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Interactive comment on "CO profiles from SCIAMACHY observations using cloud slicing and comparison with model simulations" *by* C. Liu et al.

Anonymous Referee #1

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In my opinion there are a number of major issues with this manuscript in its current form. The majority of those were also raised by one of the other reviewers (Joiner). Nonetheless I feel the need to repeat those as most of my concerns are not taken away by the response of the authors as published on 5 June 2013.

Main concerns :

1. I do not understand why the authors do not stick to the original SCHIAMACHY product they derive which are subcolumns in time. These represent physical quantities that people can easily understand, which unfortunately does not hold for the constructed

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PVCDs. These seasonally and spatially averaged PVCDs are very hard to interprete as the authors themselves also note when writing for example in the abstract 'the profiles retrieved from the CST have to be interpreted with care'. So why is this then done ?

Moreover, when showing PVCDs in Fig. 4 the use of PVCDs actually hides the problems related to this approach as becomes clear in the reply to one of the reviewers (Joiner) under point 4a and which was also noted in the manuscript (p.11665). With this method one can get negative mixing ratios under certain conditions, which obviously is physically impossible. By using the PVCD representation this is not directly obvious and the authors find this 'aesthetically' more preferred (as they write in their response 5 June 2013). From a scientific point of view I have to disagree. As a scientist I want to know where a chosen approach fails. More concerning is the fact that this product may thus give wrong/inaccurate results which may not be so obviously wrong such that it produces unphysical results, but which are nonetheless inaccurate. There is however no way of telling when and where this is the case. I do not know how to get out of this dilemma if one is not able to validate the product.

2. Validation of a satellite product is essential, so one knows what the product is worth. The response of the authors to this comment by Joiner is 'However, since the retrieved CO profiles do not constitute 'real' atmospheric profiles, but complex composites of CO measurements made under different meteorological conditions, we have some doubts that MOZAIC data are really appropriate for such comparison'. This is another good reason to stick to your original subcolumn product. This is a physical quantity that does constitute a real atmospheric subcolumn at a certain moment in time and as such can be validated by MOZAIC (or other data such as aircraft data from NOAA).

3. A back-of-the-envelope calculation shows that under all circumstances over land and with low cloud fraction there will be a VERY significant contribution coming from the non-cloudy part of the observation. In fact for cloud fractions <10% I expect in the majority of the cases that most of the signal is from the non-cloudy part. Take cloud

fraction 10%. The lowest surface albedo over land is ~5% in this wavelength range. Already in this case the contribution to the total signal of the non-cloudy part is as good as equal (if not larger) to the contribution of the cloudy part. Cloud reflectivity in this wavelength range does not get higher than ~50%, and is more often quite lower. Surface albedos over land vary between ~5% and ~60% in this spectral range, so in most cases over land –assuming 10% cloud fraction- the contribution from the non-cloudy part is in fact the largest contribution to the observed signal and I don't see how the approach taken in this paper would then work. Therefore, I expect you need to take cloud fractions of say >70% for this approach to work well everywhere over land. The reason why the results in Fig. 2 might suggest otherwise could be due to the fact that this is a mixture of cases over land and over ocean. Over the oceans the approach works for any cloud fraction [ref. Gouldemans, 2009] as the ocean surface reflectivity is very low («1%) (apart from sunglint situations).

The authors responded -in reply to a similar comment by Joiner- they will calculate the relative fractions of received signal from the clear and clouded part of the observations and will effectively re-do their analyses based on that approach. Also, they will provide cloud radiance fractions for the selected threshold values of effective cloud fractions for a set of representative surface types. I think this is very important.

4. Why is FRESCO used to obtain information on cloud top height, which implies the use of the oxygen band at a very different wavelength, and more particularly why is not methane used in the same spectral range where CO is measured ? (see also point 2 raised by Joiner.) The fact that in both spectral ranges the photons penetrate the cloud to some degree –as described in section 2.1- is not a very strong argument as the point is that the effective penetration will be different in the different spectral ranges.

5. Given the considerations above (in particular those under point 3) I do not know what the conclusions in section 3.4 w.r.t. model performances are worth. This needs to be re-assessed after the analyses is re-done as replied by the authors to the comments by Joiner (point 3 above).

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 11659, 2013.

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