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Atmospheric Measurement Techniques Discussions

Interactive comment on "The MIPAS2D database of MIPAS/ENVISAT measurements retrieved with a multi-target 2-dimensional tomographic approach" *by* B. M. Dinelli et al.

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article color

First of all we would like to thank the reviewer for his useful comments and careful check of the paper's text. To ease the reading of our answers we have reported the reviewer comment in red and our answers in black.

The paper presents a set of temperature and trace gas distributions retrieved from MIPAS/Envisat spectral observations by a two-dimensional retrieval approach. The data set covers the complete mission span of MIPAS, at least for some of the retrieved

parameters, as long as the so-called nominal observation mode has been applied. The paper described the data base and give some time series and global distributions as examples. General comments: The paper is a qualitative description of the data set, with some example data characterization. The algorithm applied to produce the data set has been published earlier (Carlotti et al., 2006). A thorough description of the parameter settings for the specific application is missing,

There is large number of parameter settings involved in the analysis. Part of them are relevant only for reaching convergence and their description is not meaningful in the manuscript. . The others are discussed in the paper and we list below the improvements that we have introduced in the revised text, along with where the settings had been already discussed in the original version:

Retrieval grid: discussed in sect 3.1 MWs: discussed in sect 4.2. In the revised paper we have added the MWs for the minor species in table 2 Continuum: Discussed in sect 4.1, in the revised paper we have added a discussion on the vertical range and on the a-priori errors used for the OE. Cloud index: discussed in Sect 4.1 Altitude interval: discussed in Sect. 3.1 and details added in the revised paper in sec. 4.1 Initial guess profiles and a-priori information: discussed in Table 1, in sect 4.1 and in sect. 4.2.2 Auxiliary data: Discussed in Sect. 4.2.2 and in the reference to Carlotti et al. (2006) Boundary of the atmosphere: It was set to 80 km, added in Sec. 4.1

as well as a complete characterization of the data in terms of error budgets and spatial resolution.

The systematic error budget is described in Sect. 4.3.1 (and references). To support the discussion of the section in the revised text we have added a table (table 3) reporting the values for all targets in both MIPAS configurations and removed the figure. Random errors are reported in the database, one value for each retrieved parameter. Spatial resolution : in the revised text we have added a new section (after section 4.3.5) discussing the vertical and horizontal resolution evaluated through the AK using two

standard FR and OR orbits and a standard atmosphere. Relevant figures have been also added (Figure 8 in the revised manuscript).

The authors claim that the MIPAS2D data set is homogeneous which would be of great importance given the increasing need of long term global data sets for climate monitoring. For this reason, the homogeneity of the data set needs to be proven, in particular since some obvious discontinuities and inconsistencies are present in Figs. 5, 9, and 11.

The homogeneity claimed in the paper has to be intended in terms of the retrieval grid, retrieval algorithm, vertical and horizontal resolution and measuring instrument as stated in sect 3.1. Of course in the passage from FR to OR configuration some discontinuity can be appreciated mainly due to the different analysed MWs (due to the change in the spectral resolution) and consequently to the different systematic and random errors affecting the retrievals as shown in Figure 5. Following the reviewer's comment, this has been now discussed in the revised version of the paper (sec 4.3.1, new section on horizontal and vertical resolutions). The discontinuities noted by the reviewer in Figure 9 are discussed in sect. 4.4 and the ones in Figure 11 are in any case within the total error bars of the retrievals. In further validation papers eventual discontinuities will be addressed and characterized.

Certainly the authors have compared their data set with other available MIPAS data sets (e.g. the standard operational ESA produced one) or to observations from other satellite sensors. Have any peculiarities been found in these comparisons?

This paper is meant to describe the MIPAS2D dataset, and it has been submitted to AMT for this reason, as it has been done e.g. by Von Clarmann et al. AMT (2009). Comparison with other datasets will be subject of independent papers on appropriate journals as it will require a full analysis

I recommend publication of the manuscript after the following major revisions: - given the increasing needs of long-term homogeneous data sets for the assessment of cli-

mate change the laim of homogeneity of the presented data set is not at all proven. If the authors insist on their statement that the presented data set is homogeneous, they must provide a careful analysis and quantitative proof (at least lack of biases in temperature and vmrs between FR and OR mission phase, similar error budgets and spatial resolution must be demonstrated).

The manuscript has been revised to avoid misleading the readers about the homogeneity of the dataset. See the comments above.

- provide error budgets and vertical and horizontal resolutions for all species, not only temperature and ozone.

Done in sect. 4.3.1 of the revised version of the paper

- provide a description how the azimuth-view of MIPAS near the poles has been tackled.

MIPAS azimuth view is always slightly deviating from the orbit plane. The geofit approach tackles this by making the assumption of atmospheric homogeneity across the orbit track. ENVISAT orbit inclination is of about 8.9 deg. In order to compensate for this inclination the MIPAS pointing azimuth angle changes along the orbit from about -20 (in proximity of north pole) to +15 deg (in proximity of south pole) as depicted by the red plot in the figure included that refers to a real orbit. This implies a displacement of the tangent points with respect to the orbit plane that has both a longitudinal and a latitudinal component. It is commonly accepted that atmospheric variability is minor when moving at adjacent longitudes. In the figure 2 the green plot represents the variation of the difference between latitude of tangent points as reported in the level1b file and latitude of tangent points as calculated by the geofit in the orbit plane (within its pointing calibration phase). It can be seen that this difference reaches a maximum value of about 2 deg which corresponds to about 220 km. Along this distance the homogeneity assumption cannot be ensured, however the approximation is 5 times better than that of 1D algorithms that assume homogeneity in the whole portion of atmosphere spanned by a single limb scan (more than 2000 km). A sentence is added in the paper

in section 3 and in the new section on spatial resolution (new Sec 4.4)

1 Specific comments:

abstract, l 22: the homogeneity of the data set has not been demonstrated; in contrary,Fig. 5 demonstrates that at least for ozone, the total systematic errors differ considerably. Fig. 9 demonstrates different fit qualities between the FR and OR mission phase. Fig. 11 hints towards a bias in ozone vmr between the FR and OR phase. Further, the impact of various level-1b versions (as outlined on page 2645) may deteriorate the homogeneity of the data set, which has not been analyzed.

The sentence is 'a homogeneous database in altitude and latitude' and it is the geographical homogeneity that we claim here, not the systematic and random errors and retrieval set up, that is impossible to be homogeneous given the different measurement strategies in the FR and OR missions. Tests have been performed in the past years to identify possible inhomogeneities caused by the different level-1b software versions used to produce the level 1b files, especially in the FR data. No evident dependence of the retrieval results from that was highlighted. A sentence has been added in sect. 4.2.1

p 2642, l7: the MLS mission on Aura applies a 2D retrieval approach. This should be mentioned here.

References to the MLS instrument and processor have been added in the introduction of the revised text.

l9: a more appropriate reference instead of Stiller et al., 2002 would be von Clarmann et al., 2009 (in your list: von Clarmann, T., Hopfner, M., Kellmann, S., Linden, A., Chauhan, S., Funke, B., Grabowski, U., Glatthor, N., Kiefer, M., Schieferdecker, T., Stiller, G. P., and Versick, S.: Retrieval of temperature, H2O, O3 , HNO3 , CH4 ,

N2O, ClONO2 and ClO from MIPAS reduced resolution nominal mode limb emission measurements, Atmos. Meas. Tech., 2, 159–175, 5 2009).

The reference has been suggested by von Clarmann himself; it is the first description of the use of a horizontal gradient in KOPRA. The reference suggested by the reviewer is the last application of this method.

l18: an explanation is missing how limb-scans near the poles are treated which are not in the orbit plane.

See the answer already given above

p2643, l1-2: the authors should mention that minor species are only available for the high-resolution phase of the MIPAS mission.

OK (specified in the introduction and in the abstract)

l23-24: please mention that MIPAS is looking out of the orbit plane at high latitudes, and explain how this is handled in the 2D retrieval.

See the answer already given above

p2644, l12: the spatial sampling has been increased (improved), not necessarily the resolution.

OK. changed in the revised text

l17: define "sweep".

Changed sweep with limb view in the revised text

l21: MIPAS measures again with 100% duty cycle since December 2007.

OK, we have used the exact date in the revised text.

l23: the duty cycle since December 2007 (until December 2009) was 8 days nominal mode, 1 day MA mode, 1 day UA mode. Please mention in the revised manuscript,

that it is changed now to 4 days NOM, 1 day MA, 4 days NOM, and 1 day UA.

OK. The sentence was only indicative since in the OR mode the definition of the measuring modes has been changed several times, and the database reported in the paper was for the GMTR analysis on the nominal observation mode only. We have changed the sentence in the revised text.

p2645, l16-18: how do the various level-1b data versions affect the claimed homogeneity of the level-2 data set?

No in-homogeneity is introduced by the two L1b versions in the FR data. The same applies to the OR data. Obviously there is a in-homogeneity between the FR and OR data, mainly because of the different measurement setup. In the work performed to retrieve the vertical distribution of the H15NO3,(published in Brizzi et al. J.Geophys. Res. 114, D16301, doi:10.1029/2008JD011504, 2009) we have thoroughly checked the consistency of the data produced with different level1b version and no effect was present. A sentence has been added in sect. 4.2.1 but no reference has been added since we do not discuss the issue in the paper.

l18-28, Fig. 2: How does the variation in the "noise level" affect the error budget of the level-2 data products, with respect to the claim of homogeneity of the data set? I do not really find a discussion of this topic in Sects. 4.3.2 and 4.4.

The noise level affects directly the ESD calculated for the retrieved targets. Because of the changes in the noise level due to the reduction in resolution (see Fig 2), the ESD for OR targets are reduced approximately by a factor 0.6 compared to the respective target in the FR. This is also influencing the chi-square of the retrieval in a consistent way (Fig. 9). A comment has been added to the revised section 4.3.1 together with the discussion on the increase of systematic error from the FR to the OR (see also table 3),

p 2646-2648 (section 2.2): why is such a detailed description of the ESA level2- pro-

cessing given here? It would make sense if a comparison of the MIPAS2D product with the ESA level-2 product would have been provided later in the manuscript, but this is not the case. I recommend to remove this detailed description.

The two analysis systems have some common features since GMTR is an evolution of the scientific version of ESA level 2 code. The description is given to highlight the differences between this analysis and the ESA level 2 analysis since for some targets the same auxiliary data (MWs LUTs and OM) are used.

p 2648, l6-7: this statement ("assume the atmosphere horizontally homogeneous") is obviously not true, as stated in the sentence of line 8-11. Please be more consistent.

The sentence is correct. The cited papers perform the analysis assuming the atmosphere horizontally homogenous. It is only in von Clarmann et al. 2009a that the horizontal temperature gradient has been introduced in the routine analysis of the MIPAS OR data, however leaving the inhomogeneity of the vmr fields unaccounted for

l10-11: Did von Clarmann et al. 2009a claim that "horizontal inhomogeneities ... were making their retrievals unstable"? All I can find in this paper is that the convergence rate was improved (von Clarmann et al., ACP, 9, p160, middle of 2nd col).

We interpreted the convergence rate as the number of retrieval reaching convergence. Therefore an improved convergence rate implies that an instability of the retrieval has been resolved. If the retrieval was stable there was no need to introduce the gradient just to speed up the retrieval procedure. Since the reviewer thinks that our words are misleading in the revised text we have cited the sentence as reported in the aforementioned paper.

p 2648, l26 - p 2649, l1: This statement sounds as if all targets have been retrieved simultaneously. As outlined later (p2652), MTR has been applied to temperature, pressure, ozone and water vapor only. This should be clarified here.

Actually here we describe the MTR functionality, not how the retrievals have been per-

formed. This is discussed thoroughly in section 4.1. We have modified the sentence in order to avoid misunderstanding in the revised text.

p 2649, l2-7: It does not become clear at this point if the OE approach has been used to generate the MIPAS2D data base, and to which extend. For all observations throughout the complete mission? Or only selected scans? What about homogeneity then? Please clarify.

This section is used to describe the GMTR code, not the retrieval setup. See section 4.1 where the retrieval setup is described in details for all targets.

l23-25: Within a 1-d retrieval, the atmosphere does not vary along the orbit (by assumption), therefore the geolocation of the limb scan is not an issue at all!

Independent of the geolocation (lat., lon.) of the retrieval grid, a given retrieved profile point is determined by the atmospheric state found in the region where its horizontal averaging kernel (HAK) peaks. In case of 1D retrievals assuming horizontal homogeneity, a retrieved profile point can be assigned to any of the geolocations spanned by the analyzed limb-views. However the geolocation of the peak of the related HAK is well defined and independent of the horizontal coordinates conventionally assigned to the considered retrieval grid point. Since HAKs are often ignored in intercomparisons with correlative measurements or models, it would be desirable to geolocate each retrieved profile point in the region where its HAK peaks. This reasoning partly applies also to the 2D retrievals, therefore in the revised manuscript we prefer to remove the sentence under discussion.

p2650, l2: here you should add: "... and the pointing problems inherent to the MIPAS mission (Kiefer et al., 2007)". Reference: Kiefer, M., von Clarmann, T., Grabowski, U., De Laurentis, M., Mantovani, R., Milz, M., and Ridolfi, M.: Characterization of MIPASelevation pointing, Atmos. Chem. Phys., 7, 1615-1628, 2007.

Actually the pointing problems reported by Kiefer are relative to a difference between

the tangent altitudes reported in the level1b data and the real values retrieved with the KOPRA algorithm (using the assumption of hydrostatic equilibrium). What we are saying here is that the vertical sampling of MIPAS along a single orbit is not performed at fixed altitudes even if the pointing angles are commanded to be the same along the orbit. We have modified the sentence in order to avoid misunderstanding in the revised text.

l10-14: how does the vertical resolution of the retrievals behave if the retrievals are not performed at (or very close to) the tangent altitudes?

Discussed in the subsection introduced

p2651, l14-15: I understand from p 2649, l2-7 that you already apply the OE regularization scheme? Then I do not understand this statement here. Does it mean you apply the OE regularization scheme for either the FR or OR observation only? What about homogeneity of the data set then?

OE is applied to the two datasets in the same way. We use an a-priori covariance matrix that is diagonal, without introducing correlations among the retrieved values. The regularization here implies the fact that we have to introduce off –diagonal terms in the a-priori covariance matrix. We have clarified this issue in the revised text.

l25: Again the authors claim that a homogeneous data set was intended to be produced, but they fail in proving it later on in the paper.

See answers above

p2654, l10-16: I got the impression that a priori and initial guess have been mixed up here. The two quantities may - and often are chosen to be - but need not be identical.

We know, that's why we specify in line 19 page 2652 that we use the initial guess as a-priori. We have clarified this issue in the revised version of the paper

l10-13: have you demonstrated that your retrieval indeed *is* independent of the initial

guess?

Yes, through several tests not reported in the paper. We have added a clarification in the revised text.

l27: The micro windows selected for the minor species are missing in this table and need to be reported for reasons of completeness.

OK. We have added them in Table 2 in the revised paper

p2656, l12: A full error budget for each species needs to be reported in order to thoroughly characterize the data base. Just giving an example for temperature and one species is not enough. An explanation is missing why the total errors of temperature differ so much between the FR and OR mission phase. How do the less accurate temperature retrievals of the OR mission phase affect the consecutive trace species retrievals?

OR temperature has larger total systematic errors and most of them affect all VMRs in the same direction. As outlined in http://www.atm.ox.ac.uk/group/mipas/err/ the VMR error estimation assumes a constant 1K error on temperature. This means that, if the actual temperature error is greater than 1K, the VMR error component due to temperature error should be scaled accordingly. The future validation work will provide a final reliable estimate of the errors affecting the retrieved targets. In the revised manuscript we will clarify this concept and will include a table (table 3) reporting the error budget of all the targets of the database.

End of section 4.3.1: for a full characterization, the averaging kernels of all the species, together with the achieved spatial resolution in the vertical and horizontal domain should also be discussed.

Added a subsection on this topic (new section 4.4) of the revised version of the paper

p2657, l12: if I understand correctly, retrieval results are accepted if the retrieval random error is about 70% (should be 50%) of the a priori error or less. Other satellite

instruments are much more stringent in their filtering; e.g. ODIN/SMR rejects retrievals with retrieval errors being larger than 20% of their S_a. By the way, from the later discussion, I conclude that a threshold of 0.5 and not 1/(sqrt(2)) is meant which I also derive from the given eq.

As said in section 4.4 (page 2660 lines 19-28), we report all values along with their information gain. It is up to the users to decide which threshold on the information gain to use. Please note that the equation involves the covariances (squared errors) and the log2 therefore when information gain is 0.5 the final retrieval error is 1/sqrt(2) times the a-priori error (not squared).

Section 4.3.4: Does this discussion mean that the data base is presented versus geographical altitude? Since pressure is retrieved together with temperature and other quantities, I would expect that the 2-D fields are presented on pressure. Or do you just want to say that if the user should decide to use the altitude information of level-1b data instead of the pressure representation of the 2D data base he/she would produce the discontinuities as demonstrated in Fig 7?

Of course we report the data using the altitude grid, since this is the way we retrieve our data. We perform the retrievals on the altitudes described in section 3.1 (page 2650 lines 12-13). If the users want to use pressures, the pressures at the retrieval altitudes are reported in every file a part from the Temperature files. We also include a file with the retrieved pressures. Remember that we do not constrain our pressures with the hydrostatic equilibrium. So the discontinuities shown are present in the database.

p2662, l 27 - p2663, l1: why is averaging over 5 days necessary?

The averages were performed to give an example of smoothed trend removing short term/small scale variability and the effects of atmospheric waves (as a multi-day average of orbits implies also zonal averaging). We adopted a 10-day average for latitude bands and 5-day averages for maps at constant pressure as they gave the best results for the simple purpose of these examples.

p2663, l15: the ozone depletion is not correlated with the breaking of the vortex, in contrary, it is terminated by the vortex breaking (which happens later, in November, usually).

OK. We have changed the sentence in the revised text

l23: "meridional circulation": do you mean the position of the inner tropical convergence zone?

Since this is only an example, the changes seen at stratospheric altitude are generally referred to the (wave-induced) meridional circulation to avoid entering into discussion of stratospheric dynamics mechanisms (e.g. Plumb, Reviews of Geophysics, 45, RG4005 / 2007), The interested reader can find relevant literature. The link of the subtropical dynamical barriers with the tropospheric ICTZ is not straightforward.

p2664, l1-4: a bias between the FR and OR mission phase is quite obvious for the night time (OC between 180 and 360 deg) ozone distributions. Could you comment on that, in particular regarding the homogeneity of the data set?

We did not intend to do any science with the pictures shown, but just we wanted to give the potential users some examples of applications of the database. However, the differences in the FR and OR ozone are within the total error bars.

l8-9: referencing of previous work by Lopez-Puertas et al. (regarding solar proton events) and Funke et al. (regarding downward transport of mesospheric air) would be appropriate.

OK, we have added the suggested references in the revised version of the paper.

Fig. 16: Is the increase in CFC-11 and CFC-12 at the upper end of the plotted range (27 km for CFC-11 and 40 km for CFC-12) real?

CFC-11 and CFC-12 VMR averages are acceptable since the percentage error of the VMR average is almost always below 10% and 20% respectively. However the increase

of the VMR of the two species occur mostly in regions (time period – altitude) where the statistical error increases. This suggests that particular conditions (i.e. low temperature) may act as source of systematic errors in that regions and further analysis would be needed for scientific use of the data.

l23-28 and Fig. 17: From other observations and modeling studies (compare Journal of Atmospheric Sciences, special issue, vol 62(3), 2005) there is no evidence of a real vortex split in the range of 20 km, but only a distortion in shape of the vortex seems to be observed (two lobes with a bridge in between)(compare von Savigny et al., ibid, and Feng et al., ibid; see also Manney et al., ibid, for a 3D figure; see also http://www.isac.cnr.it/wavacs/material/workshop/lahoz.pdf and Geer et al., QJRMS, 2006 referenced therein); the full split (without a bridge between the lobes) was only present in the upper stratosphere; the authors should discuss their findings with respect to these published results.

As already said we did not intend to do any science with the pictures shown, but just we wanted to give the potential users some examples of applications of the database.

However we have checked some of the papers suggested by the reviewer:

Von Savigny shows a two minima ozone distribution on the 27th at 22 km . No data are shown for the 23-26 Sept. period.

Feng et al. shows the measurements from TOMS (that measures ozone total columns) for the 22, 25 and 28 of September, showing a distortion in the shape of the vortex on the 22 and a split on the 25 and 28. There are also comparisons of its model with MIPAS data retrieved by IMK with 1-D code at 20 km on the 26, and the figure shows a 2 minima in the ozone concentration at that altitude.

The data that we show in figure 17 are a collection of data retrieved from measurements acquired during 3 days from the 23 to the 26 of September. We have thoroughly checked our data against the data shown by Feng et al. in figure 5, that report the

IMK analysis of MIPAS spectra on the 26 at 20 km. First of all, looking at figure 6 of Feng's paper it can be seen that the ozone values from 20 to 21 km on the 26 september profile do change of about a factor of 2. This is confirmed by our data at those latitudes; we have retrieved values at 18 km and at 21 km and the ozone concentration changes quite rapidly, from approximately 0.5 to about 3 ppmV. So the behavior of ozone at 20 and at 21 km may change quite dramatically, even if there is only 1 km difference in altitude. The clear split shown in figure 17 of our paper is caused by the values retrieved from orbit 2998 on the 26th of September. We have compared our data for that orbit (both in the NOM and GRD datasets) with the ones reported in figure 5 and coming from the IMK 1-D analysis. The VMRs retrieved by IMK close to the Antarctic peninsula at 20 km are of the order of 2.3 ppmV. We find values of 3.1 ppmV in the same area at 21 km. If we accept that there could be a variation from 20 to 21 km of up to a factor of 2, we see that our data and IMK data do not disagree. Moreover this is a strong gradients area and we expect that 2-D retrievals perform differently from 1-D retrievals.

p 2665, l5: MIPAS is back to 100% duty cycle since December 2007.

OK we have used the exact date to avoid confusion since the 100% duty cycle was not operated along the whole December 2007.

2 Technical comments:

p 2641, last line remove "on" an the end of Cartography.

OK

p2643, line 24: typo "consists"; better "observations" instead of "observation geometries".

OK

References: there are two references (von Clarmann et al., 2009) which need to be distinguished.

OK

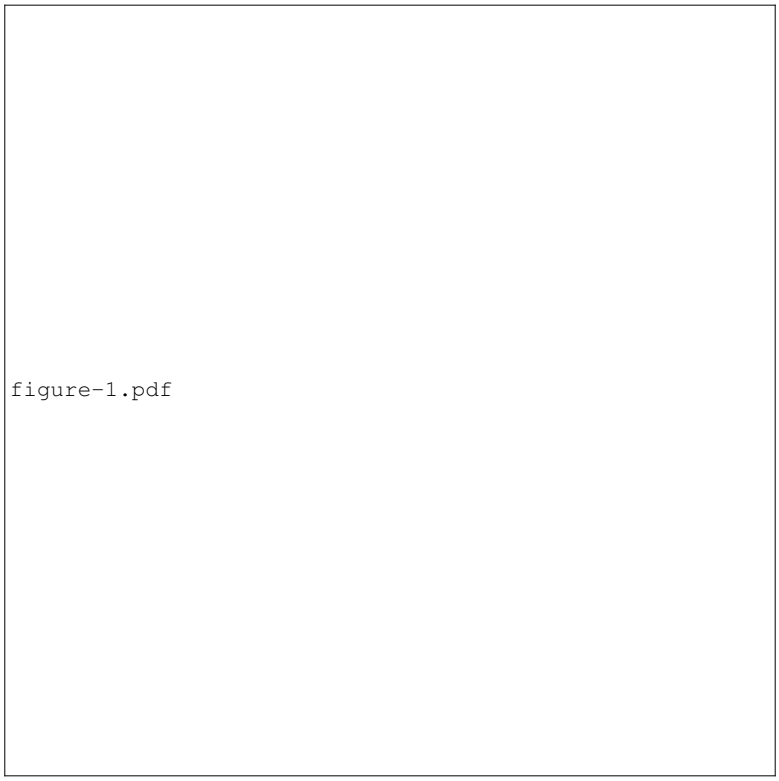
figure-1.pdf