Responses to Reviewer 1:

1) *This paper is potential and well organized, but the language proficiency still needs to be polished by Professor Sue Grimmond who is the fourth author and a native English speaker.*

Response: we thank the reviewer for the positive comments on our work. The revised manuscript has been proof read by Prof. Grimmond and other colleagues who are native English speakers.

2) *It will be great if authors compare their own results with the previous work.*

Response: A full comparison in the simulated ΔQ_S of AnOHM and OHM will be presented in an online study using the SUEWS framework. However, we added the initial comparison between AnOHM and OHM in Figure 6 and section 4.

3) *This paper is lacking a formal Discussion section. I suggest the authors develop this section.*

Response: The discussion part has been developed and formed in the new "Discussion and Conclusion" section.

4) *Line 14, suggest change "hampers application" to "hampers its application".*

Response: Changed as suggested.

5) *Line 15, change "1-dimensional" to "one-dimensional".*

Response: Changed as suggested.

6) *Lines 18-19, "From this albedo, Bowen ration and bulk transfer coefficient, solar radiation and wind speed are identified as being critical." I strongly recommend the authors revise this statement.*

Response: The statement has been rephrased as:
"The test suggests that albedo, Bowen ratio and bulk transfer coefficient, solar radiation and wind speed are identified as being critical."

7) *Line 21, change "OHM coefficients to" to "OHM coefficients".*

Response: Corrected as suggested.

8) *Page 2, Lines 9-10. "The volume of interest extends from the top of the roughness sub-layer to the depth in the ground where the vertical net heat conduction is zero on a daily basis (see Figure 2 in Masson et al., 2002)." The statement is contrary to its former statement.*

Response: We deem this statement is NOT contrary to the former statement: the former statement indicates the diurnal dynamics of heat storage in the canopy layer while the latter means the daily averaged heat storage is zero.
To better clarify our intent, this statement has been rephrased as follows:
"The volume of interest extends from the top of the roughness sub-layer to the depth in the ground where the daily average of vertical net heat conduction is zero."

9) *Page 2, Lines 11-16. It is recommended to add the references at where is appropriate, such as after "e.g. 5%", and after "the term becomes much more significant".*

Response: Added as suggested.

10) *Page 2, Lines 19-33, Page 3, Lines 1-17. What are the disadvantages and advantages of OHM compared with the other techniques to determine the storage heat flux? The parts of listed different techniques are verbose (Page 2, Lines 22-33).*

Response: Discussion on the advantages and disadvantages of OHM has been added:
"OHM features the less demanding parameterisations and more direct understanding of control of ΔQ_S by Q^* compared with other approaches. Despite the shortage of OHM coefficients for the wide range facet types, OHM captures the urban SEB processes (Grimmond and Oke, 1999; Järvi et al., 2011; 2014; Karsisto et al., 2015; Roth and Oke, 1995).

11) *Page 3, Line 3. How to determine a1, a2 and a3 by observations?*

Response: The three coefficients are determined by least square regression between ΔQ_S and Q^* observations. This has been clarified in the revised manuscript.

12) *Page 3, Lines 14-16. Suggest change the statement "Although, Gao et al. (2003; 2008) solved the 1-dimensional advection-diffusion equation of coupled heat and liquid water transport to explore the physical relation of OHM coefficients a1 and a2 to the phase lag between ΔQS and Q*," to "Although, the one-dimensional advection-diffusion equation of coupled heat and liquid water transport equation was solved by Gao et al. (2003, 2008), and the solution was used to explore the physical relation of OHM coefficients a1 and a2 to the phase lag between ΔQ S and Q* (Gao et al., 2010), ".*

Response: Changed as suggested.

13) *Page 3, Lines 19-26. What will be done and some results are mixed together. It is recommended that the authors revise those statements.*

Response: This paragraph has been rephrased as follows:
"In this paper, the solutions of the one-dimensional advection-diffusion equation of coupled heat and liquid water transport (Gao et al., 2003; 2008) are employed with the SEB (eqn 1) to investigate more fully the three OHM coefficients, the outcomes of which lead to development of the Analytical Objective Hysteresis Model (AnOHM) (section 2). Then the Monte Carlo-based Subset Simulation (Au and Beck, 2001) approach is used to undertake the sensitivity analysis of AnOHM to surface properties and hydrometeorological conditions (section 3). An offline evaluation of AnOHM's performance for five sites with different land covers (section 4) allows us to conclude that this is an alternative approach to obtain OHM coefficients. As this will allow application across a much wider range of environments and meteorological conditions, it has important implications for land surface modelling (urban and non-urban)."

14) *Page 4. t should be defined below equation (3).*

Response: *t* is time and has been defined in the revised manuscript.

15) *Page 4. It is strongly recommended putting equation (7) before equation (4), as "The steady-periodic solution of equation (3) with boundary condition, Ts=A_Ts sin(wt-Y)+Ts_aver (4)".*

Response: Changed as recommended.

16) *Page 5. Albedo should be defined below equation (14).*

Response: Defined below equation (14) as suggested.

17) *Page 5, Line 9. It is recommended adding references or state the reason that it is reasonable to assume the incoming solar radiation and air temperature follow sinusoidal forms through a day as function of the mean value for the day.*
Response: Reference (Sun et al., 2013) added as suggested.

18) *Page 5, equation (8). Where is the term of longwave radiation from soil surface in the longwave radiation scale (equation (8))?*
Response: We assume the reviewer referred to equation (10) for the outgoing longwave radiation. The longwave radiation from land surface is fully parameterised with surface temperature T_s according to the Stefan–Boltzmann law and thus no extra term is introduced.

19) *Page 7, Section 2.4. I strongly recommend adding statements about the advance of the AnOHM coefficients compared with the previous OHM coefficients. Based on the abstract, to enhance physical interpretations of the OHM coefficients is one of the paper's goals.*
Response: Advances by AnOHM in the physical interpretation of OHM coefficients have been added in section 2.4 of the revised manuscript.

20) *Page 8, Line 20. "Stull, 1998" should be placed at "(Table 1a, based on values reported in Stull (1982))".*
Response: Moved as suggested.

21) *Page 9, Lines 15-17. Based on the statement "A positive (negative) S indicates an increase will lead to increase (decrease) in simulated value", an increase in albedo will increase a_1 and a_3 while decrease a_2 . Because Figure 2 shows that the S of a_1 and a_3 are positive and the S of a_2 is negative. It is strongly recommended double-checking the other statements for surface properties and the statements for hydrometerrolgical forcing parameters.*
Response: The original statement is correct after checking.

22) *It may be interesting to compare the S of surface properties and hydrometerrolgical forcing parameters.*
Response: We thank the reviewer for the suggestion. The discussion on the comparison in S between the surface properties and hydrometeorological forcing parameters has been added in section 5.

23) *It is recommended comparing the results of sensitivity analysis to previous works.*
Response: We thank the reviewer for the suggestion. The discussion on the comparison in *S* between the surface properties and hydrometeorological forcing parameters has been added in section 5.

24) *What does the ability of AnOHM to capture intra-annual dynamics ΔQ_S impact its simulation ΔQ_S ?*
Response: Such ability of AnOHM primarily improves the nocturnal magnitude of ΔQ_S (represented by a_3) as compared with OHM. The original a_3 used in OHM approach usually adopts a single value (e.g., Järvi et al. (2014)) or two values for dry and wet seasons (e.g., Ward et al. (2016)) which constrains the dynamics of ΔQ_S and in particular its nocturnal value. As the

seasonality can be well represented by solar radiation $K_{\downarrow}$, the inclusion of $K_{\downarrow}$ in the parameterisation of a_3 improves the nocturnal magnitude of ΔQ_S.

25) *Authors should be sure to inform the reader of what may be lacking in the study as well as needs for future work.*

Response: The limitations of this work has been discussed in section 5 of the revised manuscript.

26) *It is recommended to add statement that how the current work actually advances science.*

Response: The perspectives of this work has been discussed in section 5 of the revised manuscript.

References:

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Responses to Reviewer 2:

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).

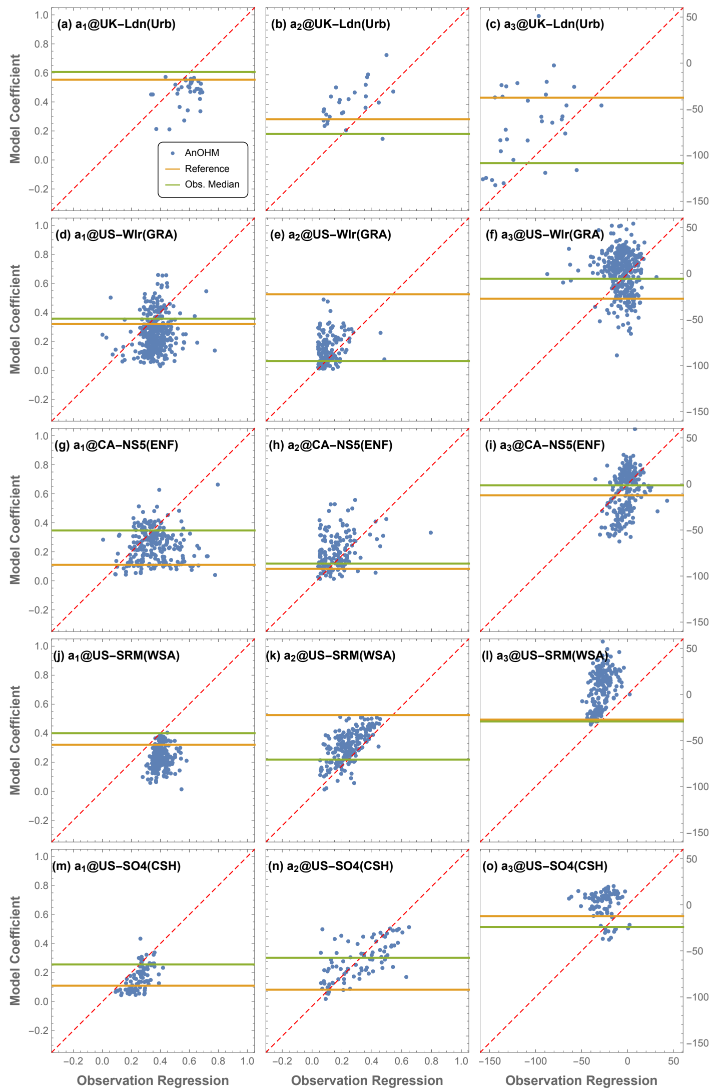

Figure R1 Comparison in OHM coefficients (left, central and right columns for a_1, a_2 and a_3, respectively) between different modelling approaches and observation regression at five sites: UK-Ldn (a, b, c), US-Wlr (d, e, f), CA-NS5 (g, h, i), US-SRM (j, k, l) and US-SO4 (m, n, o). The blue dots denote 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).

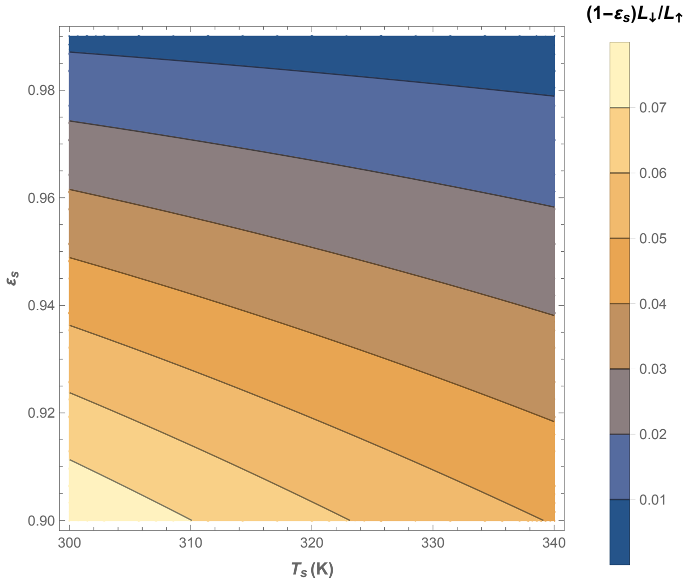

Figure R2 Ratio between reflected part (i.e., $(1-\varepsilon_s)L_{\downarrow}$) and total upwelling longwave radiation (i.e., $\varepsilon_s \sigma T_s^4 + (1-\varepsilon_s)L_{\downarrow}$) at a constant air temperature of 20 ℃.

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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