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Interactive comment on "The challenges of an in situ validation of a non-equilibrium model of soil heat and moisture dynamics during fires" *by* William J. Massman

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Response to Comments from Yijian Zeng dated 20 May 2020

My thanks to Yijian Zeng for his comments. They were helpful. Below is my response to the six yellow highlighted comments he made in the margins of the original copy of my paper. Because I basically accepted all his minor technical writing corrections I will not respond to those. My response to his highlighted comments are in italics and follow a restatement of his comment.

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YZ - Line 46 Under the extreme dry condition alike in the desert, the top surface layer (≈1mm-1cm) does not maintain local equilibrium hypothesis, this is also partially demonstrated by the papers as below:

Zeng, Y., Z. Su, L. Wan and J. Wen, (2011): A simulation analysis of the advective effect on evaporation using a two-phase heat and mass flow model. Water Resources Researcgh, 47(10), W10529, doi: 10.1029/2011WR01701.

Zeng, Y., Z. Su, L. Wan and J. Wen, (2011): Numerical Analysis of Air-Water-Heat Flow in the Unsaturated Soil Is it Necessary to Consider Air Flow in Land Surface Models. Journal of Geophysical Research Atmosphere, 116(20), D20107, doi: 10.1029/2011JD015835.

RESPONSE - Line 46 *No Change made at this time. I understand Yijian Zeng's comment and would likely agree that his model "partially demonstrates that the top surface layer of soil in dry or desert conditions does not maintain local equilibrium". But the point I am making here is subtle and specific to the vapor source term, S_v. In Zeng's model there is no explicit formulation of the non-equilibrium source term. I may have misread the papers, but without the ability to directly compare the equilibrium vapor density or pressure with the same model variable I don't see how one can be sure about not maintaining local equilibrium. So given that the model does show non-equilibrium conditions how then can there not be a specific model of a vapor source term, S_v? I suspect that whole issue is avoided or maybe overlooked in Zeng's model because it combines liquid and vapor into a single conservation equation and by combining a large term (liquid water) with a very small term (vapor) the model could induce a potential loss of precision in the calculation of vapor concentration. All that said, my response is not intended to critique anyone else's model, but because the differences between my model and Zeng's are different in some potentially important details concerning how the models treat water vapor I could not find common enough ground to cite the suggested papers. But I do agree that citing one of the Zeng model papers is appropriate as I mention in my response to Zeng's last comment, Lines 565-567, below.*

YZ - Lines 97-98 Could you explain a bit more on why this non-dimensional form of matric potential? What are benefits? Numerical stability?

RESPONSE - Lines 97-98 *No Change made at this time. My non-dimensionalization of the matric potential, ψ, is purely idiosyncratic to me. To my knowledge there are no numerical benefits. I prefer the use of the matric potential in general (as opposed to pressure (Pa) or pressure head (m)) because it can be directly related to thermodynamic variables. I also prefer to emphasize the parabolic nature of the equation of mass conservation for liquid water and its similarity to the advective-diffusive equations (ADEs). So, the non-dimensionalization allows the physical dimensions of K_n (m^{2}s^{-1}) and K_H (ms^{-1}) to be directly comparable to similar terms in ADEs. I am willing to include a sentence in the paper if the reviewer thinks that it would be helpful. My own feeling is that to do so would not benefit the paper.*

YZ - Line 228 First time appearance - full spelling needed for a better readability.

RESPONSE - Line 228 *A Minor Change was made. The revised manuscript now places quotes around the term BFD curve. Barnett (2002) gives no explanation of what the "BFD" actually means. Nor could I find any thing in any of his subsequent papers or any papers citing his original 2002 paper that provided any explanation. I note that the second referee also made the same comment.*

YZ - Line 300-301 Is there any specific reason for putting 40%? In other studies in desert environment, the soil vapor pressure is not saturated only for the very top soil surface layer, for the rest of soil profile, it is pretty much saturated (i.e., at 99%).

RESPONSE - Line 300-301 *No Change made at this time. In order to assure that everything is in equilibrium when the model (or fire) is "turned on" I assume that the soil vapor density and vapor pressure profiles are uniform and equal to my best guess as to what the ambient atmospheric vapor density or pressure would be at the measured ambient atmospheric temperature. So in this sense the 40% relative humidity is a bit arbitrary. I do agree that the soil relative humidity is likely to be much less near the surface than deeper into the soil. But including this in the initialization of the model would then mean that there is a vapor pressure/density gradient, and therefore, there would likely be some soil evaporation occurring. This is very likely the case, but under the circumstances of the fire any soil evaporation simply cannot be known or verified and so for the modeling purposes this extra level of detail is just ignored.*

YZ - Line 409 Could be this point more specific?

RESPONSE - Line 409 *A Change was made. The manuscript now reads "The design of this particular probe is fairly standard, but the material used to house the steel needles and the connectors attaching them to the coaxial (data/signal) cables had to* *continue operating and providing reliable data at temperatures exceeding 250 C. To ensure this external portions of the coaxial cables that were likely to be exposed to such high temperatures were wrapped in silicon tape." Here the changes are marked in red.*

YZ - Lines 565-567 In the studies as mentioned in the desert environment, the inclusion of airflow increase surface evaporation 33% on the day right after rainfall event (<6mm).

Zeng, Y., Z. Su, L. Wan and J. Wen, (2011): A simulation analysis of the advective effect on evaporation using a two-phase heat and mass flow model. Water Resources Researcgh, 47(10), W10529, doi: 10.1029/2011WR01701.

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RESPONSE - Lines 565-567 *A Change was made. The text now reads: "Additionally it would be worthwhile to include the dry air density, ρ_d, as a separate model variable. Certainly in any real fire the temperature and pressure of the dry air within the soil pore spaces would respond dynamically to heating. But including ρ_d as a dynamic variable should yield a more physically realistic simulation of the diffusional and advective transport of water vapor during the fire. Certainly the results of Zeng et al. (2011) for less extreme conditions support this notion." Where I have added the second sentence (in red) and I refer to the second of the two Zeng et al. papers.*

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