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Interactive comment on “Precise pointing knowledge for SCIAMACHY solar occultation measurements” by K. Bramstedt et al.

Anonymous Referee #1

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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 instrument’s 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.

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

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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.

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 Earth's 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.

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

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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.

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.

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. 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?

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?

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.

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.

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