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Atmospheric Measurement Techniques Discussions

Interactive comment on "A geostationary thermal infrared sensor to monitor the lowermost troposphere: O_3 and CO retrieval studies" *by* M. Claeyman et al.

Anonymous Referee #1

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This paper present a through analysis of the sensitivity of two instrument concepts to CO and O3, with an emphasis on comparing sensitivity in the lower most troposphere. The paper is very well constructed, with a clear motivation, background, and presentation of results. Conclusions are drawn from he analysis, and presented in a useful way.

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.

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 the prior and constraint be identified, if not shown in a figure.

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.

4 - page 5, line 14, the pollutants of interest early in the introduction are ozone, NOx, and PM. Here on page 5, lines 14, they are O3 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.

5 - page 6, ine 14 - you mention mission concepts in europe, us, and Japan. How about GEMS - the Korean plan - http://adsabs.harvard.edu/abs/2009AGUFM.A53A0251L

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

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.

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.

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

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.

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?

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 pollutants.

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