

Interactive comment on “Balloon-borne limb measurements of the diurnal variation of UV/vis absorbing radicals – a case study on NO₂ and O₃” by L. Kritten et al.

Anonymous Referee #2

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General Comments

This is a nice paper that describes a novel technique for the retrieval of time dependent trace gas profiles from balloon-borne limb scatter measurements. It is generally well written and is certainly suitable for the scope of AMT. I recommend publication, especially if the following minor comments could be addressed.

Specific Comments

1) I wonder if the current title is the best choice for this work. It seems that the novel work here is in the presentation of the time dependent retrieval methodology for the balloon measurements, and not in the measurements of the diurnal variation of NO₂

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and O₃. I would suggest rewording the title to indicate this.

2) The "proper choice" of reference spectrum is referred to several times. It would be helpful to discuss how this is chosen in practice. For example, is it always the largest elevation angle? Does it vary with species (i.e. what about for O₃?) according to the height of the number density peak and the float altitude?

3) Although more detail is provided in later sections, it would be useful to expand upon the calculation of K in terms of L and C in the introduction to 2.3 as it seems unclear how they are related after reading this section.

4) Can you comment on why the correlations in DSCD's are (or can be) neglected and what effect this may have?

5) Is the technique for choosing the diagonal elements of the a priori covariance matrix new to this work? If not, please provide a reference. Also, how is the number of degrees of freedom calculated in this case? I realize that the time dependence has a contribution in this case but the numbers seem very large. More detail regarding the degrees of freedom calculation and interpretation would be helpful.

6) Can you clarify what you referring to as Type I and Type II oscillations with specific reference to Figure 6? The statement referring to 13:15 UTC and 14:15 UTC may be confusing as it is written. It is not clear why the numerical calculation of K_{EA} would fail. Can you comment on this please? Also, I completely understand the blurring and shifting interpretation of the effect of the two types of oscillations discussed on pages 447-448; however, I cannot see these effect as the authors state are illustrated by the profiles in Figure 6. Finally, the authors state this effect is more pronounced in the O₃ retrieval. Then why not show the O₃ retrieval results in the figure?

7) I would suggest that the authors carefully consider the use of the word "validation" for the work presented in 3.1-3.2. This is not necessarily a "validation" in the sense typically used in this field.

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8) It would be useful to explore how the independent retrievals of single balloon scans (performed in a typical sense) before and after the SCIAMACHY measurement compare to the result obtained at the SCIAMACHY measurement time using this new technique.

Technical Corrections

p 433 Line 7: move the reference to Ferlemann et al. to a more suitable place later in this sentence

p433 Line 20: do you mean Table 1?

p436 Equations 2 and 3: I think the subscripts '0' and 'o' are getting confused.

p440 Lines 12-13: I would suggest "variability" rather than "variety", "background" rather than "common", and "influences" rather than "influence".

p442 Equations 12 and 13: Is ":@" an accepted symbol?

p443 Line 443: Please be careful in what you mean when you state that the "a priori is a first guess".

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 431, 2010.