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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.

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The authors are grateful to referee #2 for the report he/she has provided. His/her remarks and comments have been of great importance to us and have allowed us to improve the quality of the paper. Below, please find the reply to the major comments as well as the other comments.

Referee #2

This paper reports on the capabilities of a thermal infrared FTS remote sensor to measure O3 and CO for air quality purposes, using the geostationary orbit. The study uses the instrumental specifications of a planned instrument (MTG-IRS, here called GEO-TIR2) to compare it to a concept of mission (GEO-TIR) and shows that when both the signal to noise (SNR) and the spectral resolution are improved, one can gained sensitivity towards the lower layers of the atmosphere. Some model simulations are also provided to demonstrate the usefulness of geobased observation, at least when thermal contrast is significant. I find the paper clear and well written. But I also find the scientific content weak and some parts of the paper are very misleading. I would not recommend the paper to be published as it is.

Major comments: 1/ About the GEO-TIR versus GEO-TIR2 comparison The goal of the paper is to push a concept of an improved TIR mission, which (and I fully agree on that) should be useful for AQ purposes. This is done by comparing a potential instrument (GEO-TIR) to a planned mission, here called GEO-TIR2, and that is in fact MTG-IRS. But the study is limited to the comparison of the instrumental specifications only, whereas other criteria such as temporal and horizontal samplings are equally important. The pixel size is also crucial, and as the complexity/cost of an instrument depends on all this factors together, a smart compromise between all these criteria has to be found to define a useful mission.

Our reply:

In this study, as we already answered to referee 1, we focus on the need for hourly measurements adequate for air quality (AQ) 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 geostationary instrument configurations. The following features are shared by the geostationary configurations that are compared:

- Same pixel size: corresponding to our AQ model grid of 0.5°x 0.5°; - Same integration time of $\sim$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.

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. GEO-TIR2 has, in fact, only the SNR and the SSI from MTG-IRS, and that is why we do not call it MTG-IRS in the paper. Similarly, because GEO-TIR has only the SNR and the SSI of the TIR instrument of MAGEAQ, we do not call it MAGEAQ.

Furthermore, in this paper we focus more on the vertical sensitivity of both instruments and less on the horizontal sensitivity which is, by construction, limited by our experimental context. In any case, state of the art AQ models are not able to represent the O3 and CO horizontal variability at 4 km over the European domain. In MACC, for instance, models that cover Europe have currently a resolution of 0.2°x0.2° ($\sim$20 km). In this paper, as we focus on the relative performance of the GEO-TIR and GEO-TIR2 instruments, we do not detail the complexity/cost of the instrument or the instrument concept, but we focus on the TIR satellite observations we need for AQ and, in particular, fundamental instrument specifications such as SNR and SSI. However, we checked with the ASTRIUM-EADS team that the configuration chosen for GEO-TIR is technically and economically feasible. The description of these two configurations (GEO-TIR, GEO-TIR2) is now detailed in the new version of the paper (new paragraph entitled 'instrument configurations').

The study provides obvious conclusions, such as p3503: "For both CO and O3, Fig. 5 shows that the GEO-TIR2 AVKs are broader than GEO-TIR ones, especially in the LmT, which confirms that GEO-TIR has a better vertical resolution". People involved in the field all know that a better SNR and better spectral resolution helps to lower the errors and increase the DOFS.

Our reply : We remove this sentence.

The paper spends a lot of time to re-demonstrate that, without properly compare the 2 instrument concepts (ie: a better spectral resolution has a cost in terms of acquisition time for each measurement, so e.g. how does it impact on the horizontal resolution,



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also a very important variable for AQ). IASI and AIRS are examples of such compromises, what you loose in terms of vertical information (eg as compare to TES), you gain in terms of horizontal resolution.

Our reply : In the first part of comment 1 (see above) we explain what instrument configurations we have considered (see new paragraph entitled ‘instrument configuration’). We have limited our study by using the main instrument parameters to get information in the LmT, and taking into account the constraints of the AQ model (namely, the horizontal resolution). In particular, we focus on the sensitivity in the lowermost troposphere, which in our view is the highest priority for AQ purposes. To do this, we use the TIR spectral band for both ozone and CO. Taking into account all possible compromises associated with a satellite mission (e.g. between acquisition time, spectral resolution and pixel size) is a worthy but complex task. We think this paper is an important first step toward such a task, and that appropriate refinements can be done later, building on the results from this paper.

Another example of misleading sentence: Page 3502 at the end: We simulate the observations from an instrument configuration close to MTG-IRS, which is dedicated to NWP (temperature and humidity), referred as GEOTIR2, to determine the added value of GEO-TIR. Here again only the SNR and spectral resolution are used in the comparison (is it why there is “close to”?).

Our reply : We modify this sentence and remove “close to”. As already mentioned, we now detail and clarify the configurations of the instruments considered (GEO-TIR, GEO-TIR2) in the new version of the paper.

2/ About the use of DOFs Much of the discussion is based on DOFs values. These figures in absolute values do not mean much, unless they are obtained using similar a priori and retrieval assumptions (which is the case here for “GEO-TIR” and “GEO-TIR2”). There is too much emphasis put on the values, that could be changed easily by eg allowing for more variability in the a priori.

Our reply:

As the referee mentions, the DOF can be modified using different a priori, retrieval schemes or regularization values (in particular for the Tikhonov-Phillips method used in this paper). Using the same retrieval method and the same a priori for the GEO-TIR and GEO-TIR2 configurations allows for easier evaluation of the information provided by the measurements. This allows us to show the added value between instrument configurations. We now include a discussion of this point in the paper (see 2.2 Retrieval scheme)

P3501: Considering current IR instruments, technical feasibility and cost (Astrium-EADS, personal communication) a DOF of ∼1.5 for O3 and of ∼2 for CO seems to be a good compromise to have vertical information in the troposphere. It would have been more useful to make the discussion on the errors in the PBL (which is also done, but more importance is given to DOFs)

Our reply : The DOF (which is the trace of the averaging kernel) provides information on the averaging kernel and, indirectly, on the smoothing error. Following the referee, we also consider the errors in the LmT in Figures 3b, 4b, 7g-j, 8g-j.

3/ About the focus of the interest on Lmt : All the intro focuses on the fact that data are needed for AQ purposes, but several plots are presenting results for 0-15 km. Then the discussion should be limited to 0-3 km, or at least to the low troposphere. I doubt that what's happening above 8-10 km is influencing the PBL. Eg the title says "to monitor the lowermost troposphere", but: Figure 1 (2) is for 0-15 km; Figure 3 (4) is for 0-20 km, as well as Fig 5 Figure 7 and 9 are useful Figure 8 (10) a, b, c, d are for 0-15 km Figure 8 (10) e,f,g,h are useful Figure 11 is useful

Our reply : In Figures 1 and 2 (2 and 3 in the new version of the paper), we consider the DOF between the surface and 15 km to have values that are physically understandable (a DOF between 1 and 2 means more than 1 piece of independent information). We then check the shape of the corresponding averaging kernel in figures 3, 4 and 5 (4, 5



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and 6 in the new version of the paper).

In these figures, we plot the averaging kernels between the surface and 20 km to have the full shape of the averaging kernels (maximum and minimum) at different levels, especially the ones corresponding to the LmT. The discussion in the paper focuses on the averaging kernels corresponding to altitudes between the surface and 3 km. Moreover, we redraw Figures 8abcd and 10abcd (9abcd and 11 abcd in the new version of the paper) by calculating the DOF between 0-3 km to focus on the LmT.

Last but not least: to answer the question if these high resolution measurements from a geo platform would provide added value as compared to the current ground-based and polar orbit satellite data, one need to perform specific OSSEs. In the conclusion section the authors announce that these studies will be available. I think these results are really needed here, otherwise the whole context of AQ description and discussion with the improved GEO-TIR instrument is meaningless.

Our reply : The authors fully agree that a specific study using OSSEs complements this paper. A companion paper (see attached document) is submitted to AMTD. However, these two papers have different objectives: the present one evaluates the information content in O3 and CO concentrations of geostationary TIR instruments in the LmT; the second one evaluates the added value of such instruments in the context of a state of the art AQ model. We think that combining both papers into one would make for a very unwieldy paper and, therefore, one that is less useful.

Other comments: General: - Emissivity is assumed to be 1 in the simulations? It is worth noting than if not one it changes the conclusions on the value of thermal contrast more favourable to see lower in the atmosphere.

Our reply : We confirm that the emissivity is assumed to be 1 in the simulations and that, if not 1, it can change the impact of the thermal contrast. We assume that the uncertainty due to the emissivity (which does not vary much over Europe and is close to 0.98) is low compared to the uncertainty of temperature in ARPEGE analysis (which we use as the surface temperature in our studies). Furthermore, as we use the same emissivity for GEO-TIR and GEO-TIR2, the relative difference between both satellites is meaningful. We add this comment in the paper (see 3.2 -Geostationary system).

- MTG-IRS should be called MTG-IRS, not GEO-TIR2, everywhere. A reader not aware of the Eumetsat mission program will be very confused.

Our reply : We clarify in the paper the differences and similarities between GEO-TIR2 and MTG-IRS, and between GEO-TIR and MAGEAQ in paragraphs "2.4 instrument configurations" and "3.1 Optimum instrument characteristics onboard a geostationary platform". As the referee mentions, since GEO-TIR2 has only the SNR and the SSI from MTG-IRS (similarly for GEO-TIR and MAGEAQ), we prefer to keep the distinction between MTG-IRS and GEO-TIR2.

- Section 3.2, Geostationary observation system: the model simulation is performed at the model grid resolution. But an instrument with a pixel of eg 4 km (as IRS-MTG) will provide more horizontal structures than an instrument with a larger pixel. Is GEO-TIR also 4 km? The model simulation is done with a larger grid, so it can not show the horizontal detailed structure.

Our reply : We fully agree that a TIR instrument with a pixel size of 4 km can provide better detail of the horizontal structure for AQ purposes if it is sensitive to the LmT. However, state of the art AQ models are not able to simulate variability of CO and O3 concentrations with a spatial resolution of 4 km over the whole Europe. To our knowledge, there is no AQ model running routinely over Europe at such resolution and considering a significant number of vertical levels in the troposphere. We thus use the same pixel size for both GEO-TIR and GEO-TIR2 ($0.5^{\circ}$x$0.5^{\circ}$).

Page by page: - Abstract: I find it a bit too long + see my comments before on LmT versus 0-15 km

Our reply : We modified the abstract as suggested by the referee.



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- In the introduction, p3492, it is reported that $O_3$, $NO_x$ and PM are of great concern. But the paper focuses on CO and $O_3$, it should be better justified why.

Our reply: This remark was also made by referee #1 and now we justify better the focus on CO and $O_3$ in the introduction of the new version of the paper.

- Intro, page 3494, lines 5-12: True for the advantages of a geo platform but it should be mentioned that the observation is limited to 1/3 of the Earth

Our reply: As suggested by the referee, we mention this limitation of a GEO platform.

- Intro, Page 3494 the second para is very misleading: GeoTrope- GeoFIS, GeoCape, and MAGEAQ are mission concepts, whereas the MTG satellite is in phase B. This mission is not well described here (it is better described after): the launch dates should be checked on the Eumetsat web site (not 2017 for MTG-S), Sentinel 4 is the UVN instrument, not a full platform, and the MTG-IRS instrument is not even mentioned here (whereas the info is provided on page 3503).

Our reply : We have clarified the status of the missions and introduced the MTG-IRS mission in the introduction. The web site of EUMETSAT: (http://www.eumetsat.int/Home/Main/Satellites/MeteosatThirdGeneration/MissionOverview/index.htm?l=en) indicates a launch date from 2017. We left this date unchanged but added "from 2017".

- Page 3497: retrieval is performed from 0 to 39 km: what is assumed above 39 km for $O_3$?

Our reply : Above a height of 39 km, the forward model and retrieval schemes use a climatology; we assume this does not impact results in the LmT. We add this comment in the new version of the paper (see "2.2 –retrieval scheme").

- Page 3500: Note that the DOFs depend on the instrument configuration but also on the retrieval method : it also depends on the a priori (covariance matrix) used in the retrievals



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Our reply: The Tikhonov-Phillips method does not use an a priori covariance matrix, only an a priori. However, we agree with the referee that the DOF depends on the retrieval method and the a priori. We add this comment in the paper (see paragraph 3.1)

- Page 3500, lines 26-30: the information content at eg. 2 or 3 km would be more useful that DOFs 0-15 km

Our reply : In the new version of the paper, we calculate the DOFs in figures 9 and 11 between 0-3 km to quantify the information content in the LmT. However, we still present the DOFs between 0-15 km in figures 2 and 3 to have values that are physically understandable (see comment above).

- Page 3503: model grid used not provided

Our reply : We provide the model grid used (see paragraph 3.2)

- Page 3505: I don't understand why the model fields are not smoothed by the avgk.

Our reply : We do not apply the averaging kernel to the model fields to evaluate the total error (note that the total error includes the smoothing error - see eq 7 in the paper).

- Page 3506, line 3-5: this depends on the (vertical) correlation length for the cov matrix

Our reply: Since for the Tikhonov-Phillips method we do not use an a priori covariance matrix, this depends in fact on the regularization parameter. However, we add a comment in the paper (see 3.3.1) stating that there is a strong vertical correlation in the covariance matrix (Sx in eq 7).

- Page 3508 : I like Figure 11 and this plot is more useful than the previous plots to evaluate the impact on AQ

Our reply : As already suggested by referee #1, the scales of the figures have been changed.

- P3510 Suggestion to remove or modify: We confirm that the shape of the averaging kernels of TIR instruments is highly dependent on the thermal contrast, as this was shown on real measurements in several publications, it is expected that simulations do not show the contrary.

Our reply : We remove this sentence.

- Page 3510: O3 and CO distributions over Europe as measured by GEOTIR and the future GEOTIR2 are simulated: as said earlier I don't think the simulation takes into account all the specifications of the MTG-IRS mission.

Our reply : We fully agree with the referee that the GEO-TIR2 synthetic observations do not take into account all the specifications of the MTG-IRS mission; this is why we call it GEO-TIR2. We removed "future" in the sentence, which is not appropriate.

- Similarly, on page 3511: We have shown that such a configuration (GEOTIR) is capable of bringing added value in the LmT compared to a configuration optimized for numerical weather prediction (GEO-TIR2).Same comment, the demonstration should be done using proper pixel size, spatial resolution, temporal resolution. Eg for MTG the frequency of obs is 1/2h, here it is assumed to be one hour. By accumulating the data on one hour you will improve the SNR. . .

Our reply : As already mentioned above (see answer to comment 1), we focus on the impact of different SNR and SSI for an observation frequency of 1 hour, which corresponds to the threshold for MTG-IRS (see web site: http://www.eumetsat.int/Home/Main/Satellites/MeteosatThirdGeneration/Instruments/index.htm?l=en). We are aware that the observation frequency could be less for both MTG-IRS and MAGEAQ. However, as already mentioned, GEO TIR and GEO-TIR2 (the configurations considered in this paper) are different from MAGEAQ and MTG-IRS, respectively. Using the instruments GEO-TIR and GEO-TIR2 as defined in this study, we evaluate the impact of an improved SNR and SSI to monitor the LmT, and we do not consider other parameters. Using the full configurations of both instruments one could build on the results in this paper, but this is outside the scope of this paper.

- Fig 1: in the text the authors use SSI and here spectral resolution

Our reply : We now use SSI throughout the paper.

- Fig 1: I don't understand why the spectral res value for GEO-TIR2 is not 0.625 as in Table1

Our reply : The spectral sampling interval of GEO-TIR2 is 0.625 and the FWHM (non-apodized) is 0.8. We changed the figures to reflect this (see Fig 2 and 3 of the new version of the paper) and put in the SSI value.

- Fig 3: The text (page 3501 l23) says thermal contrast=2K (not zero as here)

Our reply: We changed the text; the thermal contrast is effectively 0 K.

- Fig 7 and 9: avgk should be used to compare the obs. with the model.

Our reply : We do not apply the averaging kernel to the model to quantify the total error (see previous comment).

Typos: -Page 3504 : . . . theseS radiances (remove S)

corrected

- Page 3500 (-2K Fig 2a and b) » 2K Fig 1b and 2b

corrected

So, in summary my recommendation would be to add realistic model simulations that take into account all variables, ie SNR, spectral resolution, pixel size, horizontal sampling, and frequency of the observation to demonstrate: - 1/ the benefit of GEO-TIR on MTG-IRS;

The purpose of this study is to evaluate and quantify the added value for CO and O3 concentrations in the LmT from improved SNR and SSI values by considering several



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instrument configurations and, in more detail, 2 particular ones: GEO-TIR and GEO-TIR2, equivalent in terms of SNR and SSI to MAGEAQ and MTG-IRS, respectively. But the comparison of results provided by the full MTG-IRS (including pixel size or revisit time) to the ones provided by the full MAGEAQ mission (which also includes a visible channel) is a huge task, and our approach is to address this problem step by step. We envisage carrying out future work where we refine this study and consider the full characteristics of MAGEAQ together with appropriate modification of the AQ model (e.g. horizontal resolution, parametrization of sub-grid scales).

- 2/ the usefulness of such measurement to improve AQ forecast And that would be a nice and useful paper.

We fully agree with the referee that a study using OSSEs can complement this paper. In fact, such an OSSE study is presented in a companion paper to this one. We add in an attached supplement a study, submitted to AMTD, to evaluate the added value of GEO-TIR and GEO-TIR2 in an AQ model.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/3/C2209/2010/amtd-3-C2209-2010-supplement.zip

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