

Overview

This paper presents a method to retrieve solar-induced chlorophyll fluorescence (SIF) from satellite measurements with only moderate spectral resolution. The paper does an OSSE-type validation using synthetic data, and then applies the method to both GOME-2 and to SCIAMACHY data.

General Comments

I have both positive and negative comments regarding this work. The application to SCIAMACHY has not been done before and in itself merits publication. However, the primary problem with this paper is that, so far as this reviewer can tell, the method simply takes an established method (Joiner et al, 2013) and makes two minor modifications to it. Those modifications, however, have a significant impact on the results, and may in fact be a WORSENING of the algorithm beyond the originally published method. Indeed, in certain areas with active vegetation, the seasonal cycle amplitude of SIF changes by a factor of 2-3 between the original and modified algorithm. This is a huge difference, and the cause should be better understood. Is this due purely to the underlying algorithm or due to the filtering method chosen (ie which soundings to average together)?

Considering these large differences, I cannot recommend publication until more foundational work is done to show if there is indeed a problem with the original Joiner algorithm (or the latest version of the Joiner algorithm available). The simulation-based validation may be sufficient for this purpose. In fact, because it appears that the identical simulations were used for validation of both the original Joiner et al. method as well as the presented method, it should be possible to do a more extensive validation with that synthetic data set. It would be best to provide a table like that of Table 1 Joiner et al. (2013), comparing your results directly to the Joiner results for the synthetic data set.

Indeed, the method presented appears to have some significant problems, which I describe in the specific comments section below. These comments should be fully addressed, and if necessary, modifications to the algorithm should be made to address them.

Specific Comments

- To motivate why a departure from Joiner et al is necessary, this work should state what the deficiencies of the Joiner method are. The fact that the Joiner forward model is nonlinear is not one of them. If it takes three of four steps to converge to the cost function minimum, so what? That is not a valid reason, unless three or four steps (instead of one) makes the algorithm too slow to apply to SCIAMACHY or GOME-2, which explicitly appears not to be the case. So it must be a reason concerning the accuracy of that algorithm.

- Please note that equation (1) is identical to Joiner et al. (2013) equation (1).

- In all your equations, please state that bolded items indicate vectors (ie quantities with a spectral dependence), or write out the wavelength-dependence explicitly.

- Equation (3) – please explain why the ρ_0 term can be (and has been) neglected. Certainly Rayleigh scattering could play a role and it seems strange to ignore it, when it is pretty easy to include at least to first order. I realize Joiner et al. (2013) also neglect this term.

- The linearization method described in equation (4) takes a reasonably valid forward model and makes it less valid. This is because the coefficients which determine $T_{\uparrow\downarrow}$ also determine $T_{\uparrow}$ (via your equation 4). But then you determine $T_{\uparrow\downarrow}$ separately. This doesn't make sense. One needs to solve for all these quantities simultaneously (and yes this makes the problem nonlinear!). Perhaps this is only a very small error, but this may explain why the performance of the "linear" method is relatively poor. It certainly seems like a wholly unnecessary step.

- Further, the step in equation (5) seems bizarre. The authors state that Joiner et al. solve for (i*j + 1) variables. In fact, they solve for (i+j+1). So if i=4 (# of reflectance coefficients) and j=10 (# of PCs to determine atmospheric transmittance), they would solve for 15 parameters. i*j+1 = 4*10+1 = 41 which is many more (and is simply incorrect). So the motivation to even perform this step seems to disappear.

- It may be that reducing the number of solved-for parameters would be useful, as this paper proposes, but it would be good to see that method applied directly to the model of Joiner et al (which uses the correct forward model, and keeps the transmittance parameters separated from the surface albedo parameters, which in itself greatly helps to reduce the number of parameters needed).

- Equation (6) is not generally applicable. This assumes Gaussian white noise. Many instruments have a noise that increases with increasing signal – in particular grating systems. Therefore, I question whether this is really the noise model that is suggested by the GOME-2 instrument team. Can the authors verify that this is the case? One should *always* use the noise model suggested by the instrument team, unless you have good reason to believe you have a more representative noise model for that particular instrument.

- Section 4.1 – please state if this is the identical synthetic data set used by Joiner et al. (2013). Both had 230,400 samples so it seems that that is the case.

- Please write the equation for "BIC". Is it the same as the reduced chi-squared of the fit to the measurements? If so, please used reduced chi-squared instead, as it is much more common than "BIC". If not , please state why BIC is preferable, with a reference if possible.

- Joiner et al. (2013) provide a statement that they have determined the theoretical error on the retrieval and it closely matches the true error, for the synthetic data test. Have you done so as well? What is the result?

- Page 12195, line 25. Does the SD achieved with the presented method agree with that calculated from theory (ie via simple error propagation of the assumed instrument noise model via your equation 7)?

- Page 12196, line 15. Why would one expect high reflectance surfaces to be associated with higher errors? I assume it is because scattering is enhanced (because of scattered light reflected from the atmosphere to the ground that can make it to the satellite). It would be good to state a hypothesis here.

- Section 5.4. This is interesting. Is this also seen in the Joiner et al. data set? If so, it would provide further proof of an instrument artifact rather than something particular to your retrieval algorithm.

Technical/Grammar Comments

- Pg 12177 line 17. "Comprehends". Suggest replace with "encompasses"
- Pg 12182 line 21. "in a sufficient precision" → "with sufficient precision"
- Pg 12183 line 1. "that a removal" → "that the removal"
- Pg 12183 line 12. "SIF free spectrum in an" → "SIF-free spectrum with an"
- Pg 12184 line 1. "by applying a previous step." → "by applying an additional step prior to minimization."
- Page 12187 line 17. "A further filtering beside" → "Further filtering besides"
- Pg 12188 line 3 "simulations comprehend" → "simulations include" or "simulations contain"
- Pg 12189 line 5 "TOCs." → "TOC SIF spectra."
- Pg 12189 line 10. "which impact the retrieval of SIF inevitably." → "which inevitably impact the retrieval of SIF."
- Pg 12192 line 6. "should amount at least" → "should be at least"
- Pg 12195 line 17. "discrepance" → "discrepancy". Also pg 12196 line 4.
- Pg 12196 line 3. "congruent" → "similar" (at least this is what I think you are trying to say)
- Pg 12199 line 2. "most homogeneous" → "mostly homogenous"
- Pg 12199 line 8. Suggest you cite Frankenberg et al. (2012) here, which shows how the SIF signal is impacted from cloud effects directly.
- Pg 12200 line 2. "against cloud contamination" → "against moderate cloud contamination". To make clear it is not impervious to all cloud contamination.