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Interactive Comment

Interactive comment on "Retrieval of aerosol parameters from the oxygen A band in the presence of chlorophyll fluorescence" *by* A. F. J. Sanders and J. F. de Haan

C. Frankenberg (Referee)

christian.frankenberg@jpl.nasa.gov

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The manuscript "Retrieval of aerosol parameters from the oxygen A band in the presence of chlorophyll fluorescence" describes a sensitivity study of the potential to derive chlorophyll fluorescence as well as aerosol parameters using the oxygen band. The paper focuses on linear error analysis and precision estimates derived from the model Jacobians and prospective instrument specifications. As such, the topic of the study is interesting. However, I feel that the authors misinterpreted previous studies that have dealt with exactly the same topic and are being criticized here. There is already a big discussion about how to best perform fluorescence retrievals and this manuscript un-

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Interactive Discussion

Discussion Paper

fortunately adds more confusion than clarification by focusing on precision errors rather than accuracy (which is most important, esp. once you have millions and millions of data points from satellites with which you can, in principle, reduce the standard error to arbitrarily low values by averaging; accuracy errors, however, won't reduce by the factor 1/sqrt(n) but stay).

On top of that, the underlying aerosol formulation is very simplistic and forward model error (the dominant source of error in these kind of retrievals) is entirely neglected. Precision estimates are derived from a very simple retrieval setup and the claims/accusations made in the paper are, in my mind, not substantiated. Given the cost of satellite missions, one should be very careful what to claim and acknowledge that specific forward model errors (e.g. different aerosol types, height distributions, fractional cloud cover, cirrus clouds and the list goes on) can bias the retrieval, both in fluorescence and aerosols. This has been shown in Frankenberg et al (Frankenberg, C., O'Dell, C., Guanter, L., and McDuïňČe, J.: Remote sensing of near-infrared chlorophyll ïňĆuorescence from space in scattering atmospheres: implications for its retrieval and interferences with atmospheric CO2 retrievals, Atmos. Meas. Tech., 5, 2081–2094, doi:10.5194/amt-5-2081-2012, 2012.) but the authors here strongly criticize this work and claim that our statements are erroneous and that chlorophyll fluorescence and aerosols can be retrieved alongside just based on the oxygen A-band even in the absence of Fraunhofer lines. This would indeed be wonderful and I would love to be proven wrong in that respect but I don't think that this particular study does a good job in that. We are happy to share simulated data to prove us wrong. Also, there are plenty of satellites out there that already provide data with which the claims by the authors could be easily corroborated. Without this exercise, however, the manuscript can't be published in its current form. I will go into more detail in the following:

Page 3186, lines 1-3: What we say in the paper is that aerosol and fluorescence parameters are not linearly independent and thus difficult to disentangle. It may still be true that both can be fitted but strong cross-correlations will occur (which we should try to avoid). On top of that, forward model errors (given that we can't model all aerosol possibilities) will bias both retrievals, fluorescence and aerosols. This won't happen using just Fraunhofer lines.

Figure 2: At the continuum level (around 772-775nm), the Fs derivative seems to be systematically higher than the other two. Do you know why that is (I assume because the solar irradiance is not flat)? I am wondering what added information this apparent slope (between the continuum at 755 and 774nm) provides for the Fs retrievals. It would be good to fit a slope to the albedo term as well to ensure that no information is provided by this effect (which mustn't be the case!). When you used a flat irradiance spectrum, was it just without Fraunhofer lines or entirely flat (so that the Jacobians in that particular case should be pretty similar and no difference in slope should be apparent between the albedo derivative and the Fs derivative. This could be crucial, so please check that.

Page 3186, line 15: You say that the shape is sufficiently different within the O2 A-band. This really only holds for tau. The albedo term and Fs is very similar and you also haven't shown the derivative of the height parameter. Also: If you would include the presence of thin high clouds, the story might also be different. You can still argue that in your particular case, they look different but I wouldn't generalize too much.

Page 3186, line 27: If this is right (focus on aerosol parameters), the authors should reshuffle their claims and focus more on that aspect (for which the bias in aerosol parameters may indeed be not too malign). Only the claims regarding fluorescence are going way too far in my opinion while precision estimates of 0.01-0.02 also seem too good to be true (as they will be overshadowed by forward model errors which are not taken into account in this study at all). This should be discussed.

Page 3188, line 26: I don't fully understand how you treat the finite resolution of the solar spectrum. If Figure 1 shows the simulations before convolution, the depths of the Fraunhofer lines seem to be way too small (Transmissions at high resolution are almost

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going down to 50% for some lines).

Page 3198, line 23:Why are you treating Fs as independent of wavelength? It changes dramatically within the fit window (as you also state earlier) and my hope was always that this may somehow actually provide some more information (which you ignore). For a better quantification, one would need to model that strong spectral slope (but then also fit a slope in the albedo term as otherwise the slope in continuum level radiance would provide most informations).

Page 3190, lines 5-10: To really get an estimate of what can be expected under realistic scenarios, test cases should include different aerosols types (and height profiles) in the simulated radiances (and then still be fitted with your “average” aerosol type). This will provide accuracy estimates in the case of forward model errors (which is what we did in the 2012 AMT paper).

Also: You don’t seem to fit for surface pressure while you might have to include it in order to see how independent it is. Surface pressure biases in met-fields can be 1-2hPa and if you have some pointing uncertainty in elevated areas, it will be even higher.

Page 3190, line 15: What happens if the temperature profile is a little bit off (between simulations and retrievals)?

Page 3194, line 10-11: This is just not true and shows a conceptual misunderstanding of our study. Your study is full of a priori information. You assume that you perfectly know the aerosol type, its height profiles (basically a delta peak), the surface pressure, the atmospheric temperature profile, the absolute absence of clouds (no cirrus clouds, no fractional cloud cover etc). This is very stringent a priori information (basically a prior with zero uncertainty in the “Rodger’s sense”) and your conclusions are sound only in the condition that these assumptions are fully valid in reality (which is a rather strong assumption).

Page 3197, line 8: To really differentiate the information from the A-band and the Fraun-

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hofer lines, you would have to run two cases: One (which you did) by flattening the irradiance spectrum and one, where you turn off oxygen lines. Ideally, you zero out both to ensure that your Fs retrieval will break down in that case as it won't be distinguishable from albedo changes.

Page 3198, lines 14++: The authors state (in a very direct way) that we basically disproved ourselves by making apparently contradictory statements in our 2012 paper. First of all, if I find such contradictions (which apparently no one writes on purpose), I would advise just directly asking the respective authors what they actually meant instead of citing entire sentences in a rather condescending way. Here is what the sentence meant in a nutshell: "using the potentially added information contained in the O2 absorption structures seems to do more harm than good for the ïňĆuorescence retrieval because interferences are introduced". → It means that the inclusion of Fs in the full retrieval can in principle (theory) improve the Fs retrieval but in reality won't because there are forward model errors that (even in the absence of instrument noise!) bias the fluorescence retrievals because we don't include the right aerosol types and height profiles in the retrieval model. Please compare the errors in Fs retrievals in Figure 8 and 11 (on our 2012 paper) and say that the errors (all of which are systematic, i.e. accuracy errors because we used noise-free data) in Figure 8 are smaller. By including Fs in the full-physics O2-A band fit for our XCO2 retrievals, we still improve the XCO2 retrievals, however. This is because we get rid of the apparent bias that may depend on true Fs (which is very critical as it can co-vary with CO2 fluxes). Note that we really focused on pure accuracy errors as we used noise-free retrievals. The Fs precision may have been improved but the accuracy using the O2 band deteriorated. This is all we wanted to say, maybe we could have phrased it more clearly as such a misinterpretation is unfortunate and can certainly happen to other people too if you saw it that way.

Bottomline: I really wish it was that easy to get accurate retrievals of Fs from just the A-band but you are not disproving us as you only focus on precision, not accu-

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racy. We can share the model simulations from the 2012 paper and you can try your retrievals. There is also plenty of data out there from other satellites (GOSAT, SCIAMACHY, GOME-2, etc) and you can try to disprove us using real data. I would indeed love to be wrong but this simple study based on precision errors only doesn't show it. Our latest operational ACOS XCO2 retrieval from GOSAT also already includes the full Fs fit in the oxygen band but we advise not to use the Fs retrievals from this fit but the ones from the Fraunhofer line only fit (if you want to look at Fs). Looking at the precision error in our GOSAT Fs fit, they are on the order of 0.6-1.5 e11 photons s^{-1} cm^{-2} sr^{-1} nm^{-1}. These are on the order of what you see (a little higher I would say) but GOSAT has a 0.02-0.03nm FWHM (and SNR are also approaching 500 in the combined P and S polarization). This may have to do with the fact that we use a more complex aerosol fit (though we even get aerosol information from the strong CO2 band, so including the CO2 bands should in turn help constrain aerosols in the O2 band). Again, we prefer to trade precision with accuracy and this is at the heart of the problem, actually for both, fluorescence and aerosols. If the focus of the paper is more on aerosols itself, the authors should also attempt to quantify systematic errors in those retrievals by having retrievals with inconsistencies between simulated radiances and the retrieval (which is what will happen in real life).

There are also citations missing regarding paper that already dealt with aerosol or cloud retrievals from the O2-A band: Sanghavi, S., Martonchik, J. V., Landgraf, J., and Platt, U.: Retrieval of aerosol optical depth and vertical distribution using O2 A- and B-band SCIAMACHY observations over Kanpur: a case study, Atmos. Meas. Tech. Discuss., 4, 6779-6809, doi:10.5194/amtd-4-6779-2011, 2011.

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Interactive comment on Atmos. Meas. Tech. Discuss., 6, 3181, 2013.