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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Separation of biospheric and fossil fuel fluxes of CO_2 by atmospheric inversion of CO_2 and $^{14}CO_2$ measurements: Observation System Simulations" *by* Sourish Basu et al.

Anonymous Referee #1

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Basu and colleagues investigate the potential to quantify fossil fuel CO2 emissions from atmospheric CO2 and 14CO2 data. They use synthetic atmospheric inversions for North America assuming either a data availablity as existing in 2010 or a realistic assumed extension for the future. If this data availablity becomes true, Basu and colleagues conclude that monthly fossil fuel emissions can be quantified within 5% in the more densly observed regions of the USA, which can be used to independently verify reported emissions. This is a highly relevant piece of information, as the build-up of observational capacities is expensive and requires time.

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Discussion paper

The results are obtained by widely accepted methods which are convincingly applied and described. The presentation is very clear and covers the relevent aspects. I clearly recommend to publish this work in ACP.

I only have a few minor comments and suggestions:

p2 l6: maybe add "e.g., Andres. . ."

p3 eq1: Strictly, this equation is only true for a well-mixed volume, not for the whole "atmospheric burden". I'm aware that the message of the equation is about the individual flux components, but maybe remove the ambiguity by saying e.g. "local CO2 mixing ratio" (or "local atmospheric mass balance" in line 15).

p4 l1 and beyond: It seems the unit is misspelled and should be "PgC/yr * per mil" (not "/ per mil").

p6 l13: It would be easier to use the labels "e", "r", and "h" already in the enumeration in lines 8-12.

p9 l5: Clearify if you sampled at the two times *each day* (I assume so but it remains open).

Sect 3.4: You denote the absence of transport error as a limitation, but I'd actually see this as an advantage, because the result specifically diagnoses the constraining power of the observations. (I nevertheless agree that the investigation of transport model errors as done later is interesting information.)

p10 l15: Fig 3 is referred to later than Fig 4.

p11 l4-12: As you do not use the explicit covariance matrices anyway, I feel this description rather confuses and could be omitted.

p11 l24: I agree the metric is objective, but how to interpret it quantitatively? (see comment below)

p12 l8: I'm astonished why you cannot be sure about your convergence. Couldn't plot the result as a function of iteration count and check if the behaviour is still transient?

Sect 4: In rare cases (e.g. fig 7 region 5 in summer), the 2010 stations give considerably better fit - do you know why this is?

p13 l1-4: You invoke transport model errors (which is somehow contradicted by the absence of model errors), but couldn't that just be "leakage" from neighbouring regions due to incomplete seperability?

p14 l13: To make this more understandable, say what you in contrast expect for less well constrained regions, and why.

p14 l21-25: I'm not sure I fully understand this. Wouldn't the criterion for separability be a correlation range overlapping zero? As said earlier, I'm not fully convinced that the correlation coefficient can be interpreted quantitatively.

Conclusion: Ingeborg Levin and colleagues had concluded that fossil fuel emission changes can be detected from 14CO2 data if larger than 7–26% for five-year averages, being limited also due to interannual variations. This seems somewhat more pessimistic than your results. Can you add a comment whether (or to which extend) these results are compatible, and why? (reference: I. Levin et al, Naturwissenschaften (2008) 95:203–208, DOI 10.1007/s00114-007-0313-4)

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