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Interactive Comment

Interactive comment on "Radiation budget estimates over Africa and surrounding oceans: inter-annual comparisons" *by* A. Ben Rehouma et al.

Anonymous Referee #3

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General Comments:

This paper examines the long-term variability of radiation budget over Africa and surrounding oceans using ERBE/ScaRaB/CERES scanner data, ERBS Nonscanner data, and ISCCP-FD data. I like to commend the authors for taking up such a difficult task for analyzing data from these diverse sources. However, I am sad to say that I am disappointed in quality of the analysis. Specifically, this paper tends to be too qualitative in nature and lacks detailed quantitative analysis in the form of statistical error analysis to backup much of their conclusions. Furthermore, some of the underlying principles for the analyses are somewhat mis-guided (see details in the section below). Due to

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these deficiencies, it will be very difficult to convince the scientific readers about the overreaching conclusions of this paper.

In the following section, I have outlined some suggestions for improving the overall scientific quality of this paper.

Specific Comments:

1) While the ERBE/ScaRaB/CERES scanner data are self-calibrated in space for stability, they are not cross-calibrated with each other in space since there was no overlapped period in which a pair of these scanner instruments was operating at the same time. So there can be large absolute calibration (or precision) differences between these instruments, which in term, can translated into artificial jump or shift in the time series. A good example of this problem can be seem in the AVHRR LW time series as it changed satellite instruments. The problem is even worse for SW reflected flux. So one cannot just simply join these three different scanner datasets together for long-term analysis without first removing these instruments artifacts. Long term trends deduced from such analysis will be inherently fault.

Effort for putting these scanner instruments on the same radiometric scale is currently on-going. Until this is done, I would suggest that the authors de-emphasizes the scanner analysis in the paper and concentrates on the Nonscanner and ISCCP-FD analysis for the overlapping period between 1985 and 1999 instead. The nonscanner data is very stable with SW and LW stability uncertainty on the order of 0.3 to 0.4 Wm**-2 over the 15-year period for tropical mean. While the ISCCP global mean radiance stability uncertainty is quoted as 3% in the visible and 1% in the infrared, it's LW and SW flux stability uncertainty is comparable to the nonscanner data for tropical mean based on recent published results.

2) The first principle reason that the authors said they want to do regional analysis, instead of looking at global analysis, is that the long term change signal will be larger and easier to detect. This statement is incorrect. While the regional signal may be larger,

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it may not be statistically significant because regional data are inherently more uncertain than global data. Over the globe, random errors (due to time, space, and angular sampling uncertainty) of opposite sign tend to cancel each other to produce a global mean with small uncertainty. At regional scale, this is no longer true. Uncertainty of the radiation budget data tends to increase as the spatial scale of interests decreases (i.e., from global scale to regional scale). Therefore, the regional analysis does not necessary increase our confidence of the detected trend because of the much larger uncertainty associated with the regional data. I would suggest modifying the texts in the paper accordingly.

3) The second principle reason that the authors said they want to do regional analysis is that the regional anomalies are relative values and independent of calibration drifts, which mainly impact all the data in the same way. This statement is not quite correct. Anomalies fields, even they are relative values, are not immune from calibration drifts. On the contrary, anomalies tend to bring out the calibration drift more clearly. This is demonstrated again in the long-term time series of the AVHRR Pathfinder Earth Radiation Budget data, which have both drift and absolute calibration problems. Again, I would suggest modifying the texts in the paper accordingly.

4) The authors will need to perform more vigorous error analysis to convince the readers that the results are robust. For example, 2-sigma uncertainty of the detected trend should be given in the trend analysis in addition to the trend itself. If 2-sigma uncertainty of the trend is larger than the trend itself, the detected trend will then be statistically insignificant; meaning it is not statistically different than a trend of zero. One-sigma is not enough to separate noise from signal.

5) I would suggest the authors extending or contracting the regions to between 50 S and 50 N or 40 S to 40 N to match the 10-degree grid of the Nonscanner data. The current analysis regions of 45 S to 45 N leave a possibility of aliasing issue due to data mismatch between Nonscanner data and the other dataset used in this study.

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6) In section 3.1, it would be useful to give a physical meaning for standard deviations. For example, is this related to the interannual variability?

7) I would suggest adding more analysis of Net radiation in section 3.1 to 3.4. Since Net radiation is the key to understand the energetic of the Africa and surrounding oceans regions.

8) Table 2 in section 3.2 is very user-unfriendly. The authors will need to remind the reader that they cannot compare the values from different pairs of data because the data periods are different. The authors will need to remove texts in the paper that compares these values. These comparisons are fault due to the different periods and climatology used in each pair of analysis.

9) In section 3.3, the comparisons of trends from different datasets are fault due to (1) discontinuity in the various datasets can influence the climatological mean used to derive the anomalies, (2) the absolute calibration differences between the three set of scanner datasets, which can create fault trend in the scanner analysis, and (3) differences in ending period can create artificial differences in detected trends since trend analysis is very sensitive to starting and ending point. I would suggest redoing this section using the same common data period (same starting and ending point) and minimizing missing data between the datasets. In addition, it will be useful to run the 2-sigma test on the trends to see if they are statistically significant.

10) In section 3.4, it is not clear what is the meaning of regression coefficient. Is this the offset A in the liner regression equation (Y = A + B * X)?

11) The trends quoted in section 3.4 have the same problem as outlined in item (9). In additions, they will need to be put in content with the 2-sigma uncertainty of the trends to see if they are statistically significant.

The 20-year trend quoted for the Nonscanner is an extrapolation since there is only 15 years of nonscanner data. This type of extrapolated analysis can be very misleading

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since trend analysis is again very sensitive to the starting and ending points of the data period. This needs to be noted in the paper.

The scanner trend quoted in this section is again fault due to absolute calibration issue outlined earlier.

12) The 4 Wm**-2 accuracy for the Net flux trend quoted in section 4 should read 4 Wm**-2 uncertainty. Is this one-sigma or 2-sigma value? This type of statistical error analysis is needed throughout the paper to determine the significance of the detected trends. However, this is currently missing and leave the conclusions and findings of this paper open to questions and uncertainties.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 13139, 2006.

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