

Interactive comment on “Long-time global radiation for Central Europe derived from ISCCP Dx data” by N. Petrenz et al.

Anonymous Referee #2

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General comment: In this study, the authors use several versions the ISCCP D datasets as input to a radiative transfer model in order to obtain estimates for surface solar radiation in central Europe over the period 1984-2000. So far, our knowledge on the temporal and spatial distribution on surface solar radiation is still afflicted with unsatisfactory large uncertainties, and any attempt, such as the present study, to reduce these uncertainties should be encouraged. The main consideration I have with this study, is to what extent this attempt has been successful in the present case. In my point of view the authors should point out more clearly, what added value their methodology can provide compared to already existing approaches. There have been various attempts to estimate surface solar radiation based on satellite data, such as done by the groups at NASA Langley (Paul Stackhouse, GEWEX SRB), University of Mary-

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land (Rachel Pinker), NASA GISS (Bill Rossow, ISCCP FD) or the ESRB of FORTH, Greece (Ilias Vardavas), to name a few. I would be happy to see a discussion on how the methodology proposed in the present work differs from these approaches, and what the potential advantages of this new method could be compared to the existing ones. I see one argument is a higher spatial resolution. But is it the only one? This is even more important as the results of this method by themselves do not suggest a great improvement compared to earlier estimates. Rather they suggest the opposite. A comparison with two of the existing estimates (ISCCP FD and GEWEX SRB) suggests a systematically higher surface solar radiation calculated by the new method compared to these two earlier datasets: The authors state that their estimate is on the order of 25 Wm⁻² higher than the GEWEX SRB and 5-20 Wm⁻² higher than the ISCCP FD estimates. Later on the authors state that their estimates are also 20 Wm⁻² higher than the surface reference measurements. Therefore this gives me the feeling that these new estimates might be substantially stronger biased (in terms of overestimates) than the earlier estimates from ISCCP FD and GEWEX SRB (at least in the mean). The overestimation of surface solar radiation in satellite derived estimates and climate models is a known and well documented problem in the literature. I am not sure to what extend this new approach can help to remedy these problems. To sum up, for the publication of the present manuscript I would at least like to see a more in depth discussion of both methods and results in the context of the considerable amount of literature that exists on the estimation of surface solar radiation. This would then allow a better identification of the contribution of the present study to improve our knowledge on the spatio-temporal distribution of surface solar radiation, which should be the ultimate aim of this study.

Further detailed comments:

Section 1 introduction:

1. page 8335 line 15: omit “or derived”, all mentioned programs provide direct flux measurements, as opposed e.g. to the satellite-based estimates which provide “derived”

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fluxes.

2. p. 8336 line 7: add “the sum of” direct and diffuse incoming solar

Section 2.1 data sources:

3. Problems of inhomogeneities in the ISCCP time series have been discussed also in other studies, which should be mentioned here, such as in Evan et al. 2007: Arguments against a physical long-term trend in global ISCCP cloud amounts GRL 34 (4): Art. No. L04701 FEB 17 2007.

4. page 8837: is the time averaging (three hourly versus three-hourly monthly means the only important difference between D1 the and D2 datasets? In light of the considerable differences caused by the different ISCCP D datasets in Figure 3 I assume there must be further differences between the datasets. The authors may comment on this.

5. page 8337 line 15 should be Fig 1a and not 1d

6. Fig. 1: The gray curves in Figures 1b) und 1c) are not discussed. What do they represent? The figure caption should mention this also. The figure caption is in general too concise. It should contain all relevant information to understand the Figure. Contoured areas are not well discernible. An additional hatching/shading may help.

Section 2.2: 7. Why is it necessary to work with lookup tables? Would full Streamer calculations for every time interval not be feasible with current computational resources? To my knowledge other products such as the ISCCP FD calculations do not have to rely on such simplifications. Can the additional uncertainties due to the introduction of lookup tables be estimated, e.g. by comparing continuous streamer calculations for limited number of gridpoints and time periods with the interpolated look up table estimates?

8. page 8340 line 8. Similar trends in global radiation over Europe have been noted in previous studies based on surface observations (Wild et al. 2005, Science 308). Note the reasonable qualitative agreement of the present Figure 2 with Figure 1 of Wild et

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al. 2005, which may be worth mentioning.

Section 3.1 results:

9. page 3841 line 11. add “compared to SRB and ISCCP FD” after “Central Europe”
10. page 3841 line 12. “considerable differences COMPARED TO” rather than “for”
11. page 3841 line 13 “suggesting an incorrect atmospheric parameterisation”: of which product? SRB? Dx? “Incorrect atmospheric parameterisation” is a fairly general statement and rather meaningless. This paragraph needs to be formulated more carefully. As noted in the general comments, a more detailed discussion on potential causes for the large discrepancies between the different estimates (despite using similar ISCCP data input) could greatly enhance the quality and usefulness of the paper.

Section 3.2: 12. It would be worthwhile to perform a similar evaluation with GEBA and DWD data not just for the newly estimated Dx fluxes, but also for the SRB and FD fluxes, in order to get a handle on the uncertainty of the new product compared to the existing SRB and FD products.

13. Fig. 6 (right) does this figure not show DWD - Dx rather than Dx - DWD as given in the Figure caption? As it stands, Dx-DWD would signify an underestimation of Dx at all German sites which is not the case.

14. Fig. 7 can be omitted since DWD and German GEBA sites are virtually the same.

15. p. 8343 last paragraph of section 3.2, related to trends. Increasing surface solar radiation as seen in the surface measurements in Germany over this period is again in line with earlier studies pointing to a “brightening” in surface solar radiation measurements over this period (Wild et al. 2005, Science 308). My suspicion on why the satellite estimates do not show a similarly strong increase, is due to their difficulties in treating changes in absorbing aerosol, which seem to have played a significant role in the transition from “dimming to brightening” of surface solar radiation.

Section 3.3: 16. the smaller difference in winter can be expected since the absolute

values are also smaller. 17. Fig. 9 (right) there are no absolute values given on the y axis. 18. Good agreement of Dx with REMO in summer might indicate also an overestimation of surface solar radiation of REMO in summer, according to the evaluation of the Dx dataset in Section 3.2. Overestimation of surface solar radiation over continents in summer is a common problem in many climate models, partly caused by an excessive summer dryness, as e.g. first noted in Wild et al. 1995, Climate Dynamics 11:469-486.

19. Despite the fact that I am not of English mother-tongue myself I got the impression that the English in the paper could benefit if revised by a native English person (if available).

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 8333, 2007.

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