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

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A. General Comments

1. Indeed, as mentioned by the Referee, today there are various long-term distributions of radiative fluxes based on the ISCCP D1 data available. Therefore, it is very interesting to perform intercomparisons between them. Especially, note that recently the ISCCP-FD fluxes were produced (Zhang et al., 2004) including DSR fluxes. Ongoing work has started to compare our results with the ISCCP-FD ones; this is very interesting, because the ISCCP-FD fluxes have a 3-hour temporal resolution, while our results are computed by using monthly averaged ISCCP-D2 input data. This is essential given the existence of non-linearities, as mentioned by the Referee. These non-linearities refer not only to time, but also to space (for example downscaling from 1°x1° to 2.5°x2.5°

latitude-longitude). To face this concern, we have performed a thorough comparison between our DSR fluxes computed by using ISCCP-D2 at 2.5°x2.5° latitude-longitude resolution and on a monthly mean basis with corresponding fluxes computed with the same model by using input data at 1°x1° latitude-longitude resolution and on a daily mean basis. The results of this comparison, totaling a number of 1.142.330 matched data pairs, reveal a good agreement, with a correlation coefficient of 98.5%, and bias and RMS values equal to 9.2 and 13.7 W m-2. A relevant note was made in the text, page 4550, line 7. Nevertheless, it is certain that further and more detailed analyses and intercomparisons are required before definite conclusions can be taken on this subject.

2. Note that in our model the clouds are assumed to be non-overlapping (this was stated in page 4552, line 1). Of course, assuming overlapping clouds is essential for cloud vertical structure, especially in terms of cloud microphysics (associated with changes in cloud phase, determined by its height in atmosphere, and water vapour amount). However, the effect on DSR should be much less important than that the effect on downward surface longwave (LW) radiation (DLR), since SW radiative fluxes are critically dependent on cloud optical thickness, apart from cloud cover. Recent sensitivities (Zhang et al., 2004) have shown that changes in DSR fluxes induced by using overlapping instead of non-overlapping clouds, are smaller in magnitude than those for DLR fluxes by a factor of about 6. As far as it concerns the computation of mean optical properties for the three layer clouds (low-, mid-, and high-level ones) it has been according to the methodology described in the work by Hatzianastassiou and Vardavas (1999, Equations 3 to 6).

3. It is now clarified in the text, page 4556, line 11, that the results shown in this paper were computed by using both humidity and temperature data taken from the NCEP/NCAR Reanalysis. However, as noted in section 5, page 4570, using either NCEP/NCAR or ECMWF humidity and temperature data in our model, has generally little impact on the computed DSR fluxes. This has been also stated now in Summary,

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page 4574, line 3.

4. Certainly, the better agreement of our model DSR fluxes against GEBA than BSRN measurements is of interest and merits some discussion. This has been addressed in section 6, page 4573, line 9. We agree with the comment of the Referee, that the geographical distribution of the stations used from the two networks is critical. Indeed, 7 out of 22 BSRN stations (i.e. about 30%) whose data are used in our validation are located poleward of 50°N and S, against a comparatively small fractional number of GEBA stations located in these latitudes. This can explain the worse performance of our model against BSRN than GEBA, given that sub-polar and polar areas are much less well sampled by satellites, and hence the quality of the associated satellite model input data is inferior to those of lower-latitude areas. Of course, one has also to take into account the smaller number of matched data pairs for BSRN (939) than for GEBA (27858), due to the smaller temporal coverage of BSRN (1992-2000) than GEBA (1984-2000).

5. An effort was made to shorten the manuscript as much as possible by eliminating repetitive sections, as suggested by the Referee.

B. Specific Comments

1. In the middle of page 4548, lines 9 to 19, we refer to the possibility of deriving surface solar radiation from TOA SW fluxes. This is just one method, the other being the use of physical deterministic models along with input data for surface and atmospheric parameters. Different specific techniques can be used when applying the first method, and some of them are used in the works cited in this paper. Certainly, there are other relevant papers, but we only give just some examples here, therefore we noted this in page 4548, line 11.

2. Indeed, on page 4549, at the beginning, the text was misleading, giving the impression that all studies listed in lines 3-8 were based on ISCCP data. In fact, we meant that these studies refer to SRB, but not necessarily using ISCCP data. Thus, the beginning of the second sentence of page 4549, line 2, was rewritten to avoid confusion,

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as suggested by the Referee.

3. The selection of the wavelength limit of 0.85μm for splitting the SW interval into two spectral bands is due to the structure of the much more detailed spectral radiative convective model (Vardavas and Carver, 1984) from which the present simplified version has resulted. There is no other reason for this. However, we agree with the Referee's comment that there is a drastic change in surface albedo for various surface types, so such a modification will be considered in the future in our detailed spectral model.

4. On page 4551, line 24, it has been mentioned that the ERBE TOA fluxes have an uncertainty of about 6 W m-2, as indicated by the Referee.

5. On page 4553, eq. (5) provides the way we computed the mean surface reflectivity for a grid-cell of 2.5°x2.5° latitude-longitude. Such a computation is not an easy task, and certainly, the way by which we solve the problem is not ideal. Nevertheless, this is the best we can do, and the situation can be improved when working at a higher spatial resolution (e.g. 1°x1° latitude-longitude).

6. In our modeling work, we have also used land surface albedo from the ISLSCP climatology (Sellers et al., 1995), as an alternative. However, in this paper we only present the DSR fluxes computed with the ERBE derived data (page 4554, line 18); therefore we eliminated the part referring to ISLSCP (page 4554, lines 20-26), to avoid any confusion (as suggested by the Referee).

7. It is true that GADS only provides aerosol optical properties for two seasons (summer and winter). This certainly induces uncertainty related with the seasonal variation of the effect of aerosols on DSR. Nevertheless, this effect is rather small compared to the effect of other physical parameters (as shown by the model sensitivity tests, Table 3, page 4587), though not negligible, especially at the grid-cell level. We have chosen to work with GADS, as in a first step, since it has many other advantages. For example, it combines natural and anthropogenic aerosols, while it gives spectral aerosol optical

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properties at many wavelengths, instead of one wavelength provided by climatological satellite data for our study period. The most important, however, is that it not only gives aerosol optical thickness (as do the satellite data), but also single scattering albedo, and asymmetry parameter, which are necessary for our model computations. The situation will be alleviated in the future, with the new generation instruments, such as MODIS, MISR etc. The limitations of GADS were reported in page 4559, line 4.

8. Table 2 has been shortened and updated.

9. As far as it concerns Figure 5, unfortunately the variations of the global mean value cannot be seen more clearly, because the y-axis (DSR) scale is large enough. This is due to the fact that Fig.5 also includes the time-series of the hemispherical mean DSR fluxes. However, references to recent trend studies were reported in the text, page 4564, line 6, as indicated by the Referee.

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