

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.

Anonymous Referee #2

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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

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(Carlotti et al., 2006). A thorough description of the parameter settings for the specific application is missing, as well as a complete characterization of the data in terms of error budgets and spatial resolution. 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. 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?

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 climate change the claim 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).
- provide error budgets and vertical and horizontal resolutions for all species, not only temperature and ozone.
- provide a description how the azimuth-view of MIPAS near the poles has been tackled.

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

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homogeneity of the data set, which has not been analyzed.

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

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).

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

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

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.

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

l17: define "sweep".

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

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.

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

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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.

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

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.

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).

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.

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.

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!

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 MIPAS elevation pointing, Atmos. Chem. Phys., 7, 1615-1628, 2007.

l10-14: how does the vertical resolution of the retrievals behave if the retrievals are not

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performed at (or very close to) the tangent altitudes?

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?

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.

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.

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

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

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?

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.

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

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discussion, I conclude that a threshold of 0.5 and not $1/\sqrt{2}$ is meant which I also derive from the given eq.

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?

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

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).

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

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?

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.

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?

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 Sav-

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igny 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.

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

Technical comments:

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

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

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

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 2639, 2009.

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