

Interactive comment on “Actinic flux and O¹D photolysis frequencies retrieved from spectral measurements of irradiance at Thessaloniki, Greece” by S. Kazadzis et al.

Anonymous Referee #2

Received and published: 7 September 2004

The scientific questions addressed in this paper, the conversion of irradiance measurements to photolysis rates, are well within the scope of ACP. The new tools described and ‘validated’ are of importance for atmospheric chemistry and chemistry modelling. The scientific methods are described reasonably well, in some cases (see special comments) the paper would profit from a more detailed description. The agreement between direct photolysis measurement and converted irradiance measurement is astonishingly high unfortunately without giving more details about the limitations of the methods presented concerning the time resolution and atmospheric conditions under which these methods are valid. The paper is recommended to be published in ACP after revision.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

Special comments:

Previous related work is cited but not as carefully as desired. Citations are partly mixed up. One literature citation is missing in the literature list. Title and abstract describe the contents of the paper properly. The overall presentation is fairly well structured.

Introduction

page 3, last paragraph: to my knowledge Bahe et al did not measure with filterradiometers, at least not JO1D. Photolysis rate actinic filterradiometers were first developed by Junkermann et al, 1989.

– some typing errors in the citations– . Page 7, 3.1 description of the method. The method was developed by Kazadzis, the lead author for this paper. For this author the method is clear. Any other reader would need a better introduction on how the method works. In the formula and later in the text some parameters are given as wavelength dependent some not, although all parameters are wavelength dependent. The use of abbreviations should be consistent in the whole text.

Page 7, paragraph 3 A small graphic would facilitate the understanding of the argumentation, better than a table.

Paragraph 4, $A(\lambda)$ varies by far less with clouds than without. That's essential for the later evaluation of errors. Is there a physical reason that explains why the variability with clouds is lower than without?

Page 8. Citation Vasara et al. missing in the reference list.

Paragraph 4, The data in Figure 1 suggest that there are two well defined cases, one with constant ratio, one with zenith angle dependence. And some mixed cases in between. Is there any reason for this behaviour?

Section 3.3. Comparison I assume that the data obtained from the METCON spectrometer were produced in the ADMIRA campaign. Is this instrument provided and

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

operated by METCON?

Page 10, The discussion on the non ideal angular response of the METCON instrument and the uncertainty in the retrieval method due to this angular response needs to be more detailed. The METCON instruments has the same optical inlet systems as it is used also for other measurements within this paper for example with the Bentham monochromator.

Page 11.

The discussion about the unisotropy of the diffuse radiation field requires to be more detailed. I would assume a better isotropy for higher Aerosol Optical Depth. It seems to be contrary?

Page 12. Paragraph 2, The method is based on the assumption, .. Paragraph 3, The INSPECTRO project should be cited (web page) Paragraph 4, The single monochromators used in both the BREWER and in the METCON instruments may have additional uncertainties as the cannot suppress the straylight sufficiently. How is the picture changing when a real wavelength for the JO1D photolysis is used.

Page 14, INSPECTRO campaign.: Please give date and location for this campaign. There is an inconsistency between the text (17 day campaign) and the data in Fig. 8, 40 days of ATI data at Weybourne.

Page 18, paragraph 2:

Characteristics are much different from those at Weybourne. Can these differences be specified?

Page 18, Conclusions. The zenith angle dependence in the UVA band is not relevant for the JO1D. It would be nice to get some impression about the possibility to derive also JNO2 from the data.

Figures

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

all figures should be harmonized in the graphic layout.

Fig. 8. The data from Weybourne are give as Julian day (50 - 90) of year 2002. That's springtime. The INSPECTRO Weybourne campaign took place in fall 2002. or are this two different campaigns?

Some grammar errors.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 4191, 2004.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper