

***Interactive comment on* “Evaluation of stratospheric chlorine chemistry for the Arctic spring 2005 using modelled and measured OCIO column densities” by H. Oetjen et al.**

Anonymous Referee #1

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The paper introduces SCIAMACHY OCIO columns and validates these columns using ground-based OCIO measurements made at three NH sites during spring 2005. Secondly, a comparison between the measured OCIO columns (ground-based and satellite) and model results is presented. Possible sources for the discrepancies between both modelled and measured OCIO are discussed in detail using sensitivity studies. The SLIMCAT-initialised stacked box model simulations of OCIO highlight our still incomplete understanding of chlorine chemistry especially at low sun angles and large chlorine activation.

Both, the presentation and validation of SCIAMACHY OCIO columns and the discus-

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sion of possible shortcomings of the model simulations are scientifically relevant results and I recommend this paper for publication in ACP after the following comments have been addressed.

General comments:

The authors conclude in the abstract that ‘overall, good agreement is found’ and in the summary ‘The agreement is excellent . . .’ which is not really well enough supported by the data comparisons shown in this paper. It helps that the uncertainties are quite high (uncertainty in SCIAMACHY OCIO is estimated to be 30% or higher and for ground-based OCIO quoted as 20% for 90 SZA) and I agree that the comparison is qualitatively good but quantitatively not quite as convincing (certainly not excellent) for the following reasons: 1) The g.-b. instrument operating at Bremen is not sensitive enough to detect the low amounts of OCIO present at the small SZA during the SCIAMACHY overpass and hence no direct comparison is possible for this station. 2) Based on Figure 3, the comparison at Ny-Alesund seems not that great for the first time period (high chlorine activation) of about 2 weeks (roughly 12-26 Feb), it then is excellent for the next time period of about 2 weeks (26 Feb – 14 March, modest chlorine activation), and for the 3. period (roughly 14-30 March) both instruments agree that there is no more OCIO. 3) Also based on Figure 3, at Summit the most days the g.-b. data set is substantially higher. So overall, there seems to be only one time period (with modest chlorine activation) at 1 of the 3 sites when the agreement is truly excellent and meaningful. At high chlorine activation, the g.-b. instrument sees generally more OCIO than the satellite. This could be discussed in a bit more detail in the text. And the agreement should be described more adequately in the relevant parts of the manuscript.

More detailed comments:

Page 26547, lines 8-9: The authors state that ‘in Ny-Alesund, apart from the first few days, the agreement . . . is excellent.’ As discussed above and based on Fig 3, it looks more like the first 2 week period is showing a rather pronounced offset between g.-b.

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and SCIMACHY data, at least while the chlorine is stronger activated. That should be taken into account in the text.

Page 26549, lines 14-15: The authors state: ‘However, the model generally underestimates the OCIO columns. This effect increases towards larger SZA but also with increasing chlorine activation.’ In the next section (page 26550, lines 20-21) the authors state: ‘At 90 SZA, the measurements exceed the modelled columns by 30% on 24 Feb and by about 40% on 4 March, respectively.’ Doesn’t this contradict the first statement (i.e. increase in the underestimation with increasing chlorine activation and not the other way around)??

Page 26552, lines 25-28: If the higher forward rate constant based on Boakes et al. would be used – which obviously would not lead to an improvement but rather the opposite – by how much would the modelled OCIO change?

Pages 26553, lines 11-13 & page 26554, lines 26-29: Could a mix of several scenarios lead to a better simulation of the OCIO diurnal variation as well as absolute amount?

Page 26557, line 4: ‘The agreement is excellent . . .’ As discussed above in some detail, I don’t think that is a realistic statement and needs to be adjusted.

Figure 5: Why do the model results for 90 SZA start later than the measurements?

Technical comments:

Page 26541, line 5: change to ‘... on the particle surfaces are ...’

Page 26541, line 8: change to ‘poles’

Page 26541, line 10: change to ‘One of the channels of the BrO + ClO reaction produces chlorine dioxide ...’

Page 26541, line 15: change to: ‘... leads to O + ClO, this reaction results in ...’

Page 26545, line 18: change to: ‘... spectrum of scattered solar radiation.’

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Page 26544, line 27: Better: 'The quality of the resulting fit is ...'

Page 26548, lines 3-4: probably a typo and should be '... large peak on 3 February ...' instead of 3 January

Page 26551, lines 14-18: 'This can be achieved in three ways: ...' These 3 options could be labelled with (1) – (3) and then used to refer to them later e.g. (3) on page 26552, line 22. This would make it a little easier to follow the discussion in this section.

Page 26552, line 22: If no labels are used, then I would suggest to add something like: 'As mentioned above, another way to shift ...'.

Page 26551, lines 22-24: The authors quote: 'The recent study by Chen et al. (2009) confirmed the absorption cross-section as obtained by Burkholder et al. (1990).' It would be good to add here that this is for 351 nm.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 26539, 2009.

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