

Interactive comment on “Surface [Urban] Energy and Water Balance Scheme (v2020a) in non-urban areas: developments, parameters and performance” by Hamidreza Omidvar et al.

Anonymous Referee #2

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Review - Surface [Urban] Energy and Water Balance Scheme (v2020a) in non-urban areas: developments, parameters and performance

The manuscript aims to extend the SUEWS model to non-urban surfaces, with the overall goal to estimate the energy-balance fluxes in such areas. Therefore, specific parameters used to estimate the surface heat fluxes are inferred from observational data sampled at energy balance stations, which includes different vegetation types and different climate zones. The modelled surface fluxes with SUEWS were compared against observational data to evaluate the performance of the SUEWS model over rural surfaces. The topic of the paper itself fits well into the journal and is of interest to the

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research community, especially since reliable input of phenological data and surface data play a key role for reliable estimation of the surface-energy balance components in models. However, after extensive review, I cannot recommend the manuscript for publication until major revisions have been done and extended analysis is presented. My major concerns are outlined in the following.

Major comments

(A) You added new methods and tuned parameters to model impervious surfaces in rural areas. However, the description of newly developed parameter estimation such as for LAI or albedo is mixed with parts of model description, so that it is hardly possible to extract what is new and what has been there already before. I would recommend to first described the state-of-the-art model and describe newly developed approaches separately. Also, the manuscript provides no condense model description of SUEWS but refers to previous papers. The manuscript itself should be readable as a stand-alone paper. Hence, even though not all details need to be brought-up, the manuscript needs to provide a proper overview of the model at one place. Further, please give all information concerning model description in the text, not within the appendix.

(B) The manuscript is sometimes hard to follow due to missing logical order between sentences. In several sections, sentences appear to be disconnected from each other rather than indicating a logical order. As a consequence the text reads more like a collection of notes.

(C) The discussion of the results is not sufficient and lacks important aspects. For example, why is the bias error positive for some sites but negative for others. The authors provide the errors for all sites, but do not try to put these within the context of site-specific information. Also, one of the main problems of eddy-covariance measurements is the non-closure of the energy balance. Especially for the comparison of surface latent heat fluxes this needs to be discussed.

In this context, the manuscript need to provide also more information about the specific

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EC sites. At EC stations located in heterogeneous landscapes the measured fluxes are a mixture of signals emerging at different land-surface types rather than only one type (as assumed in this study), i.e. the footprint of the stations covers several land surfaces with different properties (LAI, roughness). As a consequence the value f_i (which is assumed to be 1 in this study) is not necessarily one. To be able to evaluate the validity of the inferred parameters in this study, site specific information should be provided, e.g. the degree of surface heterogeneity, which in turn need to be correlated to the overall error in the surface latent heat flux for the individual sites.

Minor Comments

55-56: You mention that there is a number of LSM's, but you cite only one. 54-58: In my opinion this leads the reader on a wrong track, the manuscript focuses on non-urban rural sites. 63: I guess you mean "around the globe". 66: The word parameters is unclear at that point and need to be specified. Do you mean certain (bio)physical quantities such as leaf-area densities, surface or material properties, or do you mean certain values used in parametrizations? 71: Which gap does the authors mean? Please be more specific. 96: It is unclear to what does "The former" refer to. 98-99: "Model parameters ...": As a stand alone sentence this makes sense, though it becomes not directly apparent to the reader what is exactly meant. However, from this there is not obvious connection to the following sentence. With changes of the key parameters you may describe any type of vegetation, but how is this related to the statement that parameters need to be consistent? 107:108: How are GDD and SDD defined? Are these vegetation-type specific?

Eq. 3: Does the index i includes all vegetation types including or excluding crops?
Eq. 3: Is LAI_max/min a function of the time of the year? If this is the case, please indicate this somehow within the equation or text. 115-116: Where does these max/min values come from? Here, a reference is required in the text. 121: The note within the parenthesis is unclear to me, how are shorter / longer LAI_max times are reflected in Eq. 3? 175/189: I guess you mean water vapor. 199/200: The authors should

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elaborate why removing G1 from the first term is a valid approach. According to the text it sounds to be an arbitrary decision, though I assume there is a specific reason for this? 200-201 and following: This is not really a sentence but more a note. Also, the following sentences sound more like a note. 204: To be specific, soil moisture deficit is not really a meteorological quantity. Eq. 13,14,15: G_2, G_3, G_4, G_5 are not defined in the text. 240: Parameters itself cannot have a performance. What you mean is the performance of SUEWS using parameters for non-urban surfaces. 246: What do the authors mean with surface state? 285: What do the authors mean with "are not completely independent": among each other? 347-349: In Fig. 6 the authors show the LAI distribution over the year. It does not become clear how this indicates that a constant LAI would lead to poor radiation and surface fluxes. If the authors see a link between these two things it should be given there. Fig. 6: The LAI variation for the evergreen-tree sites is surprisingly high. The minimum LAI values for the respective Canadian sites are similar compared to the deciduous-tree sites. For evergreen trees I would expect a rather time-constant value, while here also the MODIS values indicate almost zero LAI. Could the maybe connected to snow cover on trees? Fig 7b: MBE indicates that the modelled LAI values are biased towards smaller values (not for all sites, but for many), especially during the leaf-on period. However, I miss some discussion about this in the text (line 338-345). Fig 12: Please provide a full description what is shown in the figure. To switch between the figures to find out what is shown makes the figure hardly readable.

Most of the equations: Punctuation is missing.

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