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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Insights into the diurnal cycle of global Earth outgoing radiation using a numerical weather prediction model" *by* Jake J. Gristey et al.

Anonymous Referee #2

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General Comments:

In this paper the authors investigate the diurnal cycle of Earth's outgoing radiation(EOR), splitting its components into outgoing longwave radiation (OLR) and reflected shortwave radiation (RSR). Their primary focus is on analyzing the output from the Met Office NWP model for the month of September 2010 and GEBA output for July 2006 using Principal component analysis (PCA). For each EOR component they investigate the cause of the first two EOFs. In the case of OLR they claim that the first EOF, which is the dominant signal, is largely related to changes in surface/atmospheric temperature, while the second is related to the diurnal cycle of deep convection. In the

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Discussion paper

case of RSR, the first EOF is again dominant and is controlled by the atmospheric path length, while the second is related to the timing of deep and shallow convection.

I found the paper to be well written and the analysis clearly presented. I think that the authors have achieved their aim of showing the dominant signals that influence the diurnal cycle of EOR. It is also interesting to see the reasonably good agreement between the NWP and observations. To this end I have no issue with recommending the paper for publication following minor revisions. I do think though the paper would benefit from a more detailed analysis of the surface versus atmospheric contribution to the first OLR EOF. It feels like the detailed analysis that went into understanding the radiative transfer leading to the RSR signal has not been replicated in the case of OLR. I detail my concerns below.

Specific comments:

Lines 88: I think a few more sentences discussing the impact of fixed sea surface temperatures is needed here. I know it is discussed later on, but the fact there is no diurnal SST cycle is quite a major caveat.

Line 139: I understand that it may not be possible to analyse the satellite data at the equinox, but it would seem that it would at least be possible to analyse the the NWP output for the same month as the satellite. This would lead to a cleaner comparison. If this is not possible, then perhaps explain in more detail why this is the case.

In general, one weaknesses of the paper is the fact the authors only look at one month of one year. Hence the need for more clarity about why just one month is looked at and some text expressing the limitations this imposes would be useful. What would the authors expect different in their results if they did same analysis with 30 years of monthly data?

Lines 230:236. I think that the authors have to dig a bit deeper here. It should be relatively straightforward to use a RT code to distinguish how much of this OLR signal is due to the surface compared to the atmosphere. This will help improve our understanding of whether the surface, boundary layer or lower/mid troposphere diurnal cycle of temperature is most important for understanding the diurnal cycle of OLR. The 10% number from Costa and Shine used here may also be misleading, as locally these numbers can be bigger and I suspect are bigger over the dry land regions that have the strongest diurnal signal shown in figure 1. Furthermore, even if the surface only accounts for 10% of the absolute OLR signal, a 25 K swing in surface temperature could still cause a big swing in OLR. Consider a change from 300 K to 325 K = 0.1*5.67E-8(325ˆ4 -300ˆ4) =17.3 Wm-2. Another issue with the claim that atmospheric temperature is important is that most of the emission from the lower atmosphere to space is dominated by emission from the H2O self continuum. However, the optical depth of the continuum scales with the square of vapour pressure and may be quite weak in dry hot regions. This again makes me think that the atmospheric contribution from the dominate regions highlighted in fig 1 might be smaller than that of the surface. Hence, I am not convinced by the term ‘large fraction’ used in the statement on line 235. Given that the paper aims to provide insight into the mechanism behind the EOR signal means that this ‘fraction’ should be quantified. I therefore encourage the authors to perform a few simple RT runs, even using idealized atmospheres, so to make the attribution of the OLR signal clearer.

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Line 281: I generally like that the analysis (i.e. Fig 4) that the authors have performed on investigating the causes behind the ‘U’ shape. However, the impact of aerosol and mean cloudiness could be dealt with a bit better. Here the authors say they use only one aerosol case; ‘rural aerosol’ to see how aerosol loading could change the relationship between SZA and TOA albedo. Would it not be more useful to look at the extremes between say a highly scattering aerosol environment(e.g. high SO4 or sea salt) versus a highly absorbing aerosol environment (e.g black carbon). Just using one simple aerosol case does not really provide much insight into how much aerosol can alter the diurnal cycle of RSR. Also I wonder about impact of the mean state of cloudiness (as opposed to the diurnal cycle). I suspect that this ‘U’ shape would be stronger for cloudy versus clear regions (as shown in fig 5), but may get weaker as the mean cloudiness of a region goes up. That is because the amount of radiation scattered to space per unit optical depth decreases with increasing cloud optical depth.

Line 335: I would place more emphasis on this result in the abstract and conclusions. The fact that your technique of analyzing the diurnal cycle highlights some clear limitations of the NWP cloud fields is an important result.

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