

***Interactive comment on “Comparisons between  
SCIAMACHY atmospheric CO<sub>2</sub> retrieved using  
(FSI) WFM-DOAS to ground based FTIR data and  
the TM3 chemistry transport model” by  
M. P. Barkley et al.***

**Anonymous Referee #3**

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First I have not read any of the other reviewers comments so this is an independent 'pristine' review.

The work presented in this study is very impressive for a one-man's thesis presenting an improved retrieval method and its application to SCIAMACHY. Therefore the paper undoubtedly deserves to be published. It is also well organised and well written.

Main critique points (1) It would be a great help to add analysis that permit the reader to get a better sense of the information content in the retrievals / satellite data. In

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particular (i) please add more information on the reference mixing ratio fields used for the retrievals - specifically please add quantitative comparisons with records from NOAA/ERSL (formerly CMDL), (ii) Same for FTIR (i.e. compare with ESRL data to the extent possible - e.g. vertical profiles), (iii) same for TM3 simulations. (2) Often you say 'excellent agreement' - there is no basis for this. The main constraint on surface sources and sinks provided by atmospheric CO<sub>2</sub> is the annual mean surface interhemispheric gradient which is on the order of 2-3 ppm. Thus anything short of a precision AND accuracy of 0.5 ppm is nearly useless (I am sorry to say it so hard). (4% precision corresponds to 16 ppm !!) (3) Section 3 - the description of the retrieval is already pretty clear but could be further clarified still. In particular I had problems to understand why the partial derivatives are 'weighting functions' - where does the vertical dimension come in ? - and whether all pixels for a scene (not sure whether proper terminology) are processed in one least squares batch calculation or separately. If you could expand this section still a bit to make it as glass clear as possible to permit reproduction it would be great. (4) The coincidence of your spatial patterns with vegetation type seems a bit worrisome to me - is your method simply biased by surface reflectance ? Are you sure this is not the case ?

Minor points Abstract 'good agreement' - not justified statement line 20 'factor 2-3' - compared to what ?

Intro line 2 - 30 % is on the small side -I'd say ( $110/270 = 0.41$ ) line 15 ff While Gurney et al. may indicate this I think this is too optimistic still - we can constrain from atmospheric data lone 5 latitudinal bands - hardly more page 5390 line 3 I'd remove 'feasible' line 8 I don't believe this - I think in situ concentration measurements a la Keeling and Tans / Conwa (NOAA/ESRL) will remain a very important 'anchor'

p 5392 see main comments also: why do you minimize logarithms - can you give an easy explanation ?

p. 5393 you mention all the input into the SCIATRAN calculations - what are the calcu-

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lations most sensitive too ? This information would be a great help for me to understand better the approach and its problems

p. 5395 what does 'apodized' mean ? add a word ? line 23 Sentence starting with 'To ensure ..' remove 'is performed'

p. 5396 how sensitive are your calculations to the transformations - (3), (4) and (5) - what magnitude of errors / biases would you expect ?

p. 5400 line 8 - this is repeditive - but the agreement between the TM3 simulations and FTIR data is NOT excellent - rather somewhat disconcerting

line 15 - omit 'excellent agreement'

p. 5401 findings in the last paragraph seem a bit weird - can you add some process ideas what would generate the signatures ?

p. 5403 I had a bit trouble what you mean by 'precision error' - can you make this more clear in the text ?

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 5387, 2006.

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