

Interactive comment on “Analysis of the decrease in the tropical mean outgoing shortwave radiation at the top of atmosphere for the period 1984–2000” by A. Fotiadi et al.

Anonymous Referee #1

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Review of "Analysis of the decrease in the tropical mean outgoing shortwave radiation at the top of the atmosphere for the period 1984-2000" by Fotiadi, Hatzianastassiou et al.

GENERAL COMMENTS _____

Variations in reflected/outgoing shortwave radiation (OSR) at the top of the atmosphere are determined using ISCCP cloud data and additional atmospheric variables from reanalyses and other sources as input to a radiative transfer scheme. Decreasing trends over the period 1984-2000 are found to be broadly consistent with observations.

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The trends are found to relate to reductions in cloud amount, primarily at low-altitudes. Analysis of the variability is conducted globally for zonal averages.

Understanding cloud variability and feedback are of great importance to improving the accuracy of climate predictions. This present work adds some new information to this subject, in particular the recent discussion concerning apparent changes in cloud radiative effect from the 1980s to the 1990s presented in Wielicki et al. 2002. Thus the work is of importance and should be published.

There are several issues with regard to the present paper which need to be addressed as outlined below. These include repetition of previous work presented in Hatzianastassiou et al. 2004 (H04) and also issues with regard to the input data used in the analysis, primarily the ISCCP low cloud. Much of the analysis and some of the results of the present study were dealt with in H04. Therefore the present study can be significantly shortened by removing repetition of H04. How does this paper extend the results of H04? The answer to this question should be clearly stated in the Introduction.

SPECIFIC COMMENTS —————

1) The Abstract is too long and repeats H04. Suggest removing lines 8 to 13 ("Atmospheric....also used") of p.456. Also, either quote the decadal trend OR the 17-year change, not both.

2) Section 2 repeats much of H04; much of this section can be removed by referencing H04. I recommend keeping the 1st sentence of Section 2 and then say that ISCCP cloud, reanalysis atmospheric profiles, TOVS ozone and GADS aerosol were used as input to the radiation scheme, as detailed by H04. The rest of section 2 should be removed unless new information is being presented.

3) The results section discuss the variability of OSR due to ENSO and volcanic eruptions (lines 15-20, p.463). Much of this discussion repeats Wileicki et al.(2002). Mentioning that changes in OSR due to ENSO and Pinatubo with reference to Wielicki et

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al. would suffice and reduce the paper length.

4) ISCCP was designed to provide cloud climatologies not trends. Although Zhang et al. (2004) show consistency between ISCCP derived radiative fluxes and ERBS, there are still issues as to the reliability of ISCCP cloud trends. For example, see Campbell(2004) "View angle dependence of cloudiness and the trend in ISCCP cloudiness", 13th AMS conference on satellite meteorology (<http://ams.confex.com/ams/pdfpapers/79041.pdf>) This uncertainty should be stated, for example at the end of Section 4.

Low-cloud changes from ISCCP are inconsistent with changes derived from SAGE II (Wang et al.2002 GRL 29, 1397) and synoptic observations (Norris 1999, J.Clim 12, p.1864 and work submitted to JGR). Thus the main conclusion in the present paper, that reduced OSR is mainly due to changes in low cloud, is uncertain. This should be noted in the conclusion and abstract and also near line 10 of p.467.

5) The present paper appears at times to unfairly compare radiative transfer schemes with climate models. For example, p.465, lines 4-9 gives this impression and should be re-written. The radiative transfer calculations are primarily sensitive to the input variables, in particular clouds. Therefore it is not surprising that a reduction in cloud amount will lead to a computed reduction in OSR, although the agreement with ERBS is interesting.

The key question is, why are clouds apparently changing. The fact that climate models cannot reproduce the observed changes suggest that there are processes or forcings that are not resolved by climate models. Alternatively, the data is spurious. These points should be clearly presented in the discussion.

6) ISCCP derived cloud parameters from radiances. The present study derives radiative fluxes from the ISCCP cloud. There is therefore an element of circular reasoning in the present study. For example, changes in radiances that are not related to cloud may be identified incorrectly as a cloud effect by ISCCP. This spurious change in cloud

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may then give a change in radiation which is realistic but incorrectly interpreted as a cloud effect (e.g. aerosol changes due to Pinatubo). This point should be made.

7) The Lin et al. (2004) paper found that the ERBS trends did not relate to the IRIS effect. They also showed that the IRIS study was flawed due to inaccurate parameter specification. Therefore the reference to this effect is not helpful. I suggest removing lines 16-19, p.457 ("This is very... iris feedback (Fu et al. 2002)")

8) p.468, line 16. reduced precipitable water in NCEP was shown to be spurious compared with SSM/I data (e.g. Allan et al. 2004, JGR, 109,D18107).

9) p. 467, line2 2-4. The big increase in cloud and OSR during the 1998 El Nino for 0-10oS is interesting but not discussed as promised. Presumably this relates to the extension of convection across the Pacific compared to its usual position in the West Pacific and over the south Pacific convergence zone. This should be discussed briefly.

TECHNICAL CORRECTIONS _____

a) The title suggests only the tropics are considered. Figures 5 and 6 look at global data. Suggest removing "the tropical mean" from the title.

b) There are not enough paragraph breaks which reduces readability. Suggest starting new paragraphs at the following locations:: p.458, line 25 //Recently p.463, line 14 //The OSR p.465, line 23 //To further p.466, line 14 //Linear p.466, line 23 //Our model's p.467, line 4 //According to

c) p.459, line 5. The CLAUS data is based on ISCCP-B3 data not D2 as suggested. The CLAUS data should reference the following paper on p.459, line 5 and p.466, line 7: Hodges et al.(2000) J. Atmos and Oceanic Tech., 17, p.1296.

d) p.457, line 24/25. Wielicki et al. 2002 did suggest that the radiation budget changes related to a reduction in tropical cloudiness but did not use ISCCP.

e) The first line of the Conclusions section on p.469 is rather long.

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