Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-234-RC2, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Can we detect regional methane anomalies? A comparison between three observing systems" *by* Cindy Cressot et al.

Anonymous Referee #2

Received and published: 27 April 2016

This paper shows results from a set of CH4 inversions using three different observation sets (in situ, IASI, and GOSAT) to test whether anomalies in flux can be detected across a range of time and spatial scales. The ultimate goal is to determine whether such inversions can be used to attribute methane flux signals, like the change in global growth rate through the 2000s, to a particular region or regions and, perhaps, biogeochemical processes. The authors have done a lot of work to make the results statistically meaningful, the approach is generally sound, figures and tables are informative, and the discussion is accurate, if perhaps not fully satisfying. In my opinion the material is clearly worthy of publication in ACP after satisfying the concerns of the reviewers.

The paper suffers at times from lack of clarity and some inverse methodological issues exist, which are well-characterized by Anonymous Reviewer #1 and the comment from T. G. Nuñez Ramirez. I did not find the tables too difficult, but they do take some focus.

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Discussion paper

These issues aside, the question that remains to me is: so what? What are the implications of the findings for using CH4 measurements and inverse models to understand the underlying processes? What is the message for carbon cycle science? To my reading the answer to the title question is: NO, except on the broadest of scales and strongest of signals (seasonality), which doesn't really require very extensive measurements or sophisticated mathematical techniques and holds little useful information. This is a serious problem for understanding the current CH4 budget, for projecting future interactions of CH4 and climate, and for designing mitigation policies to reduce the radiative forcing of CH4. The paper alludes to some of the most egregious shortcomings, but never really comes out and says our data and techniques are inadequate and what should be done about it. I fully agree with other comments that setting the detection criterion to SNR =1 is a very low bar for attributing anomalies to specific locations and processes. The paper is also sometimes seemingly overly optimistic about the model ability to capture signals, e.g., Conclusions line 8-11, where having any detectable anomalies (∼25% on average) is called 'fair to good' and Abstract, where regional scale signals are said to be 'properly detected.' Clearly something much better than current observations and/or existing model formulations is needed. I think the paper should not shy away from such a statement and point out specifically where the problems reside in the analysis. The fact that the detectability depends on the underlying (modeled) signal configuration is further indictment of the overall flux analysis method. The statements that inversions 'should always include an uncertainty assessment', 'attribution. . . needs more attention', and 'more observations and . . . improved transport' are platitudes that don't require a detailed analysis like the one produced in this paper. Go ahead and give the discussion some punch.

Minor Comments: The analysis does not address transport issues at all, although perhaps it could. Such analysis could include impact of transport uncertainty on inference of fluxes in unobserved regions (e.g., satellite data in dark or high latitudes) and resulting 'noise.' Expand discussion or delete from Conclusions lines 32-33.

Not clear that detection of anomalies at grid scale in Amazon is robust. Depends on signal, which may not be realistic from sparse data constraint. Maybe examine more closely or moderate expectations.

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