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Atmospheric Measurement Techniques Discussions

Interactive comment on "The added value of a visible channel to a geostationary thermal infrared instrument to monitor ozone for air quality" *by* E. Hache et al.

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We thank the reviewer for his/her positive and helpful review. Below we address the points raised by the referee.

Major comment

We agree with the referee's comments that these results are likely to be overoptimistic compared to a real instrument. Some assumptions made in the study neglect a number

of uncertainties that are likely to be found in real retrievals. The following is a point by point reply to the major comments of the reviewer:

• *As mentioned in the manuscript, clouds and aerosols are not taken into account in the simulations. In real applications, both might well be major problems and the possible effects of them and their different impact on TIR and visible observations need to be discussed at least qualitatively.*

– As discussed in the paper, we do not take into account clouds and aerosols. However, the quality of the measurements is highly sensitive to clouds. The instrument cannot measure surface ozone observations over cloudy pixels. However, because the goal of the instrument is to monitor air quality, in particular to monitor high ozone episodes, commonly associated with an anticyclonic situation (i.e., a cloud free situation), the results are likely to be applicable. In addition, this study is a comparison between two instrument configurations, and thus provides relative results.

The aerosols impact the radiances in the visible and degrade the quality of the retrieval, in particular in the Chappuis band, but because of the difficulty of accurately modelling and measuring their properties (Timmermans et al., 2009), we have chosen not to take the aerosols into account in this first approximation to the problem. Future improvements to this work will take into account the aerosol impact.

We have added these comments and the reference in the conclusions of the revised paper.

• *It is interesting to note that so far there is no publication applying ozone retrievals in the Chappuis bands to nadir satellite observations although such data is available since the launch of the GOME instrument in 1995. The reason is strong interferences from spectral signatures of surface and vegetation properties in this spectral range. As*

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long as this problem has not been solved, adding a vis channel to a TIR instrument adds little to O3 retrievals. This point should at least be mentioned.

– As the reviewer says, our study makes some assumptions about the surface properties. The study only considers a lambertian surface with a spectral albedo interpolated using a third order polynomial. This representation of the surface properties neglects the uncertainties that are likely in reality due to the Bidirectional Reflectance Distribution Function (BRDF) and to the spectral signatures of surface and vegetation properties (as highlighted by the reviewer, see Richter et al., EGU 2012). Better modelling and retrieval of the surface properties are needed, which will be done in our future studies.

We have added these comments and the reference in the conclusions of the revised paper.

• *While statistical errors have been taken into account in the simulations, the effects of mismatches between cross-sections, surface albedo and BRDF or ground scene coverage in the two spectral bands are not mentioned at all. However, none of these quantities is known to a percent level and this will introduce systematic and non-negligible uncertainties in real data. I therefore think such effects need to be discussed.*

– Finally, as highlighted by the reviewer, we also neglect the mismatches between the cross-sections used in the radiative transfer model and the real cross sections. Considering that measurements of cross-sections are made in the laboratory with errors which contribute to the retrieval error, these uncertainties could be significant (see Liu et al. 2007; Gorshelev et al. 2014). The quality of the retrievals relies also on improvements in the laboratory measurements and in the modelling of these cross-sections. Considering the role of surface albedo and the BRDF, we refer the reviewer to the previous

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point.

We have added the comment about the cross-sections and the references in the conclusions of the revised paper.

• *A lot of the discussion deals with averages over all daylight hours for a full week. It would be interesting to see a figure similar to Fig. 7, but for an individual 1 hour time step. As discussed in the introduction, for air quality applications weekly averages are not really appropriate.*

– As suggested by the reviewer, we have modified Fig. 7 to an ozone field at 12h instead of a weekly average. The corresponding comments for the revised figure have been incorporated in the figure caption and in the text.

Minor comments

• *Page 1648, line 9 – it might be worthwhile to also mention biogenic precursors which increase at high températures*

– We now mention the biogenic precursors.

• *Page 1660, line 14: compare to => compared to*

– We have corrected the typo.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 1645, 2014.