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Comment on amt-2022-305

Anonymous Referee #1

Referee comment on "Diurnal carbon monoxide observed from a geostationary infrared hyperspectral sounder: First result from GIIRS onboard FengYun-4B" by Zhao-Cheng Zeng et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-305-RC1, 2022

Review of Zeng et al., 2022

This paper describes CO retrievals from the geostationary hyperspectral infrared sounders GIIRS onboard FY-4B. This is the first publication presenting CO retrievals from a geostationary platform that could be valuable to document the diurnal cycle of this species in the lower troposphere. The paper is correctly writen and structured with some interesting information. It is therefore adapted to AMT.

Nevertheless I have some important concerns and questions that have to be adressed before publication. My most important concern is about the diurnal cycle itself which is the main topic of the paper and included in its title. It should be better documented and compared to other datasets to be validated at minima.

CO a priori profiles :

The advantages and inconvenients of a climatological a priori are mentioned in section 7.1 and the use of a single a priori as a way to improve the algorithm to detect anomalies. It should also be mentioned that using such an a priori makes the retrievals more complicated to interpret and to use for model validation.

The use of a 3 hourly profile climatology based on 5 years simulation is done to help to provide the correct diurnal cycle to the retrieval algorithm. But CO is a pollutant with a lifetime much larger than a day. The daily cycle for CO is not as important as for NOx. The authors should provide the plots of the daily variations of CO in the troposphere and lower troposphere in the 3 selected regions from the ECMWF CAMS for instance together with the surface and bottom air temperatures in Fig 2.

Figure 4 shows that the same low biases for high concentrations in the a priori are partially kept in the retrieval for North China Plain and Mongolia. The dissapearence of these biases when the AvKs are applied to the « true » profiles clearly indicates that these biases are linked to the a priori and the lack of sensitivity of the sensor/retrieval to the polluted BL. As stated by the authors, this problem could be related to the too tight a priori covariance matrix used with the climatological profiles but it is not sure. It would be very interesting to provide some results from a simple test using a single a priori profile and its more loose a priori covariance to verify this assumption. In that case the signal to noise ratio for the retrieval which has been tuned according to the a priori covariance matrices (section 6.1) should be lowered which could lead to a destabilisation of the retrieval and possible oscillations in the profiles.

What diurnal cycle ?

The problem to document the CO diurnal cycle with the GIIRS retrievals come from the fact that it could be linked to :

- the real CO cycle that is the objective

- the variability of the BL layer with probably a better detection of pollution in the afternoon when the BL is higher where the sensor is more sensitive

- the variability of the DOFS

In order to disantangle these different sources of diurnal CO cycles

- the diurnal cycles of CO total columns over the 3 selected zones should be provided clearly the same way as the DOFS in Fig 8. The plot of the DOFS for the 0-1 km could be removed as the retrieval for this layer provides no relevant information (see next comment) and as its diurnal variability is mostly similar to the total column.

- the diurnal cycle of the BL height could also be documented from ECMWF ERA5 data for instance.

- the diurnal cycle of CO from other sources such as local pollution networks in China/ Beijing area, ECMWF CAMS used for the a priori (see above) and some references to relevant publications should be provided to check wether or not GIIRS retrievals are sensitive to BL pollution diurnal variability.

I have some doubts about the diurnal variability displayed in fig 10 and 11 in the NCP : the maxima are detected between 16 and 22 UTC that is between midnight and 6AM Beijing time (If I understood correctly). So it does not correspond to the time of day (i) with the highest activity where we expect the largest emissions and concentrations (this should be highlighted by surface /CAMS data as proposed above) (ii) with the highest BL which is in the afternoon (iii) with the largerst DOFS which is the begining of the afternoon (see Fig 8 and Fig 10 b). The authors have to provide some explanations about this peculiar diurnal cycle.

Why 0-1 km layer ?

The 0-1 km could be interesting to document BL pollution but it is characterised by a very low DFS mostly below 0.1 to 0.15 (Fig 3 and 9) and below 0.125 on Fig 8 which means that there is almost no information about this layer in the retrieval whatever the thermal contrast. DOFS is even negative (what does that means?) in Fig 8 and 9 showing that this layer is absolutely not a good choice.

The sentence line 400 « The DOFS can be as large as 0.3 providing a strong constraint on the bottom 0-1 km » is a flagrant overstatement (just a couple of points at 0.3 in Fig 9!!!) and should be removed or changed. Even a DOFS of 0.3 would have meant that the information for this layer is low.

In Figure 7 that displays the AvKs we see that the AvKs peak at 800 or 700 hPa in the best cases.

I therefore do not see the relevance to display results about the 0-1 km layer in the different figures. As the DOFS for the total columns are roughly between 0.8 and 1.2, the authors should separate the atmosphere/troposphere in the two layers in which the information is equally provided and display results for the lowermost of those layers.

Comparisons with IASI :

The comparison with IASI data is made to partly validate the diurnal variations but some important information is missing :

- as there are only two overpasses of IASI daily at 9:30 LST AM and PM, the authors have to detail how they average the GIIRS data temporaly which is not clear at all.

- the correlation coefficients and rmse are given in the Fig 12 but a table with those figures and other basic statistics such as mean biases +/- rmsd should be added.

- the comparison methology to smooth IASI with GIIRS AvKs is assuming that IASI has a much better vertical resolution than GIIRS which is not the case (IASI has probably a DOFS of 1.5). In that sense it is worth to display IASI AvKs to compare with GIIRS (as in Fig 7) and to provide IASI's DOFS. It would probably be better to avoid to apply equation 11 assuming that both sensors have similar vertical sensitivity or to use the more (too) complicated methodology detailed in Rodgers and Connor, JGR (2003).

- IASI CO "diurnal cycle" is mostly related to its decreased sensitivity at night. So the agreement with GIIRS for day and night described as good by the authors is just indicating that GIIRS has the same decreased sensitivity at night. There should be some statements about this issue.

Detailed comments :

Section 3 and 4 :

Some generalities about radiative transfer and retrieval methodology and basic known equations could be removed. Equations 3 to 8 have been largely documented such as in Rodgers (2000) and it is unnecessary to repeat this here.

Time : the time is given in UTC but it is not a correct choice to interpret diurnal cycles around China. Beijing local solar time would be much better. Furthermore the time is often given without the precision that it is UTC.

Figure 7 : precise the time system chosen.

Figure 8 : we suppose it is UTC !

Figure 10 : please provide hour in LST because UTC is not adapted to the geographical zone.

Fig 12 : Dayth => Day

Line 59 : The authors mention Kobayashi et al. (1999) as one of the first attempt to document CO from space with the japanese IMG ADEOS. Nevertheless, this paper do not present CO retrievals from IMG. The only retrievals of CO from this first spaceborne IR FTS have been published later by Barret et al. (2005).

refs:

C.D. Rodgers and B.J. Connor, Intercomparison of remote sounding instruments, JGR, 2003.

B. Barret, et al., Global carbon monoxide vertical distributions from spaceborne high-resolution FTIR nadir measurements, ACP, 2005.