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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long-term global distribution of earth's shortwave radiation budget at the top of atmosphere" *by* N. Hatzianastassiou et al.

Anonymous Referee #1

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I. General Comments

This paper addresses a fundamental piece of the Earth's energy balance using calculations based on the most reliable empirical inputs available (cloud albedos, etc.). Careful attention is paid to benchmarking computed fluxes against satellite measurements from the ERBE sensors. The analyses are very thorough and clearly presented.

Some of the numerical results, such as the latitude dependence of various components of the shortwave (SW) radiation budget are "well known" in the sense that the solar zenith angle is a determining factor. Still, it is of value to have specific numbers that include all of the geophysical processes at work.

The most interesting conclusion concerns the apparent downward trend in planetary albedo over the time frame from 1984 through 1997 and its apparent origin in the behavior of low-level clouds over the latitude band 20 degrees south to 40 degrees north.

Aside from its intrinsic scientific interest, the paper provides an excellent illustration of the types of information that must be combined to investigate the budget of SW radiation and the sensitivity of the results to these inputs.

II. Specific Comments

1. The conclusions rest on the validity of the radiative transfer model as well as the accuracy and completeness of the datasets used as inputs to the calculations. The paper does not present details of the model's formulation, although these have been described in previous refereed publications. The reader can safely assume that the methodology is valid. Still, this reviewer has some uneasiness with dividing the entire SW spectrum into only two intervals. In the UV-visible the very large contribution of clouds to the scattering optical depth is essentially independent of wavelength, so a single wavelength interval is likely appropriate here. But in the solar infrared one needs a rigorous method for treating the wavelength-dependent absorption bands of water vapor. These issues have surely been dealt with in previous publications, but a few sentences added to the current manuscript to describe the model formulation would be useful.

2. Section 3.5, first paragraph: A comment on the "modified" two-stream approximation would be useful. Does the modification include a delta-Eddington type of approximation to handle a strong forward scattering peak in the phase function for scattering by aerosols?

3. Section 3.5: It is unfortunate that the aerosol properties are from a technical report that is not as readily available as a journal publication. A comment on the range of optical depth and single scattering albedos used would be useful.

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4. Figure 4 and associated text: Is the "bias" of 3.66 watts per square meter simple the y-intercept where the ERBE signal is zero?

5. Section 4.3, first paragraph: The description of Figure 7b in the text does not seem consistent with what is shown in that figure. The two largest maxima appear at the South Pole and at 45 degrees North and South. The maximum at the equator, mentioned in the text, is only the fourth largest peak. The local minimum at 60 to 70 degrees South is still much larger than those at other latitudes. I believe the paragraph that describes Figure 7b needs to be modified.

6. Values in Table 6: Do the accuracies of the data and calculations merit stating results to two decimal places?

III. Technical Corrections

1. Introduction: Suggest change in wording to "...where the model results can be used as first estimates, even though their accuracy is limited."

2. Introduction: Suggest change in wording to: "A detailed summary of all relevant parameters is given,"

3. Section 3.5: The d'Almeida paper is not in the list of references.

4. Section 4.1, paragraph that begins "The mean monthly...", line 18: The word "underneath" is redundant and can be dropped.

5. Section 4.2, final paragraph: The first sentence, "Our analysis...", needs to be rewritten.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2671, 2004.