Atmos. Meas. Tech. Discuss., 5, 3797-3835, 2012

Interactive comment on "Precise pointing knowledge for SCIAMACHY solar occultation measurements" Final response by the authors

We thank the anonymous referees for the useful and detailed comments and their suggestions to improve the article.

The answer to the reviewers will contain the following parts:

1. Main concern of the reviewers: Extrapolation length.
2. Main concern of the reviewers: Error considerations.
3. Detailed answer to referee #1.
4. Detailed answer to referee #2.
5. Summary.

1 Main concern of the reviewers: Extrapolation length

Main concern of both reviewers is the long extrapolation from the scans over the solar disk above 100 km to 17.2 km, as shown in fig. 6 of the manuscript. We agree, that selecting 100 km as "boundary" of the atmosphere is somewhat arbitrary.

Therefore we investigated the effect of the refraction to the tangent height of the sun. Fig. 1 shows the difference between the the apparent solar position and the geometric solar position versus the (geometric) tangent height. This is calculated (using also the CFI library), for a wavelength of 650 nm (center wavelength of PMD 4).

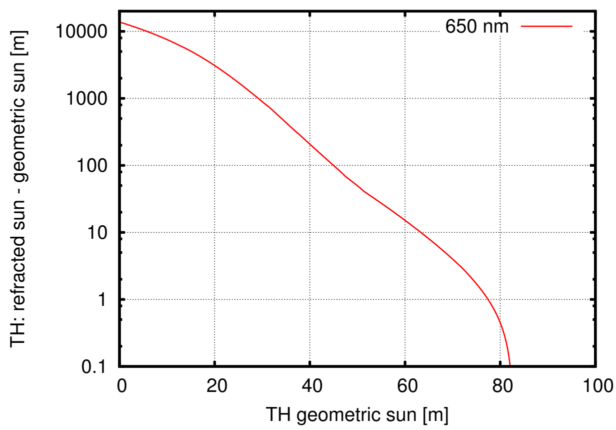

Figure 1: Refraction

At 40 km, the difference is ∼200 m, at 60 km the difference is still about 15 m. At 75 km, the remaining offset is 1.7 m, which we will now consider to be negligible for our calculations. With that, we gain 6 additional points for the determination of the straight line used for the extrapolation. The extrapolation is then from ∼75 km to 17.2 km. In the dataset used for the reviewed version of the paper the extrapolation was from ∼120 km to 17 km (The first used scan was completely above 100 km with the center of the solar disk at ∼120 km, which was not clarified in the manuscript).

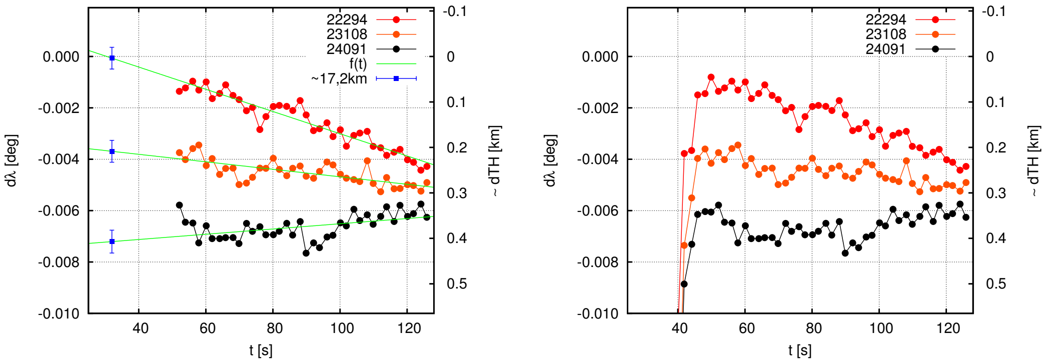

Figure 2: Left plot: New fig. 6 of the manuscript with 6 additional points. Right plot: The individual EAOs are also calculated for the scans over the solar disk below ∼75 km. Before ∼46 s (corresponding to ∼59 km tangent height) the effect of the refraction is clearly visible.

The left plot in fig. 2 shows the updated version of fig. 6 in the manuscript. The new extrapolated values agree within the error bars with the ones determined before. However, the error bars will be discussed separately in the next section.

We think, that the insensitivity of the results to the additional six points show, that the assumption of a linear dependence of the elevation angle offset (EAO) from the measurement time is valid.

The right plot in fig. 2 shows the influence of the refraction at lower altitudes (40 s corresponds to about 42 km).

For the updated paper, we will use then all scans starting at ∼75 km. All plots will be updated according to the new dataset. This will be updated for the elevation as well as for the azimuth angle offset.

2 Main concern of the reviewers: Error determination

We agree especially to referee #1, that the error description in section 3.5 is not sufficient. Two error sources have to be considered:

1) As seen in fig. 6, the EAO for the individual scans scatter around the fitted line. This uncertainty for the individual scan will also disturb the retrieval of an atmospheric parameter at the lower altitude. It can be calculated as standard deviation of the distance of the individual EAOs from the fitted lines. This error represents the limited precision of the scanner unit.

Preliminary value is about 0.44 mdeg (25m). This value will be confirmed by a larger dataset for the final version of the manuscript.

2) The error from the linear fit and the extrapolation. The linear regression of the data gives also the covariance matrix of the fit parameters. This is used via error propagation to calculate the standard deviation of the extrapolated EAO at 17.2 km. For the examples in fig. 5, this error is about 0.23 mdeg (14 m).

The addition of these two errors gives the remaining uncertainties for tangent heights at 17.2 km. We will extent the description in section 3.5 accordingly.

3 Detailed answer to Referee #1

Citations from the referee are set in *italic*.

This paper describes an approach to improve pointing knowledge for SCIAMACHY solar occultation measurements by finding the brightest point on the solar image encountered during a vertical sweep of the instruments scanning mirror. Deriving the pointing information associated with atmospheric measurements from satellites is challenging. Using measurements of the sun to calibrate out small deficiencies in the pointing model calculations is a good idea. The basic approach I think is quite reasonable but perhaps suffers a bit from a long extrapolation to the altitude region of greatest interest.
Concerning the extrapolation, see section 1.

As I understand it, the elevation angle offsets are determined in the tangent altitude region of 100 to 290 km, and then a linear extrapolation is performed down to 17.2 km. The measurements at lower tangent heights are the ones actually employed in the analysis, since that is where absorption by atmospheric constituents is significant. So, the pointing information for the measurements actually used in the analysis relies on this extrapolation. The approach described here appears to neglect any possible error associated with this extrapolation (for example, due to deviations from linearity of the angle offset as a function of time). The extrapolation is quite far, more than 80 km, and has the potential to introduce errors. The plots in figure 6 do not show the most obviously linear data I have ever seen. It looks like dropping the first couple of points could generally make a difference in the extrapolation, which means there should be a contribution from extrapolation included in the error budget.
Concerning the extrapolation, see section 1. The error budget will be revised as discussed in section 2.

The authors are not completely clear on the reasoning for the long extrapolation. On line 220, they mention that they use tangent heights above 100 km where refraction due to the Earths atmosphere is negligible. If that were the only consideration, I would expect they could push the analysis to lower altitudes to have a shorter extrapolation. Looking at figure 2, the differences between the red and yellow shaded areas at low altitudes are due to refraction. Above about 50 km, the solar disk would seem to be described very well by geometry. At lower altitudes, there might be other considerations required in the analysis (depending on the wavelengths involved), such as an intensity gradient across the solar disk from atmospheric extinction, but you would have a much shorter extrapolation. Could you push the method lower? You could always include an empirical function in your fitting that would account for the gradient in atmospheric extinction.
We will start the fitting of the EAO now at $\sim$75 km, where the effect of refraction becomes very

small (see section 1).
Broadband extinction is also still negligible small above 75 km, there is no need to take this into account.

This probably does not need to be addressed in the paper, but wanted to mention that I believe the intensity in equation 1 should be an integrated intensity (i.e., integrated over wavelength) rather than the measured intensity at a particular wavelength. The paper does not actually say how the measured intensity is generated. The sun is rotating, and there will be Doppler shifts toward the limb of the solar disk associated with this rotation. I believe this is further complicated by the fact that the rotation axis of the sun (as viewed from the satellite) will vary over the course of the year. When scanning over the solar disk, I expect an intensity measured at a particular wavelength would be more sensitive to the Doppler shift from solar rotation than would an integrated intensity.
We use the measurements of the polarization measurement device (PMD) of SCIAMACHY, which is a broadband detector, covering more than 200 nm in case of PMD 4 (see table 1 in the manuscript). So we indeed use an integrated intensity.

Using times in equation 1 assumes negligible contribution to changes in pointing from motion of the spacecraft. If there is some drift in spacecraft orientation (e.g., from the spacecraft nutating a rate that is slow compared to solar scan), I expect you would get different widths to the upscan and downscan features in figure 5.
We only assume, that the pointing of the instrument is changing with a constant rate during one scan (2s). If this would not be the case, the shape of the intensity function (fig. 5 of the manuscript) would be disturbed.
The spacecraft orientation is described by the orbit and attitude information of the satellite. What we observe, is a remaining pattern of these quantities. We have shown, that the elevation and azimuth angle offset is in most case time dependent during one measurement sequence. This means, we observe indeed some drift in the spacecraft orientation. In principle, this would also cause a difference width of the upscan and downscan, but this effect would be very small: $\sim$0.1 mdeg on a solar disk of $\sim$530 mdeg. The radius is fitted as independent quantity in eq.(1) of the manuscript and will therefore not disturb the fit of the solar center.

The solar corona is listed as a source of error. Is the corona relatively uniform about the solar disk? Unless the emission characteristics of the corona is somehow different than for points inside the solar disk, would the corona not simply change the effective radius of the sun? Since radius is a fitting parameter, the determination of solar center should not be affected.
We agree to the reviewer. Variations of the solar corona are unlikely to be a source of error. We will remove this error source from the manuscript.

For field of view, could you not integrate over the field of view? Did you consider using some sort of empirical function to account for limb darkening?
We considered both possibilities. However, we did not implement this, because the fitted function (see fig. 5 of the manuscript) for PMD 4 shows already a very good agreement. We wanted to keep the method simple, because in this work we concentrated on deriving the tangent heights.
If we want to fit the center of the solar disk at lower tangent heights, a proper description of the limb darkening, a field of view integration and a good refraction model might be necessary. We avoid this complication to be independent from additional assumptions.

You state that the best errors are achieved with PMD 4 but show no results for other PMDs. Was there agreement (within the errors) in the results derived from different PMDs? Were there

systematic differences between results for the different PMDs?
Selection of the PMDs is based on the validity of eq.(1) for the intensity function of the scan. Fig. 3 is an example for PMD 1:

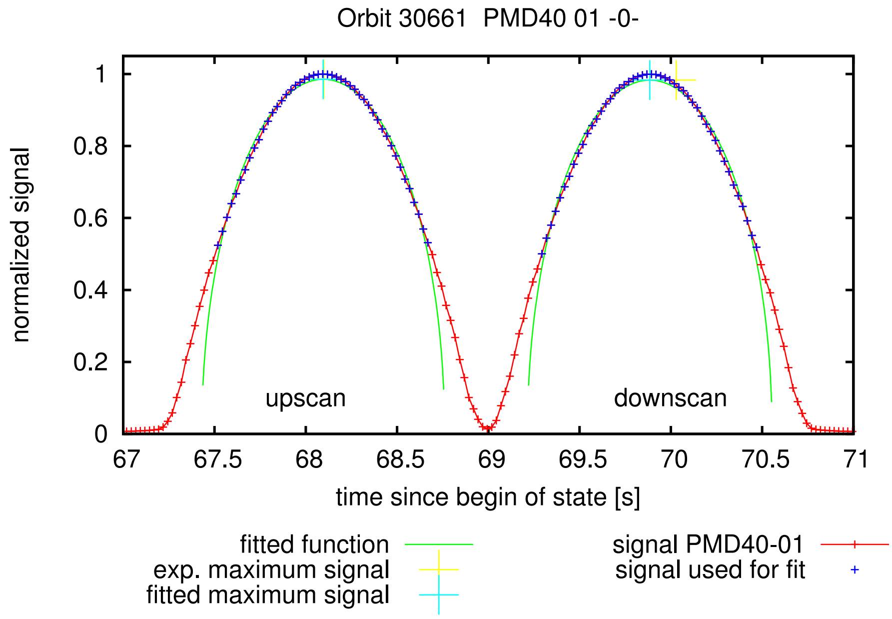

Figure 3: As Fig. 5 of the manuscript, for PMD 1 and orbit 30661.

The fitted function (green) does not fully match the shape of the measured intensity. This leads to larger error on the fitted parameter. As mentioned in the manuscript, these errors were investigated to select PMD 4.
We have not investigated the results of the other PMDs in detail. A quick look for the three orbits used in fig. 5 of the manuscript give the plots in fig. 4: In most cases, the results agree

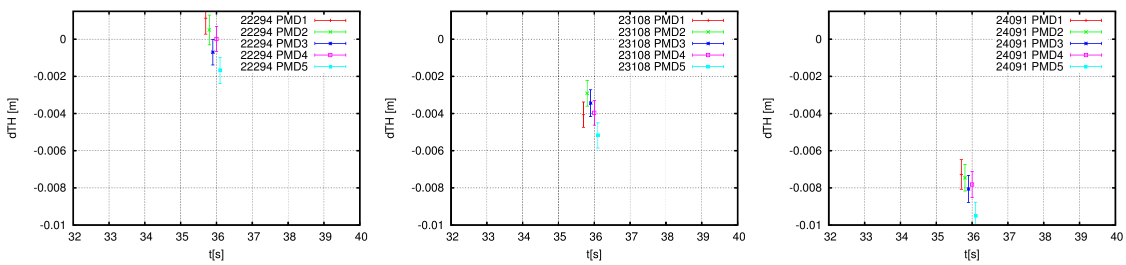

Figure 4: Elevation angle offsets using the PMDs 1-5 for the orbits given in fig. 5 of the manuscript. The errorbars are horizontically shifted for better visibility.

within the error bars (here already calculated as described in sec. 2). Please note, that we have not optimized anything for the other PMDs. Especially, the "analogue delay" as described in section 3.5 of the manuscript has not been checked.
We will add a sentence in manuscript, that the agreement of the derived EAO between the different PMDs has been checked for a subset of orbits.

There are a number of grammar problems in the paper that should be corrected. On line 63, the

acronym SFD is used without definition. It is not defined until line 349. The acronym CFI is used without definition.
We will carefully check again the grammar of the paper. The definition of the acronym SFD will be moved to p. 3800 and the acronym CFI (Costumer Furbished Item) explained.

To reiterate, my main concern is the fact that the precision of the method seems to pertain to the measurements above 100 km. However, these measurements will not likely even be used to derive information on atmospheric constituents. It is more important to derive pointing at the lower altitudes, where you have absorption by atmospheric constituents that can be used in analysis. The pointing information in that altitude region will depend on a long extrapolation, and so I think it is important to assess a contribution to the pointing error from the extrapolation, and if possible push the method to lower altitude (i.e., below 100 km) to reduce the extrapolation distance.
We have addressed both concerns in the sections 1 and 2. The data will be recalculated for the final version of the paper.

4 Detailed answer to Referee #2

Citations from the referee are set in *italic*.

This study by Bramstedt et al. reports on the precise registration strategy of tangent height from SCIAMACHY solar occultation measurements. The information of accurate tangent height for atmospheric occultation measurement is essential for scientific analysis using the satellite data. The authors described a method to determine the Elevation Angle Offset (EAO) by fitting the up- and down-scan above 100km, and then extrapolating it to lower altitude. It might be a good idea to publish this result in AMT if this method is proven solid. However, I felt that the authors have not shown enough evidence to convince the readers that the extrapolation is valid. Please see each comment shown below, and consider appropriate revision.

1) The authors fit the up- and down-scan signal from PMD 4 to determine the position of solar disk with tangent heights above 100 km for state 49 (Fig. 5). Then, they determine d(lambda) for 30 scans, and fit to a linear line and extrapolate it to the value down to 17.2 km (Fig. 6). They claimed that tangent heights above 100 km are used because refraction due to Earths atmosphere is negligible there. However, I think that refraction due to Earths atmosphere is still enough small even at lower altitude around 50 or 60 km. On the other hand, I worry about the reliability of extrapolation of a linear fit curve from above 100 km to 17.2 km. I want to see the EAO values between 17.2 and 100 km in Fig. 6 to see the validity of the extrapolation.
We will reduce the extrapolation, as described in sec. 1.
The basic idea of the method is, that we measure the solar position from the scan over the solar disk. From the measured solar position, we derive the elevation angle offset (corresponding to the geometric tangent height).
Between 75 km and 17 km, the (visible) solar position is changed by the refraction in the atmosphere, so the method is no longer valid. We cannot directly determine the EAO below 75 km without assumptions of the atmosphere. The main advantage of this method is, that we are independent from any knowledge about the atmosphere.

2) The authors dont show any other proof to show the absolute pointing error of the SCIAMACHY occultation measurements. Is retrieved minor constituent profile improved with this new tangent height registration method? If it can be proved by showing the improvement of SCIAMACHY profile compared with some other validation data (e.g., ozonesonde profile), it would be nice to see such a figure in this paper.
The mean tangent height offset is in the order of 250 m. If the derived tangent heights are an improvement, cannot be decided on a few co-located profiles. First retrieval results indicate, that we indeed have an improvement. However, full validations (of ideally more than one product) would be necessary to prove this, which is beyond the scope of this work.
Nevertheless, for a single profile, fig. 5 shows a comparison of an ozone profile from SCIAMACHY solar occultation using all corrections mentioned in this paper with a co-located profile from the HALOE instrument. The agreement is a very good.

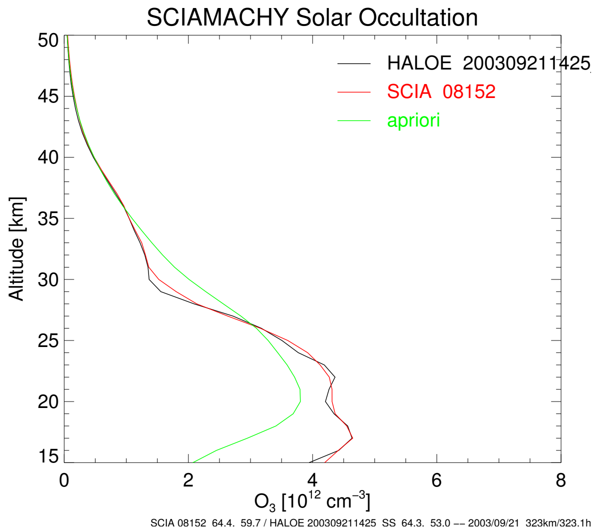

Figure 5: An ozone profile from SCIAMACHY solar occultation, compared to a co-located HALOE profile

3) The numbers (achieved precision, offset, seasonal cycle amplitude for both EAO and AAO) are shown in several part of this paper, but some of them are inconsistent among in contents, in conclusion, and in abstract. Please check the values. Also, it would be a good idea to add a table to summarize the result in errors both in angle [mdeg] and tangent height [m] units.
We apologize for some remaining inconsistencies in the calculated numbers throughout the text. With the recalculation necessary we will update and carefully harmonize the numbers.
We agree, that a summarizing table will be useful and will add it to the conclusions.

Minor Comments:

1)P.3799, L.21: 0.0045 deg → 0.045 deg
We will change it, OK.

2)P.3799, L.23: 0.0045 deg → 0.045 deg
We will change it, OK.

3)P.3800, L.19: 0.08 deg → 0.008 deg
We will change it, OK.

4)P.3800, L.20: the term SFD first appeared here, but not explained (first explained in P.3812, L.24)
We will move the explanation to this point in the paper.

5)P.3802, L.4: sun follower device → SFD
We will change it, OK.

6)P.3802, L.8: sketched Fig. 2 → sketched in Fig. 2
We will change it, OK.

7)P.3802, L.12: 0.0045 deg → 0.045 deg
We will change it, OK.

8)P.3804, L.16: to the the maximum → to the maximum
We will change it, OK.

9)P.3804, L19: the term CFI first appeared here, but not explained.
CFI stands for "Customer Furbished Item", we will add this.

10)P.3806, L.10: to noisy to be used → too noisy to be used
We will change it, OK.

11)P.3811, L.13: Fig. 10e → Fig. 10f
We will change it, OK.

12)P.3811, L.15: Figure 9e → Figure 9d
We will change it, OK.

13)P.3815, L.16: 0.025 deg → 0.0025 deg We will change this to 2.5 mdeg.

14)P.3815, L.23: as for the EAO → as for the AAO (?)
This sentence is misleading, we will rephrase it:
Figure 13b shows the trend analysis for the AAO. As already done for the EAO, the function described by Eq. (4) is fitted to the AAO.

15)P.3816, L.5: mean error is about 1.1 mdeg → mean error is about 0.6 mdeg (?)
The referee is correct here. The values will be revised for the final version.

16)P.3817, L.3: an amplitude of 3.4 mdeg → an amplitude of 2.4 mdeg (? P.3815, L.2)
Yes, 2.4 mdeg is the correct value.

17)P.3817, L.3: offset of 93 mdeg → offset of 89.5 mdeg (? P.3815, L.27)
The value of 93 mdeg is correct here, this is the mean offset in the observation period. In the paragraph of page 3815, the offset at orbit 0, i.e. the fit parameter C of Eq. 4 is given. We will add the number given in the conclusions to the earlier paragraph.

18)Figure 10H: The color between state 47 and state 47 (SFD) is very similar and are hard to distinguish. Please change one of these colors, or change the symbol.
We will select an appropriate color.

5 Summary

For the revision of the paper, we will recalculate the dataset taking into account the scans starting at 75 km (sec. 1) and improve the error budget (sec. 2). We adress the minor comments as mentioned above.
We will also use the possibility to extend the dataset by one month until the 8th of April 2012, when the communication to Envisat was lost.