Atmos. Chem. Phys. Discuss., 6, S5945–S5955, 2007
www.atmos-chem-phys-discuss.net/6/S5945/2007/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 4 January 2007

Characterization of MIPAS elevation pointing (Kiefer et al)

Overview

The paper analyses the difference between the "engineering" tangent altitudes reported in the MIPAS L1B (spectra) and L2 (profiles) products compared to those retrieved using the IMK temperature-LOS retrieval. It is noted that the discrepancies are largely in the form of a bias, ie an altitude offset of the order of 1km applied to the whole profile, and the temporal behaviour of this bias is analysed in detail.

It is found that there are regular changes in the offset corresponding to the updates to satellite pointing commanded daily at 02:00 and 14:00 UTC, which have decreased in magntitude since a software updated Dec 2003.

However, the offset is also found to vary periodically around each orbit. The strong

correlation between latitudinal variation and azimuth pointing could be explained by an uncorrected roll angle of 54 mdeg.

The long-term variations also compare well with the independent "LOS observations" based on viewing stars within the FOV, although there is an unexplained offset of 1.1km in absolute altitude between the two.

General Comments

a) There ought to be a clearer distinction between absolute and relative pointing. You make several fairly strong statements that seem to imply that the pointing accuracy is critical to L2 data quality, whereas this is only true for those who, against all advice, use the altitudes rather than pressures associated with the L2 products eg for comparisons with other instruments or models. Most of the paper discusses this absolute pointing accuracy. Any significant errors in relative pointing, however, would be more seriously impact data quality since they imply some problem in the pressure-temperature retrieval, and consequently the subsequent VMR retrievals. However, apart from noting some vertical structure in Fig.2, you hardly discuss this at all.

b) Your paper makes it seem that the IMK processor is capable of retrieving absolute altitude from the MIPAS spectra. I presume, however, that what it really retrieves is a temperature profile registered on relative altitude or pressure surfaces, which are then compared to ECMWF fields for absolute altitude. The procedure used by the IMK processor is fairly central to this paper so you should explain a little more of how it obtains this absolute altitude, rather than just give a reference to the von Clarmann et al JGR paper. Given the above comment, is there actually any fundamental reason to use the IMK processor for this? Couldn't the same results have been obtained if you just directly interpolated the ESA L2 tangent point pressures to the ECMWF data and read the corresponding absolute altitudes? Does the use of the IMK processor add anything to this, or is it just a convenient source of ready-interpolated ECMWF data?

c) Somewhere at the start of the paper, and in the abstract/conclusions, you should

ACPD

6, S5945–S5955, 2007

quote the specified accuracies of the MIPAS pointing, both relative and absolute. These are, after all, the values that you are attempting to verify.

d) I didn't really understand why you place so much emphasis on splitting the data between 02:00 and 14:00. I note that the on-board pointing is updated at these times, and that the 14:00 update produces the bigger change in elevation angle, but isn't this just a daily correction to the gradual drift in offset? Or are you suggesting that there are some significant differences in the pointing behaviour between these updates that differ in the two 12 hour periods?

Specific Comments

ABSTRACT

1) Abstract line 5: you say that the retrieved tangent altitudes are independent of the engineering LOS information. Is this actually true? Doesn't the IMK retrieval at least use some information on the *relative* spacing of the tangent points?

2) Abstract line 18 & 24: for those not familiar with mdeg, please add equivalent tangent point altitude differences in km as well.

3) Abstract last line: is the 24mdeg the difference between your results and the operational LOS calibration, or the LOS calibration and ETA?

4) Abstract should include one of your main conclusions: that absolute altitudes are uncertain to +/- 1km, so MIPAS L2 data should only be compared on the tangent pressure levels.

1.1 MOTIVATION

5) p13077 line 2: Fischer & Oelhaf 1996 is probably no longer the official reference for the MIPAS satellite instrument. Isn't there an ESA SP?

6) p13077 line 5: you say that any errors in the assumptions about observation geometry map directly into the retrieved state variables but I don't think that's true. It is quite

ACPD

6, S5945–S5955, 2007

possible to add errors of several km to your assumption where the instrument is pointing and still retrieve perfectly reasonable state variables at the tangent points. (The only problem is if you then assign your incorrect altitudes to these tangent points, when the problem becomes one of mapping your retrieved state variables to your erroneous tangent altitude grid rather than the other way around.) (General comment a)

7) p13077 lines 11-15: I don't understand what you are trying to say here.

1.3 RETRIEVAL METHOD

8) p13078 You should emphasise that what you are describing here is the IMK retrieval and not the ESA retrieval. And I suggest expanding this section to give an overview how the IMK temperature/LOS retrieval works, specifically how ECMWF data is incorporated for the absolute altitude (General comment b)

1.4 SCOPE OF THIS WORK

9) p13079 lines 6-7: "Clearly the quality of this corrected altitude ..." only applies to the absolute values of the corrected altitudes. The relative values are independent of the accuracy of the lowest ETA (General comment a)

10) p13079 lines 6-7: This is the first, and last, mention of the corrected altitudes in the L2 data. Why haven't these results been compared with your IMK results for the relative altitudes? Presumably, since both rely on the spectral information and hydrostatic equations both should bear some similarity in terms of adjusting the relative spacing of the ETA?

11) p13079 line 15: Isn't there a simple mathematical equation that the azimuth scan is intended to approximate, eg azimuth = A.sin(latitude)?

2. ALTITUDE DEPENDENCE

12) p13080 lines 1-5: This is a very confusing way of describing the MIPAS nominal scan pattern. "6,9,...,39,42,47,52,60,68" might be more easily understood. You say

ACPD
6, S5945–S5955, 2007

EGU

that the lowest altitude and the spacing are slightly modified around the orbit. I assume you mean this is intentionally varied? If so, is it to keep the scan pattern as a constant function of elevation angle around some fixed reference altitude (so the variations are due to the earth's oblateness?)

13) p13081 lines 7-10: Although you say that there has been no change in the IMK scheme, is it possible that there has been some change in the ECMWF data? To make the cause of the change in relative altitudes clearer, it would be useful to add a plot of averaged ETA v nominal ETA for the corresponding data - if there was any deliberate adjustment in the scan pattern then it should also show up in this plot. It would be difficult to understand how there could be any non-deliberate change in relative altitudes with an accompanying change in absolute altitude.

14) p13081 lines 10-14: you say that this plot shows the rms values of Delta h_i profiles, ie the rms values of Delta h_i about zero. However since Fig.2 shows typical Delta h_i magnitudes of order 1 while Fig.3 shows rms values of the order 0.2, perhaps you mean the standard deviation, ie the rms values of Delta h_i about Delta h for each profile. Or perhaps the rms value of Delta h_i about the averaged value for each orbit. Anyway, please clarify.

15) p13081 line 11: (concerning the rms values of around 200m) Earlier (p13078, lines 20-21) you state that you believe the accuracy of your results is of the order of 200m, but you don't give the precision, which, I assume is a better defined quantity. Couldn't you use the precision of your results compared with the observed SD between your altitudes and the ETA altitudes to derive some sort of precision estimate on the ETA altitudes? This would be a fairly important number to know, since its assumed value forms part of the a priori constraint on the pT retrieval within the ESA processing (General comment a)

16) p13081 Eq(1): Put another way, this is just the difference between the average of the ETA altitudes and the average of the retrieved altitudes. Since this average will

ACPD
6, S5945–S5955, 2007

EGU

include a lot of "noise" due to your temperature retrieval, I wonder if it would be better (ie less noisy) to compare just the single tangent altitude in your retrieval which is most closely tied to ECMWF altitudes (I'm assuming that the absolute altitude of this level in your retrieval is more accurate than the others).

3. TIME DEPENDENCE

17) p13081 line 25: How does this jump relate to the one described in von Clarmann et al which was rectified in June 2003?

18) Your titles of sections 3.1 and 3.2 are a bit confusing. What you really discuss in 3.1 is the diurnal variation and 3.2 the latitudinal variation.

3.1 ONE ORBIT

19) p13083 line 5: I don't understand the comment about a linear drift. If there is a single update in the offset each day at 14:00, as noted in Fig.4, then I would expect a distinct difference in Dh measured in the 12 hours following the update to the 12 hours before, ie two peaks, as you have. Perhaps I'm missing your point (General comment d)

3.2 SUCCESSIVE ORBITS

20) p13084 line 15: Could this ascending/descending difference be related to day/night differences in your temperature retrieval? For example, is it common to all tangent altitudes that make up your averaged Delta h?

21) p13084 lines 16-25: although you note other features, to me it seems that many anomalies in (in both ascending and descending) occur at regular latitude intervals of 30 degrees, ie latitudes of -60, -30, 0, 30, 60. This makes me wonder if your temperature retrieval uses some sort of climatology or other database that is tabulated at these intervals?

3.3 CORRELATION OF LOS AZIMUTH AND DH

ACPD

6, S5945–S5955, 2007

22) p13085 line 4: do you mean that roll-angle itself is not accounted for in the data processing (by which I assume you mean the calculation of ETA), or do you mean that there is some error in the roll-angle which not accounted for? I assume you mean the latter but it reads as if you mean the former.

23) p13085 line 15: it took me some time to understand these definitions, but it seems that what you mean are the *changes* in Delta h and azimuth in consecutive limb scans, equivalent to a derivative with respect to time.

24) p13085 line 28: I'm less convinced about the "good" linear correlation between Da and D(Dh) from Fig.9. However, according to your theory (as also stated in p13087, lines 14-15) the slope in Da v D(Dh) should be the same as the slope in a v Dh so perhaps if you overplotted the slope derived from a v Dh relationship in the lower left panel on the lower right panel (constraining it to pass through coordinates [0,0]), and showed that it closely matched the fitted line in the lower right panel, it would be more convincing.

25) p13086 Eq2: this relationship isn't obvious to me. Is there a simple derivation or a reference?

26) p13086 line 5: Is the same value 0.045 derived from both plots, or is this an average? See also comments above relating to these plots.

27) p13087 lines 16-18: Could you explain why you choose to separate the data into two parts of the day rather than, say, ascending and descending halves of orbits (General comment d)

28) p13087 line 20: State which of the various figures listed in Table 1 you have used to derive this roll angle, and also what (approximate) tangent height displacement this would correspond to if viewing at 90deg azimuth (for comparison with the Dh values of 1km that you have derived).

4.0 MIPAS OPERATIONAL POINTING CHARACTERISATION

ACPD

6, S5945–S5955, 2007

29) p13088 line 9: Do you mean "crossing time" in the sense of the "length of time it takes for the star to cross the FOV" or do you mean the "time at which the star crosses the FOV". I expect you mean the latter but it could be misread as the former.

30) p13088 line 22: "scanning repeatedly" - earlier you say that the interferometer mirrors are fixed during such observations so I assume that you mean scanning in the elevation sense. But "scanning" implies some sort of continuous mirror movement (as seems to be used in the sideways viewing direction) but from the rest of what you say it seems that the mirror is stepped rather than scanned for the rearward views.

31) p13090 line 3: there should be a reference for this figure of 24 mdeg, but you should also say what this corresponds to in terms of tangent height offset for comparison with your earlier results, particularly in terms of sign.

32) In this section you show the results (Fig12) of the operational pointing but here I would also expect some discussion of how these results compare with your own (ie a reference to Fig 13 and the 1.1km offset). This, and the repeat of the explanation of the PSO software anomaly gives the impression that this whole section is an independent piece of work which has been included in this paper without much attempt at integration.

5 RESULTS OF OTHER INSTRUMENTS AND CHARACTERIZATION METHODS

5.1 MIPAS

33) You have already shown (in Fig 12) the results of the MIPAS operational pointing characterisation, but here you refer for the first time to results of Saveedra et al. Are these different to the results in Fig 12? If so, how? I am confused why you choose to present some of the results of the MIPAS operational pointing in the previous section and some here.

34) p13091 lines 10-15: since this is unpublished material, much of this paragraph should probably be reduced to A Dudhia (private communication). You should also

ACPD
6, S5945–S5955, 2007

EGU

state which period of data this referred to.

6 SUMMARY AND CONCLUSIONS

35) p13093 lines 23-25: when you say "most quantities which characterise the mis-pointing" depend on the time, are you just referring to the offset which gets adjusted at 02:00 or 14:00? (General comment d)

36) p13094 lines 15-19: you seem to be saying that the mispointing information derived from the operational LOS measurements shown in Fig.12 have already been used to correct the engineering altitudes in the L1B data. However, if this were the case, you would not expect any similarity between those results and your own, whereas Fig.13 shows that they both have the same structure.

37) Conclusions are probably not the best place to introduce new information such as your Fig.13 and the figure of 1.1km offset between your results and the operational pointing.

38) The agreement between the operational LOS pointing and your results appears quite good, but you haven't really explained where the 1.1km bias could come from. Presumably you've ruled out any difference in the FOV of the channel D detectors and those from the A band which are presumably used for your pT retrieval?

FIGURES

39) Fig.2: Not obvious which are the thick and thin horizontal lines.

30) Fig 2: Sentence beginning "Only heights where..." doesn't make much sense unless you read the text, so perhaps should not be in the caption.

31) Fig.3 see comments above on p13081 lines 10-14

32) Fig.3 It would be helpful to have dates along the top axis.

33) Fig.8 it would be helpful to mark the latitudes at which the azimuth angle is

ACPD

6, S5945–S5955, 2007

changed. Also, in the figure caption, can you give the dates at which these orbits were acquired.

34) Fig.9 Since, in the text you argue that much of the structure in Fig.8 is due to changes in azimuth angle, it would be better if the data plotted in Fig.9 corresponded to one of the orbits plotted in Fig.8. The orbit you have plotted (2886) is rather early in the mission (August 2002) and it is conceivable that azimuth scan patterns, roll angles etc were adjusted between that time and the orbits shown in Fig.8.

35) Fig.10 & 11: Would be helpful to have dates as well as orbit number along x-axis.

Technical Corrections

Abstract, line 2: "allows the retrieval of" instead of "allows to retrieve"

p13078 line 16: suggest "better known" rather than "even better predictable"

p13079 line 4: "building", and "a hydrostatic"

p13080 line 3: "slightly"

p13080 line 24: dependence ... "from latitude" should perhaps be "on latitude".

p13080 line 25: along THE orbit

p13082 line 2: "appearance"

p13084 line 6: I think you mean "consecutive" rather than "subsequent"

p13084 line 11: "follows quite well" instead of "quite well follows"

p13084 line 12: "appearance"

p13086 line 5: Presumably "Fig 9" rather than "Fig 11".

p13086 line 25: "beginning of November 2002 through to early September 2003".

p13089 line 1: "six-parameter model"

ACPD

6, S5945–S5955, 2007

EGU

p13089 line 19: "...used as a backup. However, ..."

p13089 line 26: "used" rather than "asked".

p13090 line 8: " ... to be responsible ..."

p13090 line 9: "aspect" or "matter" rather than "regard".

p13090 line 15: "radians"

P13090 line 34: inconsistency in spelling "characterization" (here) compared with, for example, "characterisation" in title of section 4.

Fig.4 "beginning" rather than "begin" in plot x-axis titles

Fig.4 "three" rather than "threee"

Fig.5 "parameters" rather than "pareameters".

Fig.10 & 11 y-axis titles are clipped/illegible

———-

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