Atmos. Chem. Phys. Discuss., 3, S1454–S1456, 2003
www.atmos-chem-phys.org/acpd/3/S1454/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Inelastic scattering in ocean water and its impact on trace gas retrievals from satellite data" *by* M. Vountas et al.

W. Sturges (Editor)

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Received and published: 4 September 2003

Comments were received from an anonymous referee just after the official close of the open discussion on this ACPD paper. Since the authors have very kindly agreed to take these late comments into consideration in their reply, and in the revision of the paper, I have reproduced the comments below.

WT Sturges, Editor 4 September 2003

The authors present a study on inelastic vibrational Raman scattering (VRS) on ocean water. Such investigations are very important for several reasons: -for the correct determination of atmospheric trace gas columns from spectroscopic (satellite) observations -for the better understanding of the radiative transport through water -for the determination of properties of the ocean surface waters, e.g. the content of chloro-

phyl or geldbstoff. Therefore, I think that the subject is of high interest for the scientific community.

However, I have some concerns about the structure and the methodology of the paper. The main points are summarised below:

1. While the authors claim that their measurements over swimming pool water, from satellite as well as their modeling studies provide consistent evidence on vibrational Raman scattering as a potential source of error in the spectral DOAS fitting, this conclusion is only poorly proven by real experimental and modeling results. What I would expect here is a direct comparison of the residual structures (for the case that the vibrational Raman scattering spectrum is not included) extracted from GOME observations with those of the swimming pool experiment and the modeling results.

What is shown instead in the paper are fitting results for cases when the VRS spectrum is included. Because of the high spectral similarity between the VRS spectrum and the Ring spectrum, however, I think it is not a sufficiently convincing result that the VRS spectrum is ŚfoundŠ by the spectral fit.

2. I have some concerns about the consistency of the spectra shown in Fig. 3. It seems to me that both spectra were not obtained from the fitting of the same spectrum, because the residuals are different in shape and magnitude. The authors should clarify this.

3. Table 5 seems not to be well suited to present a clear overview of the fitting errors. I at least suggest to reduce and simplify this Table (see also point 4). What would be really convincing was the comparison of spectral fitting results, which should indicate a clear improvement when VRS spectra are included. Another convincing procedure would be to compare the improvement of including VRS correction for oligotrophic and non-oligotrophic oceanic regions.

4. I suggest to remove the section on the HCHO retrieval, since it adds no valuable

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additional information.

I would be happy if the authors could address these points, because the subject is of great importance for the scientific community.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2931, 2003.