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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ten-year global distribution of downwelling longwave radiation" *by* K. G. Pavlakis, et al.

Anonymous Referee #1

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General comment:

In this paper the authors determine the global distribution of downwelling longwave fluxes (DLF) on a monthly basis for the 10 year period 1984-1993, using a radiative transfer code with atmospheric input from reanalyses (profiles) and satellite data (clouds). I enjoyed reading this interesting paper, which gives substantial new insight into the accuracy of fluxes calculated based on the above methodology. The authors made a big effort to test the sensitivity of their results to various assumptions on the atmospheric input data. Particularly I find the comparison of the 3 reanalyses intriguing, which I haven't seen in this form anywhere else. The results are somewhat disturbing, as they show how largely the results can regionally be affected by the choice of the reanalysis. This points to the necessity to obtain more reliable estimates from reanalyses. Probably a step foreward has already achieved with the new ERA40 reanalysis,

which the authors may consider to include in the future.

I recommend the paper for publication. My only concerns are outlined below.

Specific comments:

1. I wonder whether some of the deviations caused by the different reanalyses may also be due to the fact that the authors had to interpolate the reanalyes from the original grids onto their 2.5 degree grid. Since the reanalyses have different resolutions and therefore also different topographies I believe it is quite challenging to interpolate the reanaylses onto a new topography, and still appropriately representing the lowermost levels, which are the crucial ones for the DLF fluxes. This is particularly the case over rough topography, and in these areas the effects of the different reanalyses are shown to be very strong. The difference pattern in Figures 2 and 5 indeed reveal a very strong topography signal. So part of the differnces in areas with rough orography may not entirely due to the reanalyses themself, but also to the way they are interpolated onto the new grid, since the lowermost reanalyses levels of are certainly very much influenced by their respective original topography. The authors may add a comment on this.

2. Of course it would be desirable to have also an absolute estimate for the quality of the different reanalyses. An assessment of the quality of the reanalyses with independent observational data would be particularly interesting in areas where the largest differences are found (eg with synop stations as "ground truth"). This would allow to get a handle on which reanalysis should be used for this type of calclulation, and, in future studies, allow to judge whether the new reanalyses emerging now at ECMWF and NCEP are indeed superior to the ones presented in this paper for this type of calculation. Such an analysis is, however, almost certainly beyond the scope of this paper, but might be interesting for future studies. Already existing studies on this issue might be cited where appropriate.

3. The paper uses monthly averaged input data throughout for the calculation of the

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fluxes. Except for the satellite data this has not been clearly stated in the paper and should be added. It is also to the advantage of the authors, to distinguish the present publication clearly from potential future publications, as I know that they are planning to go to higher temporal resolution with their calculations. If they have already some results (e.g. one January and one July with monthly averaged profiles versus 6h profiles), this could be used to estimate, to what extent the calculations with averaged monthly mean data are affected by potential nonlinear effects. An estimate of this effect would be very helpful as an additional sensitivity study in chapter 3.

4. Validation chapter:

The bias structure of DLF noted on p. 16 with underestimation of low fluxes and overestimation of high fluxes is in line with with previous assessments of DLF calculated fluxes in various GCMs and Reanalysis in Wild et al. 2001, JClim. This could be mentionned. P. 17: As I recall from discussions with the authors, the biases are also caused by the clear sky biases of the simple radiation scheme, confirming again results found with other simple radiation codes used in GCMs and Reanalyses as reported in Wild et al. 2001. This discussion of the problem of simple radiation codes should be added.

Technical comments:

1. Adding of zero lines (as in Fig 7b) would be very helpful to easier infer the biases in Figs. 2,6,8

2. expression "lower 100mb" in figure captions and text, shouldn't it be "lowest 100mb"?

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5099, 2003.