

## ***Interactive comment on “A geostationary thermal infrared sensor to monitor the lowermost troposphere: O<sub>3</sub> and CO retrieval studies” by M. Claeymaen et al.***

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The authors thank very much Referee #1 for his helpful and constructive review of our paper. Below, please find the reply to the specific comments.

Referee #1 This paper presents a thorough analysis of the sensitivity of two instrument concepts to CO and O<sub>3</sub>, with an emphasis on comparing sensitivity in the lowermost troposphere. The paper is very well constructed, with a clear motivation, background, and presentation of results. Conclusions are drawn from the analysis, and presented in a useful way.

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Specific comments: 1- the paper talks about a geostationary mission, because of the driver of the SENTINEL and GEO-CAPE concepts, yet the only driver they discuss is the need for hourly measurements. They should consider talking about the integration time that would be needed for the measurements they are discussing and how that could or could not work for low earth and geostationary orbits.

Our reply: In this study, as the referee points out, we focused on the need for hourly measurements adequate for air quality purposes. The aim of this paper is to study the information content that we can have in ozone and CO concentrations in the lowermost troposphere from different instrument configurations. We used the following geostationary configurations: - Same pixel size corresponding to our AQ model grid 0.5° x 0.5° - Same integration time of ~0.4 s which is widely acceptable - Same spectral band in the TIR - Same field of view over Europe - Same observation frequency of 1 h which is not achievable using one Low Earth Orbit satellite. Calculations show that we need between 3 to 12 LEO satellites to cover partially Europe (see figure 1), even on a trajectory optimized for a maximum number of revisits, as was proposed in the context of the TRAQ mission ([http://www.esa.int/esaLP/SEM712CDNRF\\_LPfutureemis\\_0.html](http://www.esa.int/esaLP/SEM712CDNRF_LPfutureemis_0.html)).

The only parameters that differ between the GEO configurations considered in this paper are the SNR and the spectral sampling interval (SSI), which likely play the most important role in the measurements sensitivity to the lowermost troposphere for AQ applications. The presentation of the two configurations are now detailed in the new version of the paper (new paragraph entitled ‘instrument configurations’).

2- the a priori and constraints can significantly impact the results in optimal estimation retrievals, yet they are not discussed in the paper. It is important they be identified, if not shown in a figure.

Our reply: As suggested by the referee we discuss the point of the a priori of our retrieval in the new version of the paper. We add a figure (Fig 1 in the paper) showing the a priori profile and its variances for CO and O<sub>3</sub> especially estimated by the AQ

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MOCAGE model over Europe and during summertime. Moreover, we used the same a priori which impact in the same way all instrument configurations.

3 - section 1, starting lines, a 2010 reference is given for air quality - certainly there is a better reference, since this has been an area of science and policy for decades. I would use one of the early books or reports - such as Seinfeld or Jacobson.

Our reply: As suggested by the referee we add the reference Seinfeld and Pandis, 1997, John Wiley and sons.

4 - page 5, line 14, the pollutants of interest early in the introduction are ozone, NO<sub>x</sub>, and PM. Here on page 5, lines 14, they are O<sub>3</sub> and CO - you should say why you focus on these two - later you discuss the UVN, but you may need to point to it here.

Our reply: In the introduction we detailed more the interest of O<sub>3</sub> and CO for the AQ purpose. We think an AQ mission dedicated to O<sub>3</sub> and CO is very complementary to the expected or existing missions capable of measuring the other AQ main species (Nox and aerosols) like S4-UVN. We reorganized the introduction in this way.

5 - page 6, line 14 - you mention mission concepts in Europe, US, and Japan. How about GEMS - the Korean plan - <http://adsabs.harvard.edu/abs/2009AGUFM.A53A0251L>

Our reply: As suggested by the referee we indeed added the web site reference of the GEMS Korean mission in the listing of future geostationary mission in the introduction of the new version of the paper.

6 - page 7, line 13 - I would use a phrase other than embarkation - maybe deployment??

Our reply: As suggested by the referee we used in the paper the word deployment.

7 - page 11 lines 1-5. You talk about IASI and TOMS/OMI measurements of ozone and variations in the lower troposphere. TES should be included in this discussion. A suggested paper is Zhang, L., D. J. Jacob, X. Liu, J. A. Logan, K. Chance, A. El-

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dering and B. R. Bojkov, Intercomparison methods for satellite measurements of atmospheric composition: application to tropospheric ozone from TES and OMI, *Atmospheric Chemistry and Physics*, 10, doi:10.5194/acp-10-4725-2010, May 26, 2010. or Yang, Qing, Derek M. Cunnold, Yunsoo Choi, Yuhang Wang, Junsang Nam, Hsiang-Jui Wang, Lucien Froidevaux, Anne M. Thompson, and P. K. Bhartia, A study of tropospheric ozone column enhancements over North America using satellite data and a global chemical transport model, *Journal of Geophysical Research*, Vol. 115, D08302, doi:10.1029/2009JD012616, April 30, 2010.

Our reply: As suggested, we include TES in the discussion in the paragraph 'infrared instrument capabilities for O<sub>3</sub> and CO'. We proposed another particularly well suited reference using tropospheric O<sub>3</sub> and CO vertical profiles from TES to monitor Mexico City pollution outflow: Shim, C., Li, Q., Luo, M., Kulawik, S., Worden, H., Worden, J., Eldering, A., Diskin, G., Sachse, G., Weinheimer, A., Knapp, D., Montzka, D., and Campos, T.: Satellite observations of Mexico City pollution outflow from the Tropospheric Emissions Spectrometer (TES), *Atmos.-Environ.*, 43, 1540–1547, 2009

8 - page 15 - line 19 - the statement that the surface is well covered by the AQ network attempts to sweep away a significant issue. Do we really know how dense of a surface network is needed for AQ prediction? Will the satellite data improve AQ prediction or not? I would suggest being less conclusive with this statement. It is true that there is a tradeoff of surface measurements and satellite measurements, but I don't see evidence that we know much about that trade space, nor was it explored in this paper. "However, both instruments have generally low sensitivity to the surface. We note that the surface is already well covered by observations from European AQ networks (Honoré et al, 2008)."

Our reply: We change the original sentence to be less conclusive as suggested by the referee by the following one: "However, both GEO instruments have generally little information at the surface. Such information may be provided at particular locations by surface observations from European AQ networks." Moreover the information in the

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LmT provided by the GEO-TIR instrument could impact the surface by transport and mixing via data assimilation in the AQ model.” (see paper submitted in AMTD and provided as supplement file).

9 - page 22, lines 25-27. The sensitivity of TIR retrievals of ozone to the surface temperature contrast was well described in Landgraf and Hasekamp - this work should be integrated in your discussion and cited. Landgraf, J., and O. P. Hasekamp (2007), Retrieval of tropospheric ozone: The synergistic use of thermal infrared emission and ultraviolet reflectivity measurements from space, *J. Geophys. Res.*, 112, D08310, doi:10.1029/2006JD008097.

Our reply : As suggested by the referee, we added this reference and integrated these results in the discussion in section 3.1 where we introduce the impact of thermal contrast on GEO-TIR and GEO-TIR2 results.

10 - page 23, line 230 would say 'we will perform observing...' rather than we will make.

Our reply: We change the original sentence as suggested by the referee.

11 - page 24 line 3 - where Worden et al is cited, would also cite Landgraf and Hasekamp - the papers we published about the same time and cover similar lines of investigation.

Our reply: We add this reference as suggested by the referee.

12 - Figure 11 - I like the time series as a way to illustrate the results, but the scale should be modified - the dynamic range of the data is only half of the scale of the figures - perhaps the line colors indicated in the legend are enough, and the legends on each figure can be removed?

Our reply: The scales of the figures have been changed.

Overall, this is a well presented analysis, with clear conclusions. It is a significant contribution to the characterization of potential satellite measurement of key air

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pollutants.

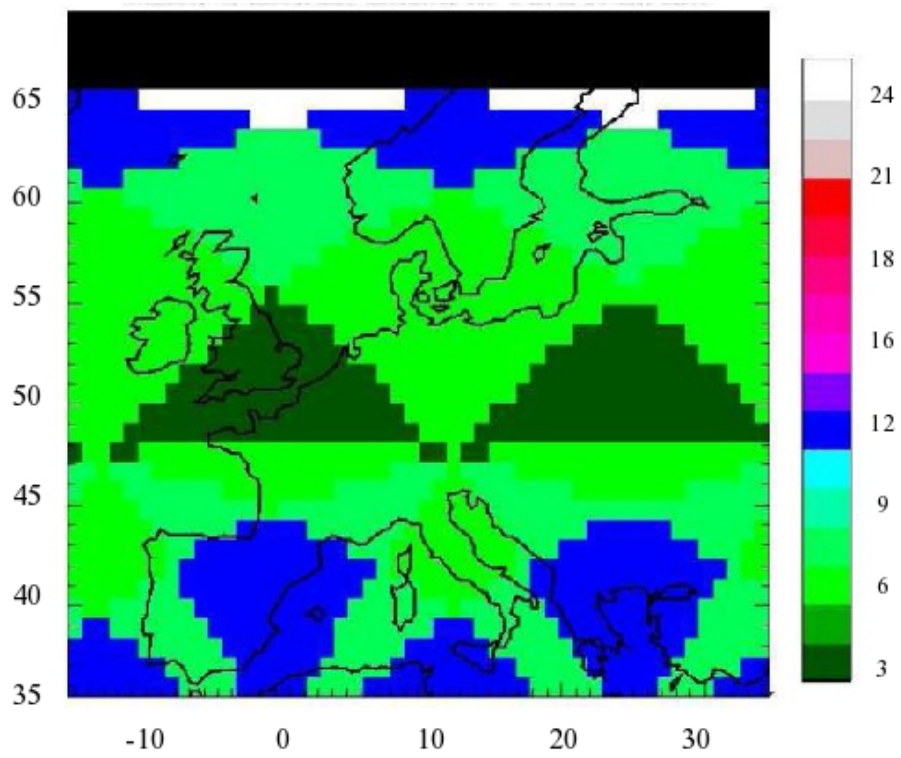
Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/3/C2202/2010/amtd-3-C2202-2010-supplement.pdf>

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Interactive comment on *Atmos. Meas. Tech. Discuss.*, 3, 3489, 2010.

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**Fig. 1.** Useful LEOs optimized (drifted orbits) required for 1 hour revisit time over Europe with a  $1^\circ \times 1^\circ$  resolution ( $\sim 100$  km)

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