Atmos. Chem. Phys. Discuss., 5, S2128–S2131, 2005
www.atmos-chem-phys.org/acpd/5/S2128/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global distribution of Earth's surface shortwave radiation budget" *by* N. Hatzianastassiou et al.

Anonymous Referee #4

Received and published: 17 August 2005

Referee Comment on

Global distribution of Earth's surface shortwave radiation budget N. Hatzianastassiou, C. Matsoukas, A. Fotiadi, K. G. Pavlakis, E. Drakakis, D. Hatzidimitriou, I. Vardavas Atmos. Chem. Phys. Discuss., 5, 4545-4597, 2005

A. General Comments:

The main authors have worked on radiative flux calculations using ISCCP monthly-mean cloud climatology for several years. The difference between this work and their previous one is that they now use D2's 15-type cloud properties in place of previous C2's mean cloud properties with minor model improvements. Because of non-

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linear relationship between radiative fluxes and the input physical parameters of the model, such monthly-mean-input-based "monthly-mean fluxes" are not the same as the monthly-mean fluxes averaged from hourly/daily fluxes. Fortunately, ISCCP-D2 cloud properties are based on radiatively linear average method that would reduce such differences but, still, this point should be explicitly emphasized to raise readers' caution in appraising the results of this work. In addition, there are already at least two global, 3-hourly and decades-long fluxes data sets that include all the TOA and surface, SW and LW fluxes, namely GEWEX/SRB (Stackhouse et al., 2001) and ISCCP-FD (Zhang et al., 2004). The authors should at least give their reasons why they still do flux calculation based on monthly-mean input datasets. Since this is one of their follow-up work, the readers are more interested in the new improvements over their previous model and results, and the remaining problems, not just describe their new results and simple comparisons in details. In their validation, people would ask why the new and supposed-better results agree with GEBA better than BSRN since BSRN is much more accurate than GEBA? Besides, many places can be made more concise and accurate. The concept, "pixel" (e.g., "pixel-level input data") is used in somehow confusing/awkward way. In satellite-derived flux terminology, 'pixel' is mostly for actual satellite-observed resolution (e.g., about 30 km for ISCCP DX, or an actual approximately 5 X 5 km pixel). The authors actually calculate fluxes on 2.5 X 2.5 grid cell with a maximum of 15 cloud types, each of which is averaged from a cluster of DX pixels (not really "pixel-level input data" as appeared in your text, Line 21, P. 4562), but in the text, e.g., line 21, P. 4551, it is said the fluxes are calculated for "2.5-degree pixel" and so on. I suggest that "pixel" should be used only if it is really the original satellite-observed pixel, and in most places in the text, "grid cell" is more appropriate as it is widely used and accepted.

B. Specific Comments:

0. Abstract. Line 13: With 14 and 6.5 W/mˆ2 bias (etc.) to BSRN and GEBA, respectively, hardly to say it is "very good agreement", dropping "very" makes more scientific

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meaning. Line 16: “which is not possibleĚ” is not necessary to appear in the Abtract. Last line: “in contrast to Ě, found also in our study” - it’s not clear what really the authors want to say.

1. Introduction. This section should be shortened by at least 1/4.

2. Section 2 (Model description). With the references cited, this section may be shortened. Line 22: “compared successfullyĚ”, using plain language “compared” is sufficient (Do the authors think of any failed comparison?)

3. Section 3 (The Model data). 3.1, Clarify which water vapor data set is used for your results presented in this work. Line: 11, P. 4556. Not discussed in section 4, but 5.

4. Section 4 (Model results). A number of descriptions are well-known for a long time and, therefore, are not necessarily to repeat in your results report. Try to focus on reporting your new results/conclusions. Line 25, P. 4559. “scattering and absorption processes Ě between .. radiation and surface .. parameters..”, too awkward. Line 2, P. 4560. Do you really think that clouds determine DSR latitudinal gradient (even though it’s “secondary”)? P. 4561, line 6 and 11, you may drop both the ‘very’ as you also say in line 22.. P. 4562 that “Although Ě some similar Ě there are also some differences” - not consistent. P. 4562. You may give your direct comments on how good or bad your results are based on your validation/analysis, not just say (Line 20), “should be more reliable”. Regarding "trend", make some comments on its uncertainty, in other words, how much do you believe the trend values you have obtained? P. 4565 to P. 4566. Be precise about DSR anomalies for 1986/87 El Nino, the negative 6 W/mˆ2 appears around 89, not 86-87, etc. How about 1998 and 2001-2003 El Nino? P. 4567. You may make some comments on which is better or worse besides pure comparisons.

5. Section 5 (Sensitivity Ě). How/why do you choose the perturbation amounts for the sensitivity studies?

6. Section 7 (Summary ..). BSRN has much higher-quality control than GEBA, but

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your results show less good (or worse) in comparison with BSRN than GEBA. This is the most important question all readers would ask. You have to explain about it very clearly in appraising your results.

7. Table 2. Trying to reduce those unimportant or less representative results, and if possible, insert some more up-to-dated results.

8. Figures 3 an 4. Use better longitude label, not just 0, -50 and 50 degree, and also in figure 4, use year/month to replace the count of months for reader's convenience.

References

Stackhouse Jr., P.W., S. J. Cox, S.K. Gupta, M.Chiacchio, and J.C., Mikovitz (2001), The WCRP/GEWEX surface radiation budget project release 2: An assessment of surface fluxes at 1 degree resolution. International Radiation Sysposium, St._Petersburg, Russia, July 24_29, 2000. IRS 2000: Current Problems in Atmospheric Radiation, W.L. Smith and Y. Timofeyev (eds.), A. Deepak Publishing, 147.

Zhang, Y-C., W.B. Rossow, A.A. Lacis, V. Oinas and M.I. Mishchenko (2004), Calculation of radiative fluxes from the surface to top-of-atmosphere based on ISCCP and other global datasets: Refinements of the radiative transfer model and the input data, J. Geophys. Res., 109, D19105, doi:10.1029/2003JD004457.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4545, 2005.