

## ***Interactive comment on “Surface solar irradiance from SCIAMACHY measurements: algorithm and validation” by P. Wang et al.***

**Anonymous Referee #1**

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### General comments

The paper addresses a very interesting topic, and provides an assessment of a new product derived from SCIAMACHY satellite. The paper discusses new measurement techniques and as such falls within the scopes of AMT. Solar irradiance data at the surface are relevant for climate studies, and a validated dataset will be very useful for the scientific community. The paper presents and discusses a new algorithm to parameterize the cloud effects on incoming solar irradiance. The algorithm and the derived product are validated against measurements at the surface and against other satellite derived datasets. The methodology is adequately described and the statistical methods used to assess the data are valid. Both the abstract and the title reflect clearly the contents of the paper. The overall structure and presentation of the paper is clear.

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If the comparisons between datasets were shown as ratios, then the overall picture would have not been as good as appears in the paper. The scatter plots are dominated by the annual variation of the solar zenith angle and large part of the correlation coefficient is due to this variation. On the other hand, one cannot expect that satellite derived irradiance data can agree very well with ground based measurements a) because satellite estimates are based on modelling and b) because the two measurement techniques (from ground and space) are based on different principles. Because of this I would not insist in changing the type of the comparison plots.

### Specific comments

11, 14: Use seasonally varying ozone? What happens over Antarctica during the ozone hole? Is fixed albedo appropriate for ice/snow covered areas? How about fixed Water vapor?

11, 19: What correction formulas and parameterizations were used? How large the correction factors are?

14, 18: How ice/snow pixels are identified?

15, 9: Averages of daily SSI for local noon over 3 months would be affected by day-to-day changes in the sza.

15, 18: The effect of the structure in the albedo is quite important. How the authors plan to cope with it? Same comment for the statement in 18, 19.

16, 21: It would be important to show the validation results also for SBO site, because this would reveal the importance of the albedo problem in FRESCO.

17, 17: I would prefer to see the figures as a function of day of year instead of case number. This way any seasonal features would be more evident.

17, 18: Undoubtedly the comparison is good, but is dominated by the seasonal cycle of the sza which improves the correlation (as seen in Fig. 6). This should be mentioned

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in the text.

17, 22: Have these differences a preference in sign? I would expect that the ground stations measure less over islands because usually clouds are formed over land. Hence ground based measurements should be directly influenced, while the satellite measurements would not, as they take into account the average albedo of the corresponding pixel.

18, 21: I am not convinced that this statement is true when looking at Figure 7.

19, 11: Is the quoted large standard deviation (50%) the average std derived from all 60 minute intervals over a year?

22,1 : From Figure 8 it appears that the two datasets agree well with respect to the shape and broad details which mainly comes from the latitudinal variation of the solar zenith angle. This does not constitute a "very good" agreement. I would prefer a softer expression.

43, 5: Is the color scale logarithmic (doesn't look like) or the data are logarithms of the density?

Technical corrections

10, 11: Shouldn't it be -.02 (minus)

47: The first 3 columns in table 1 should be aligned vertically.

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Interactive comment on Atmos. Meas. Tech. Discuss., 4, 873, 2011.