Atmos. Meas. Tech. Discuss., 6, C1954–C1961, 2013
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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 #2

Received and published: 9 August 2013

General comment

In this manuscript, Buchwitz et al. presented the specification of their planned CarbonSat instrument and retrieval method (its prototype was presented in Bovensmann et al. 2010, ACP) and then quantified the expected systematic error due to aerosols and thin cirrus clouds. They developed an error parameterization method that allows them to compute the error for each single sounding taken by CarbonSat. This approach should be useful given CarbonSat will deliver a huge amount of soundings, compared

to current GOSAT for instance. Buchwitz et al. then implemented Observing System Simulation Experiments (OSSEs) using the CarbonSat specification and the error characterization method, to assess the impact of the systematic errors on anthropogenic emissions estimate. They also presented the random and systematic errors by geographical region to provide implications of how future inversions are going to improve source and sink estimates. The paper is well written. Especially, the introduction section is very rich in text and information. I myself, as one of scientists working on carbon cycle, think it would be fantastic to have a carbon observing satellite like CarbonSat presented in the manuscript. I am pretty sure such a future satellite will provide huge research opportunities to us and I hope CarbonSat will be selected for launch and it will collect a huge amount of useful spectra for us! BUT as a reviewer of the manuscript, I am a bit concerned about the manuscript (see my comments below). I think the manuscript needs some work before goes to publication. I hope my comments could be helpful for the authors to improve the manuscript.

1. Split the manuscript into two or provide more information by text.

I agree with the comment by the referee #1. I think it would be better to split this manuscript into two (or three?). The main reason why I am suggesting this is that I see the Berlin emission OSSE part still needs more work to conclude. I think the authors could drop the Berlin emission OSSE part and conclude this paper before that. Although the authors are able to save a space and keep the manuscript in the present length by citing published works (which is nothing wrong!), I thought there are still a lot to explain by adding more text in the manuscript. I believe the authors could prepare two (or more?) nice manuscripts that deliver much clear and concise messages to the audience of AMT. If the authors go with the current form, I would suggest to adding more information to the manuscript by text (or equations and/or figures, as appropriate), instead of just citing past works. For example, I see Bovensmann et al. (2010) mentioned many times. At some places, the author could help the audience's understanding by put more information instead of just mentioning Bovensmann et al. (2010).

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I myself have read Bovensmann et al. (2010) before, but I still needed to go back and forth between the manuscript and Bovensmann et al. (2010) just to understand the story in the manuscript. Citing past works is nothing wrong and it is definitely fair. But I think it would be fair for me to comment on this given the essential huge amount of work presented in the manuscript. My suggestion would make this manuscript much longer for sure, but it should help the audience of this manuscript to understand this work better. So once again, please consider: Split the manuscript or provide more information. I am inclined to suggest to splitting the manuscript and would like to see two (or more?) concise manuscripts with clear messages.

2. Need more work for Berlin emission OSSE.

As I mentioned above, this is the main thing I would like to discuss here. My big concern is the emission estimating technique used for the Berlin emission OSSE. The feasibility of the inversion system and discussions of the errors associated emission estimates are ultimately based on the assumption that you can model anthropogenic XCO2 spatial patterns very well. You need to show information that supports your assumption above. At least, the error coming from the assumption is small enough compared to the error you are focusing here. I see many work has been done here, but feel it is still not enough to discuss the error on emission estimates using the OSSE. Also, I am really not sure if the inversion technique (two parameter estimation) would works for estimating accurate anthropogenic emissions. Even CarbonSat provides nice highly resolved XCO2 images, I think it would be still difficult to disentangle anthropogenic and biogenic contribution. One thing came up to my mind is a work by Vogel et al. (2013). They employed a similar transport model system and worked with 14C measurements. That work could tell how difficult to solely model anthropogenic components, from measurement data, model and emissions data perspectives (any chance of collaboration with them?). Although the target city Heidelberg is not huge city like Berlin (maybe some of you from Heidelberg?), but it would be worth working at the ideal site to establish an inversion method for anthropogenic emissions as they can

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tell how much anthropogenic contributions coming in. So, that being said, I thought the Berlin emission OSSE section could be an independent manuscript, given a lot of things to discuss. I also include my line-by-line comments below and hope the authors could address points I made.

3. Figures

Text on figures should be readable. Text on figures are often too small to read (although I can read them if magnified). Also, some figures are small and busy. I think this is trivial to implement.

Line by line comments

P4771, L15: BESD/C, spell out if appropriate.

P4784, L18: Or prescribing aerosols using aerosol models?

P4785, L27: criterium -> criterion

P4786, L1: criterium -> criterion

P4795, L22: A reference for WRF? (if you think this appropriate)

P4795, L24: A reference for VPRM? (if you think this appropriate)

P4795, L29: Do you keep consistency in prior fluxes between the global simulation by TM3 and the regional high-resolution simulation? Especially, I am curious about fossil fuel emissions as you used IER (which is a regional dataset) for the European domain simulation. As you are solving for fossil fuel emissions, you can just choose a fossil fuel emission data as a good spatial proxy. Is that what you are assuming? Even if so, I still think that the consistency in prior flux is important (e.g. mass conservation in your whole system). The estimated fossil fuel emissions (in the regional domain) should be consistent with the emissions used in the global simulation.

P4796, L3: I would suggest to adding an equation to what you are solving in your

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inversion (define your cost function). That would be informative for the audience of AMT. It was tough to understand what you are doing here by the information provided.

P4796, L4: So you assume you can well reproduce anthropogenic XCO2 patterns in your model. I cannot agree with this at this point. Here are several thoughts:

1) Yes, we are prescribing fossil fuel emissions in inversions. But that is a different inversion problem (often solving for natural sources and sinks). 2) Given IER dataset is constructed using (probably) the best available activity data, you might be able to well prescribe fossil fuel EMISSION patterns (I am here still ignoring temporal variations in emissions). But emission patters is not identical to XCO2 pattern due to atmospheric transport (although your instantaneous XCO2 image could put you in a better position to justify it). For instance, Pillai et al. (2010) suggested that most variability we see in XCO2 could be explained by variability in the lower atmosphere. However the correlation was still 0.37 in the better case (I acknowledge that R=0.37 at that spatial scale is great!). So you can't assume like "Emission pattern = XCO2 pattern". 3) To estimate emissions by fitting modeled XCO2 fields to your planned XCO2 (or XCH4) images that have a 2 x 2 km resolution, it seems to me that you need to have an amazing modeling capability. I imagine the errors would arise from the assumption of replicating XCO2 patterns well, would be significant. To verify, you really need to implement simulations at the 2 x 2 km resolution. 3) Once again, to solely attribute the mismatch between XCO2 images and modeled XCO2 fields to the error due to geophysical difficulties, you would need to be perfect in your simulation. Do you use IER hourly temporal variations in emissions for this OSSE? Potential biases in XCO2 modeling arising from wrong temporal variations could be significant for your CarbonSat case (There is a paper by Nassar et al. (2012) simulated hourly XCO2 fields).

P4796, L11: A reference for STILT? (e.g. Lin et al., 2003)

P4796, L20: I assume 54 MtCO2/yr is annual total emission for the region aimed. Then did you account for seasonality (or weekly variations) in emissions here in this OSSE?

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P4797, L3: We could simulate anthropogenic XCO2 contributions in a model. But in practice, we can't tell how well the contributions are replicated given the presence of biogenic sources (as well as errors associated with high-resolution modeling). Could you comment on this? If you verify your method at a site where you could really disentangle anthropogenic and biogenic contributions (e.g. Vogel et al., 2013), it would be convincing.

P4797, L10: VPRM is calculating biogenic fluxes by considering instantaneous weather (which is simulated by WRF). Correct?

P4797, L14: This could be definitely true in a theoretical world (no error!), but we don't know how the low correlation could help you to disentangle two emission contributions. Any reference or supporting information for this?

P4797, L23: It seems to me this is a big assumption. Similar to biosphere (not exactly the same although), human activities could also be a function of weather (e.g. heating/cooling). We could use "climatology" of emission temporal variations, however the assumption could introduce a significant error especially if you implement simulations at a high resolution. Any consideration for this in your OSSE? Also, modeling PBL should not be trivial at the spatial scale of interest. You need to do a good job to simulate the formation of PBL to get an accurate CO2 mixing ratio close to the surface (which CarbonSat has its sensitivity). Maybe you could comment on this from a work by Kretschmer et al. (2012)?

P4798, L10: What if you create a figure of total XCO2 (anthropogenic plus biosphere = Fig, 12a plus 12b)? Do you still see clear enhancement due to the anthropogenic emissions in the XCO2 field?

P4798, L13: I need your help to understand. I see biogenic XCO2 variability even in the white box in Fig 12. The variability is about the same order of the magnitude as XCO2 enhancements we see in Figure 12 (A). What is constant?

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P4798, L20: Once again, I cannot agree with this assumption at this point.

P4798, L25: Yes, this is what I am worried about.

P4798, L27: This sentence might be missing something.

P4798, L28: So the discussion here is based on the assumption that you can model anthropogenic XCO2 very well. You need to show information that supports your assumption above. At least, the error coming from the assumption is small enough compared to the error coming from the satellite side.

P4799, L17: much too -> too much (?)

P4800, L1: Once again, this seems to be a big assumption.

P4800, L5: Yes, you can implement an inversion by adding one more scaling factor. Have you checked if it really works that way? (would you be able to really disentangle biogenic and anthropogenic contributions?)

P4801, L9: So how would you calculate the annual total emission for Berlin, given you would get 22 (or 39) XCO2 snapshots and associated emissions estimates? Would it be possible to derive a policy-relevant number?

P4801, L16: Yes, this is true in OSSE. But in practice, if we go higher resolution in space and time, we would have more error in modeling (No?).

P4801, L18: This is exactly what I want to say. This is very critical to conclude this OSSE study even you focus on systematic errors from satellite side. Without dealing this, I think it is tough to make a conclusion.

P4804, L13: occure -> occur

P4807, L5: Given many assumptions and future works acknowledged by the authors, I like to have a statement saying like “the error could be much larger”.

P4839, Fig.12: Would it be possible to use the same color scale (especially for bio-

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genic and anthropogenic emissions) or create a figure showing biogenic XCO2 fields plus anthropogenic XCO2 fields? (which is you are going to fit in your inversion, if I correctly understood). “Modelled” or “Modeled”? Please keep consistency throughout the manuscript (probably, “modelled” for AMT?).

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