Geosci. Model Dev. Discuss., 8, C163–C166, 2015
www.geosci-model-dev-discuss.net/8/C163/2015/

Geoscientific Model Development Discussions Open Access

Interactive comment on "A non-equilibrium model for soil heating and moisture transport during extreme surface heating" *by* W. J. Massman

Anonymous Referee #1

Received and published: 15 March 2015

Introduction- It is important to clarify that the non-equilibrium process the author is discussing is non-equilibrium phase change between the liquid and vapor phases. This is not clear here, nor at certain points in the introduction and could be confusing to the reader.

Sensitivity analysis on the rho water as a function of temperature. Does this make any difference? Same for thermal conductivity.

Theory section - It is unclear why the author selects specific functional parameterizations over other parameterizations. There is no justification listed as to their performance in soil heating environments compared to other functional parameterizations. Suggest that the author provide some justification/rationale for the selection of each parameterization.

Nonequilibrum phase change approaches/formulations – I would argue that both approaches are empirical rather than truly having a physical basis. For example, the modified Hz-K approach includes a volume normalized interfacial surface area, interfacial surface transfer coefficient and equavelent pore radius, all values that are not easily determined and oftentimes used as fitting parameters. There is much work on the parameterized dynamic condensation coe_cient, none of which was mentioned here. Suggest review of Marek, R., and J. Straub (2001), Analysis of the evaporation coefficient and the condensation coefficient of water, Int. J. Heat Mass Transf., 44, 39–53.

It might be helpful to discuss why Massman 2012 required revisiting/amendment more in the introduction.

The author directly compares the models of Massman 2012 and this model, concluding that the new nonequilibrium based model is a better fit/improvement. I don't think Massman 2012 and this model make for a good direct comparison and allow for the conclusion that the nonequilibrium formulation is the reason the model works better. There are many differences between the two models, making it difficult to pinpoint if the improvements are solely due to the consideration of non-equilibrium behavior. The author should do a direct comparison between the two models with all else equal (including boundary conditions), that would be beneficial.

Sec 3.3 It would be helpful to have a figure or table of initial and boundary conditions as some of them are unclear from the discussion. In addition, the author refers the reader to another paper to better understand the boundary conditions (as well as many other things throughout this work). In addition, in section 4.1, the experiments of Campbell are not well explained, making it more difficult to understand the experiment/model comparison.

Sec 4.2 need to be clear on the definition of dynamic residual soil moisture in this context

Figure 2- the model's performance (ability to capture soil moisture and temperature behavior) decreases with depth. What is the reason for this?

Figure 5 discussion – The author discusses the experimental results of Campbell compared to numerical results, concluding that the experimental results for evaporation are flawed. This discussion is confusing and needs to be better clarified. Need to be consistent with terminology throughout – model referred to as sub-sampled synchronized, synchronized model etc. Please select one.

Figure 6 shows condensation (increase in soil moisture) at a certain depth. This needs to be discussed in the paper as this behavior is not seen in the experimental results. Sensitivity analysis – there is no quantifiable results, only statements like slightly sensitive, weak role etc. Suggest more quantitative descriptions of sensitivity. Water retention curve and hydraulic conductivity function sensitivity analysis discussion – it would be beneficial to show a figure that shows the water retention and K behavior rather than only the discussion. It is unclear how each formulation improves the overall results This would be especially helpful to understand the sensitivity in the dry soil region. The discussion, as written is difficult to follow.

The discussion of the importance of residual soil moisture and values lower than the residual value is very important to this work. This is confusing to me when the author then states that they artificially lowered the residual value in the case of the Dry Quincy Sand. If the water retention model selection is properly considered, why does the author need to make these adjustments? Shouldn't the function able to be adjusted below the residual value based on physical changes such as temperature effects?

4.3.3 Are the initial soil moisture conditions for the entire column (i.e. constant soil moisture throughout)? The author discusses how the model can better capture the evaporation behavior for dry soils rather than wet initial soil conditions, but provides little reasoning for this. Based on others works on evaporation behavior, it is difficult to capture the different stages of evaporation (e.g. atmospheric controlled stage 1,

dominated by capillary action and diffusion controlled stage 2, which is more influenced by the soil properties rather than the atmosphere conditions). Even more difficult is capturing the transition between the stages. It seems that this model is better able to capture the stage 2 dynamics but this leads to a lot of questions on the overall model performance.

The author should discuss the applicability of this model to different scenarios, to include fire burn environments. More of the contribution of this work seems to be the investigation of the specific parameterizations, such as the soil water retention function and others and how this applies to fire burn models.

Interactive comment on Geosci. Model Dev. Discuss., 8, 2555, 2015.