Atmospheric
Measurement
Techniques
Discussions

Interactive comment on "Carbon Monitoring Satellite (CarbonSat): assessment of scattering related atmospheric CO$_2$ and CH$_4$ retrieval errors and first results on implications for inferring city CO$_2$ emissions" *by* M. Buchwitz et al.

Anonymous Referee #1

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The paper by Buchwitz et al. investigates the performance of the proposed CarbonSat mission, which aims at measuring the atmospheric column average dry air mole fractions of CO2 and CH4 with high accuracy. The paper describes retrieval simulations and discusses the residual random and systematic errors induced by light scattering on atmospheric particles. It estimates these error contributions on the global scale and presents a modeling study how well anthropogenic CO2 emissions from cities can be estimated. The study further touches on CarbonSat's ability to retrieve plant fluorescence.

Overall the paper is quite well written. It is ambitious in covering various topics but – in my opinion – lacks some thoroughness in the individual steps. It might be worthwhile considering shortening the paper by dropping the parts on plant fluorescence and the proxy retrievals.

I have several comments and concerns which are listed below in order of occurrence. The main question I did not understand throughout the paper is the following.

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Main question:

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What is the origin of the systematic errors derived in section 4 and used furtheron throughout the study? For each retrieval simulation, an overall error is derived via "retrieved minus true" (p4785,l22). Then, the paper assumes two components, a systematic and a random one. The systematic one is attributed to light scattering effects. So, I assume, that this means that the simulation model (for scattering) is different from the retrieval forward model ie. the retrieval cannot converge to the truth. If simulation and retrieval forward models were consistent, there should be no systematic error due to scattering (unless there is "technical" issues such as non-convergence, not finding the true minimum of the cost function, etc). So, how is the simulation model different from the retrieval model?

It is important to clarify this question because the estimate of systematic errors and thus, most conclusions of the paper depend on how realistic the driver of these errors is. If I simply do not get it while this information is in the paper, please try to make it more explicit.

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Further comments:

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p4779, l27...: Why would one design a full physics retrieval algorithm that retrieves two types of AOD such as AODNIR and AODSW2 which are uncoupled among the retrieval windows? From a physics point of view, spectral variation of AOD is determined by the microphysical properties of the particle ensemble. So, it would make sense to retrieve particle size parameters, refractive indices or fractional amounts of different aerosol types. In particular, such parameters would then couple among all three retrieval windows exploiting the full information content and potentially give a scattering parameterization that suffers from least correlations with the target absorber concentrations. Using two different types of AOD with "zeroed" Jacobians in either of the retrieval windows as suggested here essentially comes down to using the spectral windows independently.

I find this strategy particularly surprising since the algorithm seems capable of performing "window-coupled" retrievals of scattering properties cf. cirrus parameters, water cloud parameter.

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p4780, section 3.2 ... : Section 3.2 seems unnecessary to me. Please consider removing it since the manuscript is lengthy anyway.

If I understand correctly, the section describes an approximate algorithm how to obtain an a priori estimate of vegetation fluorescence (VCF) to be fed into the full physics retrieval algorithm (which incorporates a full-fletched VCF retrieval): - The description of the approximate VCF algorithm is rudimentary. Maybe some describing equations could help. - Why do you discuss the results of the approximate "a priori" algorithm not those of the full-fletched algorithm? - Computational cost as hinted at in the manuscript cannot play a role, since only 180 cases are considered for the approximate algorithm which is on the same order of magnitude as the number of cases run for the full physics method anyway. - Vegetation fluorescence is somehow off-topic. The rest of the paper

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treats errors due to light scattering effects. A dedicated section dicussing the a priori estimate of VCF is not required from a scientific point-of-view.

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p4783, section 4 ... : Section 4 describes how retrieval simulations are used to setup an error parameterization. The following points remain unclear to me. - See main question: What are the error terms evaluated here? For each retrieval simulation, an overall error eT can be derived via "retrieved minus true" (p4785,l22). The systematic one is a forward model error eF which is due to the fact that the retrieval forward model (for scattering) is physically different from the simulation model, i.e. the retrieval cannot converge to the truth. Is this correct? The second error contribution is the noise error eM which comes from instrument performance parameters (table 1). How do you disentangle eM from eF in the total error eT? Do you use the gain matrix and the individual instrument noise error (known from the simulations) or do you run an ensemble of noise realizations?

- Scattering particle type (size, chemical composition) has been identified as one of the critical parameters since it is the particle type together with spectral variation of surface albedo that drives the spectral dependence of light path modification. All state-of-the-art full-physics algorithms aim at estimating the particle type with some sort of approach [eg. O'Dell et al., 2012, Butz et al., 2012, Yoshida et al., 2013]. If I understand correctly, the study includes no estimate on how the particle type affects performance. I would consider this a serious shortcoming. The authors comment on this shortcoming in section 8.

- In my opinion, the error parameterization derived in section 4.3 gives a very poor fit to the actually observed errors although the fit is based on most parameters relevant for scattering effects. Are you sure that there is no "hidden" source of error other than scattering (eg. non-convergence, too tight prior constraints) that affects performance?

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p4789, section 5 ... : The case study (for Germany) discussed in section 5 seems very benign to me. The cirrus and aerosol optical depths sum up to 0.1 for most of the scene, which is at the lower end of simulations conducted in section 4 and which for cirrus is close to the a priori value (COD=0.05). Further, the albedo is on the order of 0.2 for most of the scene, the range for which light path enhancing and shortening effects tend to cancel anyway (at SWIR wavelengths). Did you check on performance if scattering is simply neglected which could be used as a contrasting case to quantify the "benefit of the method"?

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P4792, l15 ... : In the view of shortening the paper, one could remove the section on the proxy retrieval. Maybe I misunderstood the rationale here, but the formula for propagating CO2 and CH4 errors into a proxy error (p4792, l29) seems wrong to me. For a ratio quantity, it should be the relative errors that add up quadratically not the absolute errors.

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P4794, section 6 ... : If I understand correctly, the inverse modeling study assumes that the spatial pattern of anthropogenic emissions and the atmospheric transport are perfectly known. How would errors in these assumptions propagate into the emission estimates?

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P4802, section 7 ... : The errors on the global scale seem extremely low in comparison to what other teams (eg. working on GOSAT/OCO retrievals and simulations) find. So, it is critical to discuss what systematic error means in the context of this study and in comparison to other studies (see main question).

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