<u>Responses to Reviewer 2:</u>

We appreciate the generally positive comments and constructive suggestions from the reviewer. Our detailed responses are given after each comment (*italics)* below.

1) *My main concern with the paper is the readability of the paper. In general the paper lacks a justification of the utilized methodologies (especially the parameter estimation, LOESS method etc) and complete description of these method. In terms of style, the paper reads a bit as a flood on information on equations and parameters, but a real interpretation of the results is missing. Overall as a reader I get too much a feeling that the whole paper provides a black box approach.*

Response: We have improved the overall readability of this paper in the following aspects:

a. The justification of utilized methodologies, including the parameter estimation, LOESS method and determination of OHM coefficients, has been added;
b. A discussion with more interpretations of the results has been added.

2) *Interpretation: The followed approach provides new values and uncertainties in the parameter values of the OHM model. However, the paper does not reach a level beyond these parameter values. I think the reader expects more interpretation on the various parameter values and how much it would change the surface energy balance as a whole by the new information at hand. Moreover, the bias and RMSE are still quite high for some of the presented sites. I miss an outlook on how the authors will further address this, or any hypothesis behind these biases.*

Response: More physical interpretations of the new formulations of AnOHM coefficients in section 2.4. Also, the outlook for potential use of AnOHM has been added in section 5.

3) *The paper is missing a discussion section. The authors can be more critical towards their results, the influence of certain assumptions made in the analysis on the results (e.g. assuming e=ea=es =0.85). Moreover AnOHM should outperform the original OHM, but this is not shown.*

Response: The limitations of the AnOHM framework, including the assumption $\varepsilon_a \approx \varepsilon_s \approx \varepsilon$, have been added in section 5.

For the performance of AnOHM against the original OHM, we have partially addressed this concern in section 4 by demonstrating the ability of AnOHM in capturing the seasonality of the coefficients (see Anandakumar (1999) for observational evidence). Furthermore, in the revised manuscript, we have added the comparison in these coefficients by different modelling and observational regression approaches as Appendix B, indicating AnOHM generally follows the results by observation regression, whereas the typical coefficient values adopted by OHM do not (Figure R1).

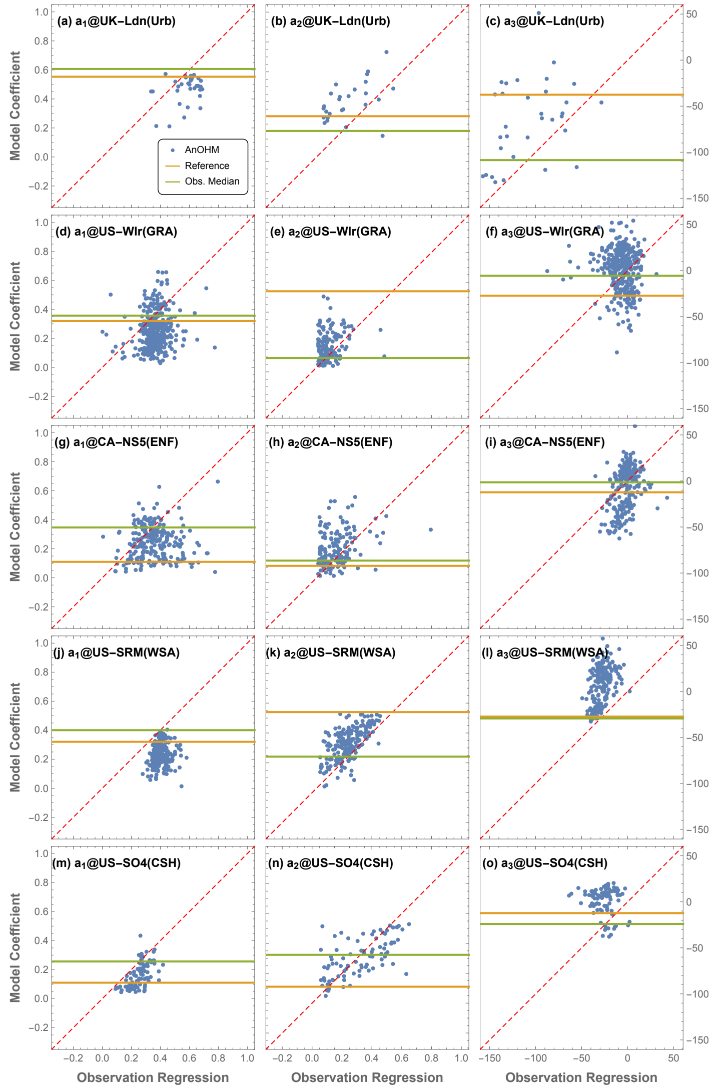

Figure R1 Comparison in OHM coefficients (left, central and right columns for a_1, a_2 and a_3, respectively) between different modelling approaches and observation regression at five sites: UK-Ldn (a, b, c), US-Wlr (d, e, f), CA-NS5 (g, h, i), US-SRM (j, k, l) and US-SO4 (m, n, o). The blue dots denote the pairing values between AnOHM and observation regression. The orange lines represent the reference value used in OHM simulations for land covers with grass and tree (Grimmond and Oke, 1999), whereas the green lines shows median values derived from results by observation regression at corresponding sites.

4) *In equations (10) and (26) the upwelling component e_s*L_down is missing. How does this missing component affect the paper's results and parameter sensitivities, especially to es?*

Response: We thank the reviewer for raising the valuable question about upwelling longwave radiation parameterisation.

In the formulation of outgoing longwave radiation $L_{\uparrow}$, a simplified form (i.e., $\varepsilon_s \sigma T_s^4$) is used for AnOHM as eqn 10 by ignoring the reflected part of $L_{\downarrow}$ (i.e., $(1-\varepsilon_s)L_{\downarrow}$). The rationale for such simplification is explained that given ε_s is usually larger than 0.9, $(1-\varepsilon_s)L_{\downarrow}$ contributes a relatively small portion to the total longwave component (Oke, 1987) and omission of this part is well accepted in the parameterization of upwelling longwave radiation for land surface modeling across various land covers (Bateni and Entekhabi, 2012; Lee et al., 2011).

Using the parameterisation of incoming longwave radiation in the AnOHM framework (i.e., $L_{\downarrow} = \varepsilon_a \sigma T_a^4 \approx \varepsilon_s \sigma T_a^4$), we conduct a sensitivity analysis of the ratio between the ignored part (i.e., $(1-\varepsilon_s)L_{\downarrow}$) and total upwelling longwave radiation (i.e., $\varepsilon_s \sigma T_s^4 + (1-\varepsilon_s)L_{\downarrow}$) at a constant air temperature of 20 ℃ and find this ratio is generally less than 5% given ε_s ranges between 0.90 and 0.99 (Figure R2).

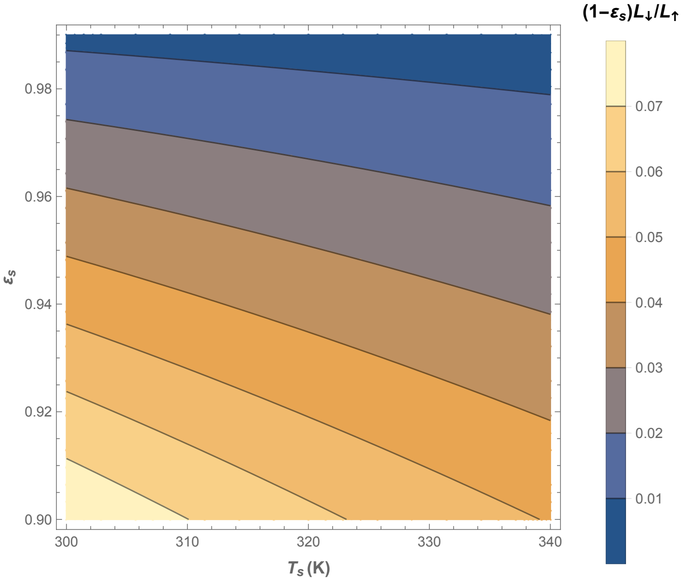

Figure R2 Ratio between reflected part (i.e., $(1-\varepsilon_s)L_{\downarrow}$) and total upwelling longwave radiation (i.e., $\varepsilon_s \sigma T_s^4 + (1-\varepsilon_s)L_{\downarrow}$) at a constant air temperature of 20 ℃.

Moreover, if $(1-\varepsilon_s)L_{\downarrow}$ is included in the net longwave radiation, the induced effect can be incorporated into a modified sky emissivity $\varepsilon_a' = \varepsilon_s \varepsilon_a$ as follows:

$$\begin{aligned} L_{net} &= L_{\downarrow} - L_{\uparrow} \\ &= L_{\downarrow} - (\varepsilon_s \sigma T_s^4 + (1-\varepsilon_s)L_{\downarrow}) \\ &= \varepsilon_s L_{\downarrow} - \varepsilon_s \sigma T_s^4 \\ &= \varepsilon_s \varepsilon_a \sigma T_a^4 - \varepsilon_s \sigma T_s^4 \\ &= \varepsilon_a' \sigma T_a^4 - \varepsilon_s \sigma T_s^4 \end{aligned}$$

Then by assuming $\varepsilon \approx \varepsilon_a' \approx \varepsilon_s$, the derivation following equation 18 still holds. Also, the sensitivity analysis suggests that the derived coefficients are insensitive to ε (cf. S for ε in Figure 2 of the manuscript).
As such, we deem the omission of $(1-\varepsilon_s)L_{\downarrow}$ will not qualitatively change the results of this work.
The above discussion has been added in the revised manuscript.

5) *Equation 21, first line: I have the impression the 4's should be removed (or the last two terms should be replaced by 4*sigma*eT^3(Ts-Ta)).*

Response: We thank the reviewer for pointing out this typo. The 4's have been removed in the revised manuscript.

6) *P11, ln 15: I find the hit rate not a good metric to evaluate this model, at least not if presented as the only metric. In terms of contingency tables, the hit rate should always be*

presented together with the false-alarm rate, and preferably with an critical success index or a threat score.

Response: As the hit rate may bring up confusion to the readers, we have removed this metric but kept the other two (i.e., mean bias and RMSE) in the revised manuscript.

References:

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