

Interactive comment on “High-resolution satellite-based cloud-coupled estimates of total downwelling surface radiation for hydrologic modelling applications” by B. A. Forman and S. A. Margulis

Anonymous Referee #2

Received and published: 29 May 2009

This is generally a very well-written paper on a topic of significant importance and interest for HESS readership. My major criticism is that the current write-up does not provide a strong motivation for the analysis. A large number of land surface radiation estimation schemes already exist. . .what is the motivation for creating another one? The last paragraph of the introduction tries to establish the need for relatively simpler schemes with less reliance on radiative transfer modeling and/or atmospheric profile measurements for data assimilation applications. . .but I don't think they quite make

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their case. A couple of points for the authors to consider upon revision:

1) All other things being equal, simpler approaches should be given preferences, but given the long-list of satellite (Section 2) and model-based inputs (the Appendix) is this approach really significantly less complex than the derivation of existing products? Is there some fundamental issue with the availability of atmospheric profile measurements that makes them prohibitively difficult to acquire and/or process? Or, alternatively some fundamental difference which makes this approach easier to implement. I would guess the answer stems from the difference between a “bulk” method and a “radiative transfer” one based on an entire atmospheric profile. . .but more text clarifying this would greatly help.

2) Make of the motivation appears to stem from the potential application of this type of approach to enable ensemble-based data assimilation approaches. Presumably, the author’s are advocating that their model be implemented on-line by data assimilation practitioners to create background model ensemble (whose covariance structure represents the covariance impact on model state predictions of radiative forcing errors). For a land model assimilation problem, this type of implementation seems improbable, since the land model will not predict (or require an ancillary description of) the types of cloud and atmospheric variables needed to run the author’s model. Instead, it is far easier for land data assimilation practitioners to continue the common practice of taking an existing product (e.g. the NLDAS LW and SW fields), assuming a given error model for it, and randomly perturbing these fields according to this assumed model. Why would the implementation of this model in a land data assimilation context confer any advantage over this much simpler approach? If the author are thinking of a different data assimilation problem (e.g. assimilation into a boundary layer model?) they should make this clear. Regardless - given that it is invoked as the primary motivation for this particular approach (in both the abstract and the introduction) – the author need to provide more detail concerning the expected benefits of implementing their approach in a data assimilation context.

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Other notes. . . #3 below is particular important:

1) The single-day illustration in Figure 1 makes the diurnal interpolation scheme look like just a bias correction approach whereby – when, based on my understanding, it is more powerful than that. Could the author’s show multiple consecutive in which true temperature exhibits periods both above and below the climatological expectation. The illustration of a single day in Figure 1 does not quite capture the functionality of the approach.

2) The manuscript makes reference to both NLDAS LW and SW products but does not describe their origin. One or two more sentences of detail would help on page 3056. page 3060 also makes reference to “NLDAS longwave and shortwave” products. . . where does the NLDAS shortwave product come from and what is it’s relationship with SRB?

3) Comparison between SW and LW products from the author’s algorithm and existing products represents some of the most important parts of this analysis. There are a couple of spots where these comparisons could be improved.

a. Table 2. The LW appears to do worse than the NLDAS-LW product with regards to RMSE and correlation validation metrics. However, the manuscript does not discuss this point. What accounts for the relatively low correlation value and why should this not be taken as evidence the proposed scheme performs relatively worse than existing schemes for LW?

b. Section 6. First paragraph. Where in the results section are the “more physically realistic” results for the LW product during “cloud-sky conditions” presented? Is this a reference to the single day results shown in Figure 3? If so, this does not seem like adequate support for such a strong statement (i.e. how do we know this particular day is typical?).

c. Figure 9 is a really nice figure, but could be improved if comparable results where

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plotted for the NLDAS and SRB products for comparison. If the LW product is more physically realistic for cloudy conditions (relative to the NLDAS LW product) than presumably the line in Figure 9 would be steeper for the NLDAS LW product. Adding NDLAS LW and SRB results to this figure would provide the necessary support for some of the stronger statements in the conclusions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 3041, 2009.

HESD

6, C1000–C1003, 2009

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