Biogeosciences Discuss., 7, C5165–C5169, 2011
www.biogeosciences-discuss.net/7/C5165/2011/

Biogeosciences Discussions

Interactive comment on "First observations of global and seasonal terrestrial chlorophyll fluorescence from space" *by* J. Joiner et al.

J. Joiner et al.

joanna.joiner@nasa.gov

Received and published: 25 February 2011

We thank Dr. Frankenberg for constructive comments that have helped to improve the manuscript.

We thank Dr. Frankenberg for pointing out his paper. We have now referenced this work in several places. In addition, we thank you in the acknowledgments for your helpful comments that we address below.

We repeat and respond to comments here.

"In figure 2, you show the GOSAT ILS and mention that the spectral sampling is depicted. But the data points actually indicate the sampling with which the ILS is provided, not the true GOSAT sampling, which is about 0.1995cm-1, i.e. only half of the FWHM.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The figure suggests that the ILS is highly oversampled which is not the case (in fact, it may introduce undersampling errors if you shift the solar spectrum wrt to the radiances)."

The caption statement that the spectral sampling is shown is indeed incorrect. We showed the measured GOSAT ILSF for reference, but as explained below, did not actually use it in the retrievals. We only used it in the simulations. This figure, as shown, is misleading and will be removed in the revised version.

Inadequate sampling will indeed produce errors when shifting the solar spectrum WRT the radiances. We have now mentioned this and have provided a reference (Chance et al., 2005). Further, we have simulated the undersampling error similar to Chance et al. and shown that it can produce a spectral structure similar to the radiance residuals that we have shown (though as you pointed out below, we originally showed the residuals on the interpolated wavelength grid, and not at the GOSAT sampling).

As undersampling most likely contributes to the majority of the residuals we see over the Sahara, we revised our retrieval approach. We now derive an average residual for various shift amounts and then build that into our radiance fit instead of trying to fit a coefficient to the derived average spectral structure. The previously used approach apparently contributed to the frequent fluorescence values frequently below zero. The new approach simplified our fitting and now the monthly mean fluorescence values in areas where no fluorescence is expected are much more centered around zero. The conclusions and overall results have not fundamentally changed. With our new approach, the scatter in the EVI vs scaled-F is also slightly reduced for some regions. We revised the text accordingly (see supplement uploaded in response to reviewer 1 for more details here and elsewhere below).

"In figure 6, you show exemplary fits in different geographical regions. The spectral sampling of GOSAT in wavelength units is about 0.01nm. This should give you about 10 data-points between 770.0 and 770.1nm. However, it appears that there are many

BGD

7, C5165–C5169, 2011

more, do you interpolate the true GOSAT spectra to a finer grid?" This might explain why your fits look smooth, not revealing the noise in individual measurements. GOSAT data are indeed quite noisy and the signal-to-noise ratio is between 100-300 in good cases. The fits you show are probably on co-added spectra and it should be mentioned how many spectra are averaged, otherwise it will give a wrong impression of the retrieval precision (which is not discussed here)."

We did interpolate the data, leading to a false impression of smoothness of the spectra. This has now been changed. Results are now shown for single spectra at the original sampled wavelengths and indeed the signal-to-noise ratios are within the range of expected values.

We also now provide sample estimates of the retrieval precision.

"It is also unclear which spectra are being used as GOSAT provides two polarization directions."

Thank you for pointing out that we did not give an adequate explanation of this point in the discussion paper. In fact, we used only the P polarization in the discussion paper. This leads to an underestimation of the absolute values of fluorescence and to the extent that the observed filling-in of the K line is polarized (e.g., solar zenith angle dependence of the rotational-Raman scattering filling-in), this could produce some errors in the derived seasonal dependence. We have now performed the retrieval for both S and P polarizations as a check. We find very similar results in the two polarizations. S polarization results for the seasonal dependence are now shown. We have provided more discussion on the 2 polarizations and now clearly state for which polarization the results are shown.

"Your fit seems to be a straight line away from the O2 bands (far edges of the fitting window). This suggests that you truncate the ILS, otherwise you would see the sidelobes of the unapodized sinc function. This may be critical, especially since you exclude the O2 lines, which have an impact on the Fraunhofer lines as well since the ILS is not

BGD

7, C5165–C5169, 2011

zero 0.05nm away from the center. Variations in the O2 column may thus impact your fluorescence retrieval and lead to biases. It is unclear to me at this moment whether or not the deviations from EVI (for example) are truly a difference in the signal or still a potential bias in the retrieval."

Thank you for pointing out that we neglected to adequately explain our approach which has lead to this confusion. In fact, we do not use the ILS and that explains why the fits are flat. As described below, we neither use radiative transfer calculations nor the ILS in the retrieval. Our approach is now more fully explained, and we have added justification of the assumptions made (see response to Reviewer 1 for more detail).

While it is true that the ILS is not zero 0.05 nm away from the center, the O2 lines nearest to the solar K line are extremely weak (see Fig. 5) and have negligible effect on the spectral structure inside the solar K line. The ILS amplitude falls off rapidly from the main peak such that the stronger lines, further from the K line, also have a neglible impact on our fitting window. We simulated a maximum possible effect by computing convolved spectra with zero O2 absorption and with O2 absorption at 20 and 70 degrees SZA. We took ratios of those spectra and saw no significant spectral structure within the K I line. This is discussed in the revised version.

"Figure 7: The need for an unexplained spectral structure of this magnitude is somewhat worrisome and may be related to an ILS problem, especially since the residual structure strongly resembles the ILS with somewhat stronger wiggles. Also here, the spectral sampling seems not to represent the true GOSAT sampling."

We do not use the ILS, so the spectral structure is not due to a dependence on ILS error. As stated above, we believe the residual structure is due primarily to undersampling as our simulations show that undersampling in the core of the K line can produce similar structure. It appears to be a coincidence that undersampling can produce a spectral structure similar to the ILS. We have also revised all figures shown originally at the interpolated wavelengths to now show the data at the actual GOSAT sampling.

BGD

7, C5165–C5169, 2011

"Figure 8 and following: Is there a reason why you cut the world-map at moderately high latitudes in summer (GOSAT should provide data)?"

Since we did not have retrievals for those locations in winter (snow/ice covered, thus inadequate cloud filtering), we cut off some of the high latitude data in summer as well. We now include high latitude summer data in our revised version.

"In the figures, it may also be revealing to include the frequently encountered negative values by e.g. starting from -1, not 0."

With the new approach negative values are far less frequent, but we have used the scale you suggested to show where those negative values occur.

"Inversion: The forward model may be non-linear. Do you use iterations (esp. the wavelength shifts will need iterations but also the albedo and maybe fluorescence if you fit in radiance space)?"

Thank you for mentioning this as we neglected description of iterations in our approach. In fact, we had tested the effect of iterations and found that they were not necessary, so we only do one iteration. Note: we shift the Earth spectra relative to the solar, because the solar spectrum is used in the Jacobian calculation. This makes the problem more linear. It is only the wavelength shifting that makes our problem non-linear, and because we do an initial shift before the fitting, the resulting minimization is very linear. We now discuss this briefly in the revised version.

Interactive comment on Biogeosciences Discuss., 7, 8281, 2010.