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Atmospheric Measurement Techniques
Discussions

Interactive comment on “IASI nitrous oxide (N_2O) retrievals: validation and application to transport studies at daily time scales” *by* Yannick Kangah et al.

Anonymous Referee #2

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This paper presents results from N2O IASI retrievals based on the RTTOV radiative transfer model. N2O satellite observations are important to understand its global distribution and maybe help characterizing its emissions. As mentioned below, IASI has already been used to retrieve N2O profiles and EUMETSAT retrieval algorithm is providing such data for the whole IASI period. Therefore the present study is not providing completely new data. It is nevertheless interesting to have more than one dataset from the same instrument inasmuch as the quality of the datasets are proven. The objective of the paper is two-folded. The presentation of the retrieval methodology and validation and a case study. As detailed below the methodology and validation part should largely be strengthened and the Hysplit transport study which is weak could be removed.

Overall the quality of this study is not good enough to be published in AMT. I have major concerns about the originality, the methodology and the results that are presented in the paper.

I-Originality of the work

There are other studies on IASI N2O retrievals which are not sufficiently acknowledged and discussed. One of the first publication about N2O IASI retrievals is Garcia et al. (AnnGeo, 2013). Based on one year of data they show that the N2O EUMETSAT v5 product (August et al., JQSRT, 2012) provides a good agreement with FTIR data at Izana for the 10-14 km vmr. Garcia et al. (AMT, 2016) make a comparison between the EUMETSAT v5 product and the Izana FTIR data for 4 years. These comparisons show a very good agreement (R=0.87) for the total columns annual cycle. In their latest paper Garcia et al. (AMTD, 2017) show a good agreement between IASI N2O and HIPPO data. The authors should use these previous studies in details rather than just citing them. In particular they should discuss and compare their retrieval methodology, characterization and results with those described in these papers throughout the manuscript.

IASI-A is flying since 2006 and the present paper presents retrievals for validation with HIPPO data and a series of situations over a limited region for a very limited time period. It is possible to accept such a limited study for a very recent mission but difficult for a ten years mission with previous studies much more extended already published. Indeed, as mentioned above, in their latest studies Garcia et al. have taken advantage of the long time series to make robust statistics and they have used different available validation datasets such as long term FTIR profiles and columns at Izana and GAW in-situ data (see reply to reviewers in Garcia et al., AMTD 2017) and HIPPO campaigns. The present paper would be more convincing if it could prove that the new IASI N2O retrievals provide robust information about the N2O variability taking advantage of the large IASI period which is not yet the case.

II-Methodology:

The retrieval methodology is not fully presented and justified. In page 5 the basic equation of the OEM are rewritten which is unnecessary. They are described and explained in Rodgers (2000) and many other publications and can therefor be removed and replaced by more interesting information. Indeed, the retrieval strategy itself is hardly described and justified. Many auxiliary parameters are retrieved together with the absorbing gases profiles but no justifications and no discussion about these retrieved parameters are given.

i-Contamination Factor: This part is interesting because it allows to document how uncertainty on an auxiliary parameter will impact the retrieved target state vector. Nevertheless it is only valid for auxiliary parameters that are kept constant and are not retrieved together with the target parameters. In case of retrieved parameters, it only gives an idea of the parameters which retrieval will mostly interfere with the target parameters but does not allow us to know the quantitative impact on the target parameters. The authors should explain that this methodology is not quantitative for retrieved parameters.

ii-Atmospheric temperature retrieval: Why is the atmospheric temperature retrieved together with N2O and the other interfering species? Where is the information about the atmospheric temperature profile coming from? Atmospheric temperature is normally retrieved from CO2 lines assuming constant CO2 vmr's. CO2 or other gases vmr's are retrieved assuming constant atmospheric temperatures. These procedures avoid mixing between T and gases retrievals. Here there are some CO2 lines in the B2 band but the most likely is that the temperature is retrieved from other absorption lines such as N2O. The risk of contamination and interference is therefore major. This is actually shown by figure 5 and 6 where the CF are drawn. Atmospheric temperature uncertainties have the largest impact on N2O retrievals in both B1 and B2 with CF a factor of 4 of more larger than for the other parameters in the mid-upper troposphere. As stated above, this means that the T and the N2O retrievals are not independent. Therefore

high (low) N2O could be caused by high (low) T or the other way around but the impact cannot be determined because of the joint retrieval.

iii-Emissivity retrieval: The authors state that in RTTOV the ocean emissivity is parameterized and that land emissivity is prescribed by an atlas. They call these emissivities a priori emissivities and retrieve surface emissivity in their procedure. Are the emissivities spectrally varying in RTTOV? How are the emissivity jacobians computed in RTTOV? Are they the same over sea and over land? It would be interesting to see results from emissivity retrievals and the differences over sea and land and over different types of land. Surface temperature and surface emissivity are parameters with signatures hard to discriminate in a small spectral window such as B1 or B2 as they basically give the background slope. The retrieval of both parameters is probably redundant. The authors should give information about how much the spectral chisquare has been improved when surface emissivity is retrieved and about the improvement it provides on the validation dataset. In case of no or too small improvements, the retrieval procedure has to be reconsidered without emissivity retrieval.

iv-Validation: Equation 10 is applied to the HIPPO profiles to take the IASI vertical resolution and the impact of the a priori profile into account. Nevertheless, in order to apply this equation, the validation profiles have to cover the whole atmosphere. How and with what data are the tropospheric HIPPO profiles completed above the aircraft profiles top? How is the tropopause altitude taken into account? Concerning the comparison between the empirical and the theoretical errors there is a conceptual error. The authors compare the standard deviations of the differences between smoothed validation profiles and retrieved profiles (Emp) to the theoretical error (sum of smoothing and measurement errors Theoret) (Fig. 4). But as the validation profiles are smoothed by equation 10, the smoothing error is already taken into account and Emp has to be compared to RetNoise. As RetNoise is larger than Smooth this would not make a big difference. The other way is to compute the differences between the retrieved profiles and the raw validation profiles and to compare Emp with Theoret. Furthermore, the authors have shown that T uncertainty is largely impacting N2O retrievals (see CF) but as they retrieve jointly both parameters they cannot compute the resulting error. If the T profile was kept constant as suggested above, the errors caused T uncertainty could be evaluated (see Rodgers 2000). The errors caused by the other parameters should also be taken into account to compute the Theroret error but the same problem arises. The authors compute the Se matrix to provide the smoothing error instead of using Sa. Nevertheless Sa should represent the actual N2O global variability as accurately as possible and is the matrix that should be used to compute the smoothing error in equation 7 (Rodgers 2000). Se computed from the HIPPO data is representative of oceanic N2O profiles for given periods and may underestimate the variability. If the authors think it is a better representation of N2O global variability they have to justify this choice and may use it also for the retrievals. Furthermore a graphic representation of Se (diagonal values and covariance/correlation) should be given and compared to Sa.

Instead of R we should have r2 which shows the percentage variation in the retrieved profile that is explained by the variations of the validation profile. Therefore R> 0.707 is needed to have more than 50% of the retrieved variability coming from the real variability. It is also important to have a comparison of the variability of the validation data and of the retrieved data. All this information (standard deviation of the differences, r2, variability) should be given synthetically with a Taylor diagram.

III-Results:

i-Validation The retrieval results are not fully convincing. When the whole HIPPO dataset is used, meaning the strongest statistics (N about 100), r2=0.18 for B1 and 0.36 for B2 implying only 18 and 36% of the retrieved variability explained by the actual variability. Even if based on a limited HIPPO dataset, Garcia et al. (2017) achieve a better correlation (r2 = 0.58) whith a similar type of comparison as presented here. As they deal with a very close type of comparison, the results of Garcia et al. (2017) even in a paper under review should be discussed here. In most latitudinal bands (weaker

statistics with N < 30) r2 is lower than 0.5 especially in the B1 case with a maximum of 0.4 in the northern mid-latitudes. In the B2 case r2 is the highest (0.85) for the tropical southern latitudes. But in that case it is based on 12 points only which makes the statistics really poor and the high R is due to the fact that the points are separated in to clusters. Furthermore, in the best r2 cases (tropical southern and northern latitudes for B2) the slopes of the linear interpolation are much larger than unity (2.5 and 3.3) indicating a largely too strong variability of the retrieved vmr's compared to the validation vmr's. For northerm mid-latitudes r2 = 0.4 for B1 and 0.29 for B2 which are rather low values. Finally, the authors state that in summary N2O_B1 and B2 are of sufficient quality to analyse N2O variations in the mid and high latitude regions. This conclusion is not really supported by the validation results as discussed above. Especially for high northern latitudes with r2= 0.1 for both B1 and B2, only 10% of the variability comes from the actual N2O variability. We would rather say that these data should not be used.

ii-Transport study The variability of IASI N2O at 309 hPa shown on Fig. 13 is probably coming from a tropopause height difference. As shown by the AvK's, IASI vmr at 309 hPa is sensitive to a very large altitude range (600-120 hPa). Therefore it is equivalent to a N2O column or mean vmr over this range. When the tropopause changes from $\sim$100 hPa in the tropics to $\sim$250 hPa in the extratropics, the corresponding N2O columns mechanically change because the N2O vmr is lower in the stratosphere than in the troposphere. The authors attribute the N2O enhancement to upward transport from the Asian BL and horizontal transport within the anticyclone. This is also probably the case as shown by an extended literature based on satellite CO observations (Park et al., JGR, 2007...). Nevertheless, N2O is a well mixed gas and the quantification of such an effect is rather complicated. Surface in-situ data generally show a very limited seasonal variability of the N2O mixing ratio even in emission regions. Therefore the Asian BL is probably not N2O enriched as it is CO enriched. If the authors have evidence and data to document an important N2O enrichment during the monsoon in south Asia they should provide and discuss it. Another element that tends to strengthen the tropopause effect is that the IASI N2O high values are not limited to the anticyclone boundaries but to the whole tropical region. See in particular the high N2O band between 15 and 5°N which is outside of the anticyclone (the southern boundary of the anticyclone is at about 15°N). In order to have a better idea of the tropopause versus BL transport effects (I) the region of Fig. 13 should be extended both in latitude and longitude (ii) the boundaries of the anticyclone should be provided on Fig. 13 based for instance on PV values (see Ploeger et al., ACP, 2017) or on geopotential height values (e.g. Randel and Park, JGR, 2006). The Hysplit study is based on online simulations and simply shows that on the southern edge of the anticyclone, transport is westward which is expected. It does not prove that the air parcels are coming recently from the south Asian BL (the backtrajectories end up between 700 and 300 hPa and with a tenths of trajectories the statistics are very poor when Lagrangian studies are performed with millions of air parcels) nor that N2O enhancements over the whole tropical band could be due to such a transport process. The Hysplit part is therefore largely insufficient to draw conclusions and could be removed. The literature is rich enough about the subject of upward transport of BL air masses to the UTLS and trapping of pollution into the anticyclone. See for instance the Lagrangian modeling study of Bergman et al. (2013). References to this extended litterature are enough.

IV-Minor comments:

p2l20-29: To my knowledge, the first paper to deal with tropospheric N2O retrievals from a satellite instrument is Chedin et al. (GRL, 2002). It shows very interesting results concerning the N2O evolution based on the TOVS instrument. This ref should be cited in the paper. p3l16: Turquety et al. (2004) does not concern IASI O3 retrievals. There are a number of recent refs concerning IASI O3 retrievals. P4l17-18: the authors should give a recent reference to justify their choice of NEDT. P5l16: the authors should give a ref or a detailed explanation that justify the shape of their a priori covariance matrix. We also need information about the shape of the a priori matrices for the other retrieved profiles (are they diagonal?). P6l2: the choice of 30% for the a priori

variability for H2O because of HDO is rather empirical and poorly justified. What does sink parameter mean? P6l4 and l6: sensitivity studies are mentioned but the reader knows nothing about what they are made of. Details about the methodology used and about the results of these sensitivity studies are needed. P6l14: ref for the radiometric noise (see above).

Figures: Fig 14: this figure is of poor quality and should be improved. The winds should be superimposed such as on Fig. 13 in order to make a more straightforward comparison.

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