Atmos. Chem. Phys. Discuss., 5, S2446–S2449, 2005
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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #3

Received and published: 7 September 2005

General Comments:

This paper describes an implementation of previously published methodology to derive surface shortwave radiative fluxes, using an improved version of satellite observations. Utilized was also more recent information on the vertical distribution of clouds and used were additional sources of information on water vapor, and some limited information on aerosols. Some general observations:

1. At present, several sources of information on long-term distributions of radiative fluxes based on the ISCCP D1 data are available. Therefore, it would have been of interest to explore the reasons behind the differences of available estimates.

This, for the following reasons. The ISCCP D2 data are monthly averaged; the ISCCP D1 are 3-hourly instantaneous. Due to nonlinearities involved, it is not obvious that the fluxes derived from the monthly averages will yield the same results as those obtained from the instantaneous computations. If the results are similar, the reason for that could have been explored by the authors by doing a month-by-month comparisons with other available data. What results will be obtained if the 17-year monthly mean ISCCP D data are used to compute directly the long term averages? Is the agreement due to error cancellations or is there a more intrinsic reason behind it?

2. Another unclear issue relates to the use of the vertical distribution of clouds. How were the clouds overlapped? Was it assumed that they completely overlap in each layer? If yes, how were they detected? How were the optical properties computed when they were averaged into three layers?

3. It is claimed that both NCEP-NCAR and ECMWF temperature and humidity data were used. Not clear how both data sets were utilized. Were the quantities averaged or was temperature taken from one source and humidity from another?

4. The fact that the evaluation results against GEBA is better than against BSRN requires some discussion since the BSRN data are believed to be of higher quality. Perhaps, it has to do with the geographical distribution of the stations. Since the satellite observations vary in frequency according to location, it is possible that one set of ground observations is located in a region that is better sampled by the satellites than the others. Needs to be addressed.

5. The paper is too long and some repetitive sections can be eliminated.

Specific Comments:

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1. On p. 4 in the middle, discussed are methods to derive the surface radiative fluxes based on correlations between TOA and surface fluxes. The references cited are a mix of methods and not only those that use such correlations. At the same time, relevant papers on such approach are not listed.

2. On page 5 (at the beginning) a list of papers is given that leads the reader to believe that they are all based on the ISCCP data. Additional clarification needs to be provided as to what was done in these studies or at least, present it in a way that the reader understands this point.

3. On p. 7 the authors explain that the radiative transfer model divides the SW μm radiation into two spectral bands, one for UV-visible and one for near-IR, this at λ of 0.85 μm. This selection requires some explanation since in general circulation models the split is at 0.7 μm and the drastic change in surface albedo as a function of wavelength is around 0.7 μm.

4. On p. 8 it is claimed that the difference at TOA against ERBE data is only 2.5 Wm^{-2}. Perhaps, it should be added that the ERBE data are believed to be accurate only within 6 Wm^{-2}.

5. On p. 10, eq. (5) shows the averaging of the reflectivities. Such averaging is questionable.

6. On p. 11 and 12, different sources of surface albedo used are described. Again, it is not clear what exactly was taken from each set.

7. Not clear how the GADS data were used since they provide only nominal values for two seasons for certain aerosol species.

8. Table 2 could be presented in a more concise manner and should be updated.

9. Figure 5 is not easy to follow. Also, there are no references to recent trend studies.

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