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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of MIPAS elevation pointing" *by* M. Kiefer et al.

M. Kiefer et al.

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We would like to thank the anonymous referee #2 for the comments which helped us to identify several sources of misunderstanding in the text and to hopefully improve the overall comprehensibility due to inclusion of additional information. Below we give a detailed point-by-point list of answers to the referee's comments.

General Comments

a) We agree that the terms "absolute pointing" and "relative pointing" should be clearly defined and used consistently in the paper. We will add the definitions (relative: difference between adjacent tangent altitudes; absolute: tangent altitudes above Earth).

We insist that pointing knowledge is crucial for limb sounding. The approach to use pressure coordinates instead of altitudes *ALSO* assumes pointing knowledge, only

that in this approach one relies on the absolute pointing knowledge in pressure co-ordinates. During the retrieval pressure may be treated as an atmospheric state variable rather than an altitude co-ordinate; the use of pressure, however, to represent T or VMR profiles, gives the pressure retrieval the characteristic of a pointing retrieval in p-coordinates.

The relative pointing of Level 1b is used as a constraint in our LOS retrieval. Therefore it would be at least questionable to use it for comparison with L1b data. On the other hand relative pointing already is a retrieved quantity of the ESA L2 product. For data users there would be no much gain in such a comparison.

b) What the IMK processor does, is to calculate the Jacobians dy/dT and dy/dLOS and to invert the retrieval equation for these quantities for a given (ECMWF) pressure-altitude profile. Then, a new hydrostatic atmosphere is built up using the retrieved temperatures and tangent altitudes, and one ECMWF data point (p,z), typically at 20 km. This is, according to our definition, retrieval of absolute pointing. ”absolute”, however, does not mean ”independent of any a priori”. Perhaps the argument is about the terminology what we mean ”absolute”.

We will add some more specific description of the method.

We feel that we do not have to justify in the paper, why we use our processor unless there is evidence that our approach has particular disadvantages, and, as carrying out this work independent from ESA funding, we do not feel obliged to conform with any external strategy or baseline. Nevertheless, we can scientifically justify the use of our processor here:

1. While the different retrieval approaches are considered nearly equivalent (the fact that our approach also corrects for the length of the line of sight through the atmosphere, not only for the tangent pressure is considered to be of higher order and only of academic interest), the temperatures retrieved by both the processors

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(ESA and ours) are not necessarily identical, which propagates directly onto the pointing information. By the time of this study, IMK temperatures had already been validated (Wang et al., JGR, 2005) while validation of ESA temperatures still was in progress.

2. The ESA code is run by the agency, and the retrieval control parameters are beyond the direct control of the scientists, even beyond the direct control of the authors of the code. Contrary to that, with our own processor we have direct access to all control parameters which helps to better understand any suspicious results.

3. The regularized retrieval we use has a tendency to correct for problems related to the fact that there still seems to be a systematic calibration uncertainty related to different characteristics between interferograms recorded with forward and backward movement of the interferometer mirror. To our understanding, with the unregularized ESA retrievals, these uncertainties fully map onto the data product.

4. To our knowledge, in the case of ESA processing, the lowest retrieved point in a profile is that related to the *central* point of the field of view related to the lowest tangent altitude in a limb scan. However, the measured signal depends also on the atmospheric state between this central point and the lower edge of the lowest field of view. Since the atmospheric state there is not adjusted, this triggers some retrieval errors (formally a particular kind of smoothing error, i.e. some mapping of the a priori onto the result, usually triggering some oscillations). Contrary to that, our retrieval adjusts the atmospheric state in the total altitude range seen by MIPAS. Therefore we think that the altitude range where useful temperatures (and other state parameters) can be retrieved is wider when our retrieval scheme is used.

In summary, we do not think that our general approach of pointing retrieval is superior over the approach of tangent pressure retrieval. Instead, we feel that there are

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other advantages to use our code, related to implementation details and validation status. The same results could probably be obtained with a tangent pressure retrieval approach — if the same temperatures are used.

c) Agreed. The MIPAS engineering pointing specification will be included.

d) The satellite attitude is corrected immediately after the on-board information on satellite attitude has been updated. This makes the two times 2:00 and 14:00 unique. We do not expect that all quantities characterizing the pointing depend on these times, nor do we claim such a behaviour. It rather is a natural choice to separate data into intervals which might show differences with respect to the quantities under consideration. Even if the result were that there are no dependences of characterization on which interval is chosen, this would be further piece of information, namely: "The change of satellite attitude performed twice per day does not have an influence on pointing, apart from correcting the drift."

Specific Comments

ABSTRACT

1) l.5: Agreed. Will be reworded: ... are independent of systematic offsets in the engineering Line-Of-Sight (LOS) information delivered with the ESA Level 1b product.

2) l.18 & 24: We shall add the altitude difference info for the pitch bias. The roll angle error can not be meaningfully expressed as an altitude difference unless a yaw angle/azimuth is specified.

3) last line: The difference is between the results given by the platform's star trackers

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and the results of the MIPAS LOS-calibration measurements. We think that the current formulation: ”a pitch bias of 24 mdeg with respect to the platform attitude information” is sufficient to express this.

4) Agreed. Text will be expanded.

1.1 MOTIVATION

5) p.13077, l.2: New reference (ESA SP) will be used.

6) p.13077, l.5: If we retrieve correct mixing ratios but don’t know to which altitude (or pressure) they are to be assigned, these numbers are of no value. If there is an error in the assumed tangent altitude, the retrieved abundance will be attributed to the wrong altitude, and in turn, at a given altitude the wrong abundance will be retrieved. We think that this ”mapping problem” is an important aspect of the pointing problem. Of course, one can solve this problem by retrieval of tangent pressure (as a simplified retrieval of absolute pointing in pressure coordinates) as well. The fact, however, that this ”pointing correction in pressure coordinates” is necessary just confirms that our statement is in general true. In the introduction section we mention the paper describing the tangent pressure retrieval approach (Ridolfi et al) in the same sentence where we mention our own method, and we claim no general superiority of our own method there, except of its convenience for applications directly targeted at geometry problems. In summary, when considering tangent pressure retrieval as a certain approach to pointing retrieval, we do not feel that there is anything wrong with our statement. Our reply to specific comment 7 will solve this misunderstanding.

7) p.13077, l.11–15: This statement was meant to shortly state what the reviewer is saying in general comment b and specific comment 6. We shall reword this and are confident that this will help to improve the general agreement between the reviewer and us.

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1.3 RETRIEVAL METHOD

8) p.13078: This is a good point, we actually forgot to state this. We shall mention in Section 1.3 that the IMK retrieval processor is used. We shall expand the Section on T-LOS retrieval, and in particular the scheme to include ECMWF information in our pointing retrievals.

9) p.13079, l.6–7: Agreed. We shall replace ”quality” by ”absolute accuracy”

10) p.13079, l.6–7: Actually the comparison with L2 is not our aim. We want to characterize the instrument respectively the engineering pointing information. We do not make further statements about relative pointing, most of our results refer to height-averaged data. Hence we do not see a benefit of introducing details on a sub-topic which is of no further use for our analyses. See also **15)**.

11) p.13079, l.15: To our knowledge there is no such simple mathematical equation linking azimuth and latitude.

12) p.13080, l.1–5: We shall communicate the nominal scan pattern as proposed. There is no intentional change in the scan pattern. Actually the elevation angles are calculated such that the scan pattern should be constant over the whole orbit, taking into account e.g. the Earth’s oblateness. We shall change the text to avoid the misunderstanding that there was an intentional change of scan pattern.

13) p.13081, l.7–10: A change in ECMWF data has a direct influence only for one single point, i.e. the point at which pressure and altitude are taken for our LOS retrieval. A shift of the entire tangent altitude profile should result. Actually we did not intend to make this a major issue of the paper but rather use the results to justify that an average over altitude differences is a reasonable quantity.

14) p.13081, l.10-14: Agreed, rms is the wrong expression. We decided to remove the figure and explain in the text what the standard deviations at single geolocations

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typically are. Again, the remark of **13)** is valid.

15) p.13081, l.11: As shall be stated in the revised text, L1b relative pointing information is used as a constraint for the IMK LOS retrieval. Therefore for us it does not seem to be a good idea to derive some sort of precision estimate for ETAs.

16) p.13081 Eq.(1): As can be seen from Fig.2 the SD of Delta h does not change significantly with altitude. Therefore we believe that the gain due to statistics by far outweighs the possible gain which might be achieved following your suggestion.

3. TIME DEPENDENCE

17) p.13081, l.25: In principle the jump is of the same nature. It is due to the upload of recent attitude data to the platform twice a day. However, von Clarmann et al.(2003) used near real-time spectra, whereas we use reprocessed spectra (see p.13079, l.19).

18) This seems to be mainly a matter of point-of-view, method versus result. We think that 3.1. gives a characterization of what is observed if one looks at the course of Delta h over one orbit. The properties of the two pieces of information we get from an orbit, namely offset and gradient (see p.13082, ls.18-19) are discussed further. 3.2 in our opinion is based on the examination of successive orbits. The main feature is a latitude dependent one, that's right. But again this seems to be rather a matter of point-of-view. We shall keep the current titles.

3.1 ONE ORBIT

19) p.13083, l.5: You would be right if averages of Delta h for 12 hours before and after 14 UTC were shown. But this is not the case. What is shown are Delta h values per orbit. Now, if per orbit the drift would shift the orbit-averaged Delta h a certain amount

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(fixed amount due to assumption of linear drift) the result were a uniform distribution for each of the time segments. Overlay two uniform distributions and you get a rather flat, single peaked distribution. That's what we state.

3.2 SUCCESSIVE ORBITS

20) p.13084, l.15: That is the first guess, of course, since ascending/descending orbit parts are dominated by night/day conditions. As we state in the text: Investigations are under way. Unfortunately, the second part of your remark remains unclear to us.

21) p.13084, l.16–25: No, definitely not.

3.3 CORRELATION OF LOS AZIMUTH AND DH

22) p.13085, l.4: The error of the roll angle is what we mean. Text changes throughout the paper will be applied.

23) p.13085, l.15: Correct, we mean the changes from one to the next (available) geolocation. Time-derivative would not always be correct, so we don't use this term.

24) p.13085, l.28: We cancelled the word good. We understand that you are not convinced of a strictly linear correlation. If so, we share this impression, although the correlation coefficients (now stated in the text) are well beyond the magnitude for 99% confidence. We shall change p.13087, l.14 to: If there were only the effect of an uncorrected error in the roll angle, both slopes, ... Further we shall add some text in the sense that we attribute the deviations between the slopes to the fact that the uncorrected error in the roll angle is not the only source for the latitude dependence of Delta h. The information you want to get by overplotting slopes can be drawn from Fig.11.

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25) p.13086, Eq.(2) There will be an appendix to the text where the derivation of the equation is given.

26) p.13086, l.5: See specific comment **24)**.

27) p.13087, l.16–18: As already explained (General Comment d)) the two time intervals between the orbit model parameter update at 2 and 14 UTC are unique, since at these times a sudden change in the platform attitude takes place. In our opinion it is reasonable to expect effects on the pointing in these two time intervals and consequently to separate the data accordingly.

28) p.1087, l.20: Text will be changed. Previously the value of the roll angle error was wrong. An appropriate correction will be implemented throughout the text.

4. MIPAS OPERATIONAL POINTING CHARACTERIZATION

29) p.13088, l.9: The time is meant, not the length of time. Text will changed to make this more clear.

30) p.13088, l.22: Text will be changed to avoid misunderstanding.

31) p.13090, l.3: A reference will be added and the corresponding tangent height offset will be given. Note that in the initial text a wrong value of 24 mdeg was given. This is now corrected to 26 mdeg.

32) Section 4 will be split into 4.1 Method and 4.2 Results. In the latter subsection we will include the major part of Sec. 5.1. The repeated explanation of the PSO software anomaly will be removed. Fig.13 will be included in this section and the differences will be discussed.

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RESULTS OF OTHER INSTRUMENTS AND CHARACTERIZATION METHODS

MIPAS

33) Agreed, see **32)**.

34) Agreed.

SUMMARY AND CONCLUSIONS

35) p.13093, l.23–25: The text will be changed to be more specific now.

36) p.13094, l.15–19: Fig.12 show all LOS calibrations used for the pointing characterization, including operational calibrations and the dedicated campaign in 2004. As we state on p.13089, lines 9-14, the corrections included in the engineering altitudes (L1b data) are not updated with the same frequency, but only if the difference between previous and current LOS calibration is bigger than 8 mdeg (450 m). This criterion never was met after the PSO update. Since end of 2002, the pointing correction to L1b data was fix and equal to 25 mdeg, because from the trend it was clearly a problem not depending from MIPAS.

37) Agreed, see **32)**.

38) Indeed there are differences in the FOV of the MIPAS detectors. However they are in the order of 1.3 mdeg (75 m tangent altitude difference). This will be stated in this section of the text. The cause of the 1.1 km bias currently is under investigation.

FIGURES

39) This figure will be reworked. The meaning of the thick lines will change. They will

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be longer because now they give a confidence measure based on Student's t. Figure caption will be changed accordingly.

40) Agreed.

41 & 42) Figure will be omitted from the manuscript, see **13 & 14)**

43) Date will be given in the caption.

44) p.13091, l.10–15: This figure will be reworked. Plots will consists of data of orbits 9027, 9028. Text will be changed accordingly, taking into account specific comment **26)**.

45) Figures will get an upper axis indicating the dates.

Technical corrections

Will be implemented.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 13075, 2006.