Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-325-RC2, 2016

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "What are the greenhouse gas observing system requirements for reducing fundamental biogeochemical process uncertainty? Amazon wetland CH_4 emissions as a case study" *by* A. Anthony Bloom et al.

Anonymous Referee #2

Received and published: 26 May 2016

This study presents an OSSE for different hypothetical LEO and GEO satellite instruments. The focus is on the requirements on these observing systems for obtaining process-relevant information on wetland emissions in the Amazon region. As explained below some assumptions are made, which are not well justified but have a potentially large influence on the conclusions. These will have to be dealt with in a satisfactory manner to make this paper suitable for publication in ACP.

GENERAL COMMENTS

Autocorrelation scales have been derived for several parameters to motivate the choice

of spatial scale that the measurements should be able to resolve in order for the OS to help us gain process understanding. It is presented as a novel approach that could be applied to other related problems. Although I appreciate the attempt to derive such scales (which indeed addresses an important question), I do not agree that the presented method solves this problem. The reason is that the results presented in figure 3 depend on the scale of the data sets that are used. What is shown is the autocorrelation of parameters that are averaged on a scale of 0.5x0.5 degree. If the resolution of the datasets were much higher, then other more local processes would contribute to variability shortening the overall auto-correlation scale. Indeed it is questionable whether the methane emission from a local pond really correlates with one that is 100 km away. What is the motivation to use datasets at 0.5x0.5 degree? If the processes themselves motivate this choice then this should be explained. In absence of such a motivation it is a probably more a practical choice. I have no problem with this choice as long as its limitation is made clear, and that it requires reconsideration for any other application.

If it is considered important that the inversion resolves the autocorrelation scale then it is not sufficient to evaluate the posterior uncertainty at that scale. This is because the off-diagonals of the posterior covariance matrix might indicate that neighboring fluxes are not independently determined. In this study, however, the performance criterion only considers values on the diagonal. In addition, the choice of 25% confuses monthly and annual fluxes. The requirement is on monthly fluxes, but it is derived from an estimate of Melack et al on the annual time scale.

It is unclear why a special effort is made to derive requirements on horizontal resolution looking at the drivers of processes, whereas this is not done for the requirements on flux precision and temporal resolution. Since the inversion solves for net fluxes it remains unclear anyway if these requirements really allow us to constrain specific processes. Wouldn't it have been more logical to vary process model parameters to determine what is needed to resolve them? You might wonder whether it is even realistic to constrain processes only by measuring XCH4 using a single instrument. Atmospheric measurements are useful for constraining regional emission budgets, which - in combination with other information - can be used to derive improved process understanding. The OSSE approach that is taken disqualifies instruments that provide useful constraints on larger scales as part of a multi-component global monitoring system.

This OSSE is extremely (and unrealistically I would say) optimistic about the uncertainty reduction that can be achieved by averaging large numbers of data. It is mentioned that the 'cumulative' uncertainty of GEO OS may be as low as 0.02 ppb. It is probably a main reason why the GEO measurement concept performs so well in this study. In reality, however, systematic uncertainties will kick in at much reduced precisions preventing any further improvements upon averaging. Some attempt should be made to assess the sensitivity of the conclusion that improved process-understanding calls for the GEO approach, to the presence of systematic errors in the data.

Further effort is needed to quantify the impact of errors due to the simplified treatment of atmospheric transport. In general, surface fluxes are proportional to spatio-temporal concentration gradients in the atmosphere. Looking at figure C1 it becomes clear that the east-west gradient in WRF is substantially stronger than in LPDM. It has probably to do with the north- and southward transport along the Andes in WRF, which is missing in LPDM. The impact of this should be quantified.

It should be made clearer why the analysis is limited to the month of March. Many things are different in other months (atmospheric dynamics, cloud cover, CH4 fluxes, etc.). March doesn't sound like a particularly good choice as average, or representative month.

SPECIFIC COMMENTS

Page 7, line 14: 'Throughout ... CH4 emissions" I don't see why the fact that 25% is in between the dynamic ranges of monthly GPP and inundation variability would make it suitable for separating their influences. Apart from this, what justifies the assumed

linearity between these drivers and methane emissions?

Page 9, line 11: 'i.e. all accepted ... 100% cloud-free' According to Appendix B, MODIS data that is probably cloud-free are considered as fully cloud-free. These two statements do not fit together.

Page 11, equation 3: Why is c{L,0} calculated? In the end all that matters is the spread in 'c' due to the random perturbation and how it maps on 'f' using 'A'. The uncertainty in 'f' does not depend on the mean of 'c'.

Page 13, line 21: 'If Amazon CH4 fluxes .... likely be lower' This depends on the distribution of cloud cover. The wettest regions will likely be measured the least frequent. This calls for further motivation of why uniform emissions have been assumed.

Page 15, line 7: Why is the purpose of the parentheses here? Please clarify further at what p-level the autocorrelations are required to be significant, and how this is determined. For example in the following sentence if is not clear what r_i refers to. Please revise the description to explain more clearly what was done.

Figure 1: What are the different lines in the inset figure?

Figure 4: Why do you call this 'cumulative precision'? Isn't it rather the precision of a 300x300km2 average?

Figure 5: Why isn't cloud filtering affecting the number of data, comparing GEO, GEO-Z1, GEO-Z2?

Figure B1: I assume that both panels represent March 2007. If so, then this should be made clear.

Figure C1: Do these values represent the total column? If so, then mention this.

Appendix B, line 18: f(omega,i) is not used in equation 1. Where do the 30x30km2 areas come from?

Appendix C, line 17: The mean in CH_4 is not the relevant quantity to compare LPDM and WRF (it is the gradient in the wind direction that matters).

TECHNICAL CORRECTIONS

-

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-325, 2016.