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Geoscientific Model Development Discussions
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Interactive comment on "The Analytical Objective Hysteresis Model (AnOHM v1.0): Methodology to Determine Bulk Storage Heat Flux Coefficients" *by* Ting Sun et al.

Anonymous Referee #2

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Review of the manuscript gmd-2016-300 The Analytical Objective Hysteresis Model (AnOHM v1.0): Methodology to Determine Bulk Storage Heat Flux Coefficients

Author(s): Ting Sun et al.

Summary This paper extends the well-known OHM model by including an analytical solution of the advection diffusion equation which is subsequently used to study the uncertainty and parameter sensitivities a1, a2, a3 in the OHM model using a Monte-Carlo analysis. In principle the study results are very welcome in the literature since the parameter estimation of a1, a2, and a3 are challenging and further detail is needed for successful application in a myriad of cities. However, I have a number of concerns with the paper that makes me recommend major revisions for this paper

Recommendation: major revisions needed

Major comments: 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.

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.

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.

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?

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*eTˆ3(Ts-Ta)).

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.

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