

Interactive comment on “Spatial patterns in timing of the diurnal temperature cycle” by T. R. H. Holmes et al.

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General comments

The paper studies the structural difference in timing of the diurnal temperature cycle (DTC) over land resulting from choice of measuring device or model framework. On the positive side, the paper is in general well written, their contents are well presented, figures and table are in general clear, and the subject is of interest for HESSD readers. In the negative side, some of the conclusions look still a bit speculative and the reader may question whether a bit more of work could have resolved some of the open issues.

Specific comments

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P6020-L11. “In absolute terms” seems unclear, do you mean “averaged over a full year and over Africa and EU”?

P6020-L13. It would have been interesting to get an idea about an “average” land surface temperature (LST) difference related to this “averaged” DTC difference. It would certainly depend on land cover, but are we talking about 1, 5 K? My experience by looking at average annual cycles for different surface types is that ~20 min at max of the DTC result in very small differences in LST for most surfaces. Without denying the interest of this work, the reader may wonder if this is not the least of our problems when assimilating LST into models, given the biases we observe between different satellite LST products.

P6022-L2. Without questioning the arguments given in this paragraph, I’d say that it is a bit skewed towards model difficulties. I’d argue that other important difficulty for LST assimilation is that errors in the remotely sensed based estimates of LST are still poorly characterized (e.g. dependence on emissivity, viewing angle, atmosphere characterization).

P6030-L21. This is an interesting point that merits further comments. In principle, the Tka and the Tnwp are all-sky, while Tir is clear-sky. Is this a problem when comparing the reconstructed DTCs? Why do we want to have a DTC with a shape close to clear-sky circumstances?

P6031-L5. What about the effect of sampling for the Tir? While for Tka the effect of sampling is easier to characterize (two of the instrument do not change the overpass time so always sample at the same time), for Tir the sampling depends on cloudiness, which changes with location and time of the year. Would that affect the modeling of the DTC?

P6031-L23. I think the most striking feature of map 4a is not the latest peak of DTC over sand seas due to a larger penetration depth (expected), but the fact that tropical forest seem to peak at the same time (unexpected?). Perhaps this is worth commenting

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already here.

P6031-L28. An earlier phase for mountain regions is also observed for the Tir and the Tnwp (e.g. see the French Alps), it does not seem exclusive for Tka. The explanation for the Tka does not seem very convincing. Can the author elaborate a bit more regarding azimuth angles and an earlier phase?

P6032-L10. Interesting that the Tir and Tnwp predict the same delay in peak temperature over the tropical forest. Could this be an indication of MERRA rightly parameterizing the heat capacity of the forests (under the assumption that Tir is a good representation of the skin temperature modeled by MERRA)? In the following paragraph (L15) the authors seem to search for an explanation for the delayed peak of the DTC for Tir, is this implying that a higher heat capacity modeled by MERRA is a wrong assumption? The authors may need to elaborate more on this to make it clearer.

P6032-L16. We also see a delay in peak temperature over the boreal forests in Tir, with very different climate conditions compared with the tropical forests. It looks unlikely that cloudiness is responsible for both. Also, this is not discussed for the Tnwp, does it mean that MERRA will not be capturing this? Clouds in MERRA not properly represented over this region? Cloudiness not affecting the DTCs from MERRA?

P6032-L18. The mention of cloudiness makes me think that some analysis of seasonality may be useful (i.e. repeating the maps and/or transects not for the whole year, but for 4 month averages). We may be able to see some features related to the variation of the climate conditions in the patterns (e.g. degree of insolation), which would make the analysis presented more convincing.

P6032-L21. Figure 6 mentioned before Figure 5?

P6033-L4. For instance, the “reds” around 0-40E in Figure 5a seem to be placed to the right of the mountains over there, more than over the mountains (e.g. Kilimanjaro), so orography may not be all. In fact, at no point in the article it is discussed that the model

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to fit the DTC may not be perfect everywhere and may have the potential to introduce artifacts in the estimated peak of the DTC for specific regions and climate conditions. As I stated before, a study of the seasonal DTC may shed some light on some of the interpretations.

P6034-L12. This may be a naïve question to ask, but do we now what we expect in terms of heat capacity for a tropical forest? Or, in other words, why is MERRA parameterizing a higher heat capacity there?

P6035-L13. As in the abstract, “in absolute terms” does not really say much about the time/geographical conditions of the DTC figures given here.

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