

Review of “A net decrease in the Earth’s cloud plus aerosol reflectivity during the past 33 yr (1979–2011) and increased solar heating at the surface”, by Herman et al., *Atmos. Chem. Phys. Discuss.*, 12, 31991–32038, 2012

I have five major concerns with this study:

1) This study claims that the downwelling broadband solar radiation at the surface could be estimated from satellite TOA LER measurements. Based on reading the abstract multiple times I am unsure whether the authors claim only the TOA reflectance is changing due to clouds and aerosols at one wavelength, or whether they imply that LER can actually be used as a surrogate for broadband downwelling surface flux. Please clarify. If the regional trends noted in the paper can be used to estimate the change in surface downwelling at the surface, I have the following concern.

Actual ground based measurements are sparse in both spatially and temporally, limited mainly over land, with various degrees of quality. The quality of surface fluxes have increased dramatically over time, for example, the BSRN (Baseline Surface Radiation Network) frequently has meetings to standardize surface fluxes and has set up guidelines for best practices. Global surface flux datasets from GCMs, assimilated (ECMWF) and observed (ISCCP-FD) compute the surface fluxes from cloud properties and may be biased spatially and temporally depending on the quality of the input. These global datasets validate their surface fluxes with ground observations and contribute to projects that evaluate the quality of long-term flux and cloud climate datasets such as GEWEX (<http://www.gewex.org/>). It seems conspicuous that the LER fluxes are not compared to any existing ground based surface measurements, which standard practice with global surface flux datasets. Due to the large size of the SBUV/2 footprint, it may be difficult to validate the SW down with a surface site not representative of the larger surrounding domain, such as coastal or terrain sites. However, the surface community has introduced ground sites over uniform regions. For example, Long et al 2009 has derived large domain 12-year fluxes over the US using ARM-SGP. An important validation is make sure that the ground based surface flux natural variability is consistent with the SBUV/2 over the same time period. If this were performed it would bring about much more confidence that the LER can be used to derive the surface downwelling fluxes.

Long, C. N., E. G. Dutton, J. A. Augustine, W. Wiscombe, M. Wild, S. A. McFarlane, and C. J. Flynn (2009), Significant decadal brightening of downwelling shortwave in the continental United States, *J. Geophys. Res.*, 114, D00D06, doi:10.1029/2008JD011263.

Augustine, J. A., and E. G. Dutton (2013), Variability of the surface radiation budget over the United States from 1996 through 2011 from high-quality measurements, *J. Geophys. Res. Atmos.*, 118, 43–53, doi:10.1029/2012JD018551.

If the authors of this study were confident of their results, would it not be appropriate for the SUB/2 team to archive the data of this study and release it to the community? The ISCCP, AVHRR-Patmos, CERES, ECMWF, assimilated, GCM, and ground based surface site datasets are all freely available to public. If two or more independent methods provide the same natural variability and trends will greatly enhance the claims of this study.

2) This study claims that LER can provide an alternative estimation of the change in the broadband SW downwelling flux due to the cloud plus aerosol cover. For wavelengths less than $0.5\mu\text{m}$ the ground reflectivity is less than 10% LER, making these wavelengths advantageous to monitor cloud cover change. Again the abstract is not clear whether the increase in the SW absorbed at the surface is broadband or for one wavelength.

If this is the case then what is not evident, is how LER can monitor over time the changes in cloud SW fluxes in other frequencies such as the in the near IR in the water vapor absorption bands, where the water vapor associated with increases or decreases in clouds may impact the total SW downwelling flux. Simply applying a scaling factor based on some broad assumptions presented on page 32009 and 32010 using Trenberth's energy balance diagram to the LER and assuming that the surface reflectivity has remained constant over time and atmospheric absorption has not changed are bold assumptions. This simply implies that the change in TOA reflectance is only due to clouds and aerosol variation, not surface reflectance changes. If this statement is actually made, then it can be verified by comparing LER with the well calibrated and diurnally corrected CERES broadband SW TOA reflected dataset over the last ten years. The regional trends should be consistent.

3) Several regional LER trends are given in the last part of the abstract. There is no distinction made in the paper if these are trends by simply placing a trend over the natural variability of clouds and aerosols over the last 30 years. Since the cloud and aerosol natural variability is dominated by the ENSO cycle as claimed by the authors, simply placing a trend line over the natural variability is dangerous and highly dependent on beginning and end points of the ENSO events, of which the magnitude has been rather small over the last decade. Is putting a trend over the natural variability meaningful? However if the point of the paper is to imply that these trends are outside of the normal annual, AO, ENSO, MJO, NAO, oscillations and volcanic eruptions, then an uncertainty factor must be given in order to give confidence to these trends and is absent in this study. These papers provide the confidence level given a trend and the natural variability.

Weatherhead, E. C., G. C. Reinsel, G. C. Tiao, X.-L. Meng, D. Choi, W.-K. Cheang, T. Keller, J. DeLuisi, D. J. Wuebbles, J. B. Kerr, A. J. Miller, S. J. Oltmans, J. E. Frederick, 1998: Factors affecting the detection of trends: Statistical considerations and applications to environmental data, *Journal of Geophysical Research*, **103**, 17149-17161.

Leroy, S. S., J. G. Anderson, and G. Ohring, 2008: Climate Signal Detection Times and Constraints on Climate Benchmark Accuracy Requirements. *Journal of Climate*, **21**, 841-846.

Page 32013 does mention that the ENSO cycle is removed from the mid Pacific Ocean in 7.1. I do not know what the intent of this paper is with revealing these LER trends. Careful studies must be performed to verify these trends above natural variability and the physics behind these trends and corroborating these with other independent datasets.

The LER trends are Figures 14 and especially 16 are not convincing as statistically significant, when overlaid with daily? measurements. Fig 9 does make an attempt with an annual low pass filter

4) Zonal diurnal corrections are performed to remove the diurnal cycle regionally. Figure 11. The LER trends over 30 years are highly correlated with regions with large diurnal cycles, such as off of the Peruvian and California coast (positive trends) with clouds in the morning, and land afternoon convective regions such as South America and the USA (negative trends). Page 32004 line 25. The Labow et al. paper describes a 5° zonal land and ocean diurnal corrections. Applying a mean land/ocean zonal correction does not remove all the diurnal variation in regions with large (maritime stratus) amplitude diurnal variations, based on other zonal regions with weak diurnal cycles. How can the authors be sure that the maritime stratus regions, in Figure 11 top panel, have been properly removed with a zonal ocean correction, where most of the ocean does not have a diurnal cycle?

As mentioned in the paper that the ENSO activity is small in the 2000-2010 decade compared to previous decades and Figure 9 does illustrate that. The 2000-2010 decade is also the time period that multiple LER datasets were used in the dataset. If the only one LER dataset were available would the same results have been obtained in figure 7, especially the degrading orbits of NOAA-14 as it cycle through the terminator? This would be an excellent validation of the diurnal correction. It seems that all the regional trends during this time are flat after 2000. Most of the trends are really the difference of the first decade compared with the last decade. The first decade of results relies solely on one dataset from Nimbus-7, indicating that the calibration of the instrument is most crucial in all of regional trends.

5) The authors claim that the reflectivity of Greenland is changing over time. The summit of Greenland has been use by many to calibrate imager visible channels, similar to Antarctica. The calibration of AVHRR and ATSR over Greenland and Antarctica were consistent with the studies listed below. The summit and Antarctica are both at 3-km in elevation, where very few clouds are found. Are clouds brighter than snow surfaces, since the authors state that cloud cover changes could be the cause? I can only imagine maybe the snow surface is changing due to water ponding or soot. In order to convince the reader I would like to see an added figure showing the Antarctica LER variability similar to Fig 15. The noise of which should be much smaller than Greenland and be very flat.

S. J. MASONIS and S. G. WARREN, Gain of the AVHRR visible channel as tracked using bidirectional reflectance of Antarctic and Greenland snow, *int. j. remote sensing*, 2001, vol. 22, no. 8, 1495–1520

WILLIAM R. TAHNK AND JAMES A. COAKLEY JR., Improved Calibration Coefficients for *NOAA-12* and *NOAA-15* AVHRR Visible and Near-IR Channels, *JTECH*, 2002, pg 1826

Dave L. Smith, Chris T. Mutlow, and C. R. Nagaraja Rao, Calibration monitoring of the visible and near-infrared channels of the Along-Track Scanning Radiometer-2 by use of stable terrestrial sites, January 2002 Vol. 41, No. 3 *APPLIED OPTICS*

Minor concerns

- The intent of this paper is not well outlined in the abstract. Is it to scale the LER to derive an actual SW downwelling flux. Then the following regional trends are still in RU. Are the regional trends that are given in the abstract the regions that are responsible for the overall global change?

- Page 31992 line 7, “much higher reflectivity of clouds plus aerosols” I believe not all aerosols are weighted equally with LER measurements. Rayleigh scattering probably obscures the aerosols close to the surface, where as the effects of stratospheric aerosols are properly monitored.

- Page 31994, line17. “Two other long-term cloud data sets exist, AVHRR and HIRS, which have diurnal cloud variation from drifting orbits.” The AVHRR and HIRS are on the same NOAA platforms as SBUV/2. There have been many algorithms developed to remove the diurnal variations of cloud properties and radiances due to the sampling time of the degrading NOAA orbits. These datasets have the advantage of smaller footprints than the SBUV/2 and therefore can resolve the diurnal cycle over small regions, unlike the zonal ocean/land diurnal adjustments used in this paper. Here are some references.

Foster, M., and A. Heidinger, 2012: PATMOS-x: Results from a Diurnally-Corrected Thirty-Year Satellite Cloud Climatology. *J. Climate*. doi:10.1175/JCLI-D-11-00666.1, in press.

Lee, Hai-Tien, Arnold Gruber, Robert G. Ellingson, Istvan Laszlo, 2007: Development of the HIRS Outgoing Longwave Radiation Climate Dataset. *J. Atmos. Oceanic Technol.*, **24**, 2029–2047.

MacKenzie, Ian A., Simon F. B. Tett, Anders V. Lindfors, 2012: Climate Model–Simulated Diurnal Cycles in HIRS Clear-Sky Brightness Temperatures. *J. Climate*, **25**, 5845–5863

- Figure 2 Only LER should be plotted; the MODIS clear-sky map should be removed. The reader would want to see the relative magnitude of LER over various cloud types. To make a more convincing argument that the 340nm channel is a good substitute for cloud variations.

- Is the LER magnitude wavelength dependent? The 412nm wavelength SeaWiFS LER has some very different trends over Peruvian stratus regions and over the Amazon, than that of 331nm SBUV/2 LER in Fig. 13 in Herman et al. 2009, which resembles the 340nm SUB/2 LER regional trend figure 11 in this study. Is this due to a difference in the wavelength being used or is it improper calibration of the data?

Herman, J. R., G. Labow, N. C. Hsu, and D. Larko (2009), Changes in cloud and aerosol cover (1980–2006) from reflectivity time series using SeaWiFS, N7-TOMS, EP-TOMS, SBUV-2, and OMI radiance data, *J. Geophys. Res.*, **114**, D01201, doi:10.1029/2007JD009508.

- How can the authors be sure that the zonal trends from 30 to 60° North latitude are not a result of the variations of the winter snow time periods in figure 8? Could the figure be replaced with a land and ocean trends separated?