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Interactive comment on "On the use of satellite derived CH_4 / CO_2 columns in CH_4 flux inversions" *by* S. Pandey et al.

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Summary of review:

The authors outline a new method to interpret space-borne atmospheric observations of XCH4/XCO2 to infer surface fluxes of CH4 and CO2. They concurrently assimilate surface observations of these gases to help separate the information embedded in the ratio. The paper is generally good but weak in describing the method in places. Unfortunately, the newness of the method is greatly exaggerated with the technique outlined and demonstrated in a recent paper in this journal. Nevertheless, once this and other comments have been addressed I don't see why it can't be accepted for publication.

Specific comments:

The authors advertise the newness of the method but this is deceitful. The broad methodology has been reported in Fraser et al, 2014. I'm sure details of the authors' new methodology are indeed new but they cannot claim the method is new. Their one mention of Fraser et al as being noteworthy is disingenuous at best. On a more positive note, it is encouraging that this method works well using a different transport model and inversion method (4D-Var vs MAP for Fraser et al, 2014). At the very least, these authors should discuss the similarities in their method and results with those previously reported by Fraser et al, 2014.

Section 2.1:

* Do the authors assume that R and B are diagonal?

* Not reporting a posteriori uncertainties is a major weakness of the method. How do they know that a posteriori fluxes are indeed significantly better than the a priori fluxes? I appreciate that small uncertainties is not a perfect metric but it is useful.

Section 2.2:

* Typo: assumned.

* The authors mention nothing about temporal and spatial correlations (see above comment about R and B).

Section 2.3:

* Did the authors sample the RemoTecv1.9 data for cloud-free scenes determined by small AODs and cloud fractions?

* Some brief details about the representation error would be useful to report in this paper rather than a simple reference to Basu et al, 2013.

* I'm not sure I completely follow the logic associated with the decision about not per-

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turbing pseudo observations. It depends if they want to characterize their inversion system ability to infer fluxes.

Section 2.4:

* The authors do not clearly explain in the abstract or elsewhere why their RATIO methodology uses the surface data. They do not explain why they are using these data.

* There is no mention anywhere that the ratio data have a smaller systematic bias relative to the full-physics products.

* Based on the remainder of the paper it is not clear why the paper title, abstract etc is focused on inferring CH_4 fluxes even though the method clearly has a capability to infer CO_2 fluxes (see section 3.4).

Discussion:

* There is a paragraph apologizing for not reporting uncertainties, which is clearly not good enough. Maybe they could compare/contrast the reporting of uncertainties from other methods.

Reference:

Fraser, A., Palmer, P. I., Feng, L., Bösch, H., Parker, R., Dlugokencky, E. J., Krummel, P. B., and Langenfelds, R. L.: Estimating regional fluxes of CO_2 and CH_4 using space-borne observations of XCH_4: XCO_2, Atmos. Chem. Phys., 14, 12883-12895, doi:10.5194/acp-14-12883-2014, 2014.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 8801, 2015.