Author's Final Response to Review Comments on HESS-2020-193
September 15, 2020

To handling editor: My thanks to you for your effort and to the three reviewers and M Novak for their comments, all of which have been helpful. I have indicated my major additions and clarifications in red, both in this response and the revised manuscript. In my response to the third reviewer and M Novak, I use the color blue to either ask a question to the commenter or to note a deleted sentence. I have also altered my online response to Y Zeng and the second reviewer. I have shortened my response to these two reviewers and I have added the exact line numbers of the revised manuscript where I have made changes in response to their comments. Unless otherwise noted by a specific response to a review's comment, I accepted all minor technical corrections without response. Finally, my response to all comments are in italics and follows a restatement of the reviewers' specific comment.

***** Author's Response to Editor's Comments on HESS-2020-193 dated August 27, 2020 *****

Editor - 0)
Editor Decision: Reconsider after major revisions (further review by editor and referees) (27 Aug 2020) by Nunzio Romano Comments to the Author:

Dear Author:

Your original submission received appraisals from three reviewers and comments from one discussant. The three reviewers evaluated this study quite well and also make suggestions aimed at improving the original paper. The discussant, Dr. M.D. Novak, was a bit more critical on some points of this study and raised concerns about a few ways you have tackled the problem at hand. However, you did not respond to the discussant's comments. Anyhow, allowing for the reviewers? rating and your preliminary replies I inform you that your paper can pass on the subsequent step under moderate/major revisions.

As editor in charge, below I report some points that deserve clarifications in your next point-by-point reply:

RESPONSE - 0) *OK. Thanks.*

Editor - 1) L. 290-297. Please, make more explicit the type of lower boundary condition you used in this study. I disagree with your statement that, I quote, "for field-based applications the lower boundary condition will never be known (or knowable without extraordinary effort before hand)" since definitely there are ways to have at least an idea of the type of condition one has to set at the lower end of the flow domain. For soil-water flow, one might at least superimpose a unit gradient of the total water potential head or a zero flux if a compacted clay soil is located at a depth. Moreover, setting the lower boundary condition at a soil depth of 0.60 m does not solve the problem that this condition might affect the outcomes of model simulation. As some modelers also do, I suggest you should put the lower boundary conditions well below the flow domain of interest, for example, you can set the lower boundary condition at a depth of, let's say, 1.0 m or 1.5 m, and then show the model results for the first 0.60 m of the soil profile. I'd appreciate it if you discuss this point in your reply and justify your choices.

RESPONSE - 1) Lines 290-297 *A Change was made. I added the following paragraph (Lines 331-347):* ***"Although this combination of pass through lower boundary condition and placement depth (0.6 m) is fairly general, it is still possible that they may influence the model's solution within the region of greatest interest (the top 20 cm or so). To test for this possibility a sensitivity test was performed by increasing the domain depth to 1.0 m. The resulting change in the model's solution was negligible throughout the upper 0.6 m. Two further simulations were done each with a zero-flux lower boundary condition for soil moisture (i.e., $\partial\theta/\partial z|_{z_{bottom}}$ = 0 or more specifically, $\partial\psi/\partial z|_{z_{bottom}}$ = 0), one with a domain depth of 0.6 m and the other with 1.0 m depth. The impact of these changes on the model simulation was, for the present purposes, also negligible. This zero-flux lower boundary condition for θ would seem to be a credible, but apparently unnecessary, alternative to the present pass through boundary condition, which is preferred here because it allows all model variables and their associated fluxes to be dynamically modeled. θ was chosen for this sensitivity analysis because it can be plausibly argued that there is enough known about a site's soil structure to make a credible guess at the depth at which the zero-flux boundary condition could be assumed. Soil Temperature is better dynamically modeled at the lower boundary because the heat pulse for the fire can extend far deeper than 0.6 m regardless of the soil structure (e.g., Massman and Frank 2004). Finally, the pass through formulation is also the logical default for vapor density because (a) ρ_v is more strongly controlled locally by the balance between evaporation and condensation than by the lower boundary condition and (2) simply decreasing the effective vapor diffusivity, D_{ve} , is sufficient to mimic regions of highly impermeable soil without the need to explicitly incorporate a specific non-pass through boundary condition."***

2) **Editor - 2) L. 323.** It seems there is a mix-up with the concept of residual soil-water content. The literature on this subject is also showing that the volumetric soil-water content takes on the zero value when the suction head attains a value of about 10^7 cm, and also should also account for film flow (e.g. see J. Hydrol., 2020, vol. 588 #125041).

RESPONSE - 2) Line 323 *No change. Thank you for pointing out the Rudiyanto et al. (2020) (Rea20) paper. It looks like it is a very useful paper. Unfortunately, I do not understand this comment.*

(1) Neither Rea20 nor my paper is concerned with residual soil moisture, θ_r. I investigated formulating the WRC with a θ_r term in Massman (2012) and found that is was not particularly useful, so I have not included it in the present manuscript.

(2) Rea20 clearly say (third line below their Equation (2)) that they are assuming that the pressure head at which the soil moisture goes to zero with the WRC is, h_0 = -6.3 $\times 10^6$ cm $\approx$ = -0.62 $\times 10^6$ J kg^{-1}. So it is to be expected (quoting the editor) "that the volumetric soil-water content takes on the zero value when the suction head attains a value of about 10^7 cm". This is perfectly consistent with my results concerning the Fredlund and Xing (1994) model, but I define an h_0 to correspond to $\psi_* =$ -1 $\times 10^6$ J kg^{-1}. I also note that Equation (1a) of Rea20 refers to Van Genuchten's (1980) WRC model, which is the identical mathematical form for my study. I further note that Figure 1 of Rea20 clearly shows that the van Genuchten (1980) model, their Equation (1a), cannot go to zero until $h = \infty$. The one thing I do not do is to use Equation (2) of Rea20. Defining $S_e(h)$ in this way, any WRC curve will produce $\theta = 0$ "when the suction head attains a value of about 10^7 cm".

(3) And finally, this is independent of the assumption about film flow because film flow is associated with the hydraulic function. But the nice thing about Rea20 is that they unify the WRC and the hydraulic functions by defining both in terms of $\Gamma(h)$ and $\Gamma(h_0)$. My model does not consider this approach at all.

Although I can see a clear advantage for using such an approach with almost any model.

Editor - 3) L. 334. I suggest you should title this section as follows: “2.7 Hydraulic conductivity function”. Please also change the name of this function accordingly.
RESPONSE - 2) Line 334 *OK. I have done so.*

Please, upload a revised version of your manuscript together with a detailed point-by-point reply to all of the comments received so far from both the reviewers and the discussant. Please be advised that a subsequent round of referees? evaluation might be required.
RESPONSE *OK.*

***** Response to Comments from Yijian Zeng dated 20 May 2020 *****

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 Research, 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? But, the differences between my model and Zeng's are potentially important and involve the details concerning how our respective models treat water vapor. I could not find common enough ground here to cite Zeng's 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 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^2s^{-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 (i.e., Line 253 now reads "BFD"). 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. Lines 473-476 now read: 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.*

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 Research, 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 - Lines 565-567 *A Change was made. Lines 647-648 now read: "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. The results of Zeng et al. (2011) for less extreme conditions support this notion." Where the citation to Zeng et al. (2011) refers to the second of the two Zeng et al. papers.*

***** Response to Comments from Anonymous Referee #2 dated 8 June 2020 *****

Referee - 0a) This manuscript makes novel and useful contributions toward an improved understanding of how soils are impacted by wildfire. But the contributions are actually much broader than that. This work addresses a number of key issues relevant to general topic of coupled heat and water flow in soils. As such, the work most certainly falls within the scope of Hydrology and Earth System Sciences.
RESPONSE - 0a) *Thank you and I hope this paper does reach a larger audience than just the fire science community.*

Referee - 0b) The manuscript is well written, the experimental work is described in sufficient detail, and the changes made to the HMV model are described clearly and with appropriate mathematical notation. The comparisons between the modified HMV model and the experimental data provide sufficient support for the interpretations and conclusions of this study. Although this manuscript is in excellent shape, the following issues need to be addressed before this manuscript is in suitable form for publication:
RESPONSE - 0b) *Thank you. The following should address your concerns.*

Referee - 1) The Abstract needs to be revised to include a description of the changes made to the forcing function and the parameterization of the surface energy balance. This is a major component of the study, yet receives no mention in the Abstract.
RESPONSE - 1) *A Change was made. Lines 11-13 now read: This study also (a) develops a general heating function that describes the energy input to the soil surface by the fire and (b) discusses the complexities and difficulties of formulating the upper boundary condition from a surface energy balance approach.*

Referee - 2) Lines 60-63: It is not clear how the parameterization of the surface energy balance was improved. On lines 243-244 we read that the surface energy balance formulation is slightly different than in the previous work, but it is not clear how this improved the parameterization of the surface energy balance. This requires clarification.
RESPONSE - 2) *A Change was made. Lines 268-275 of the revised text now read: "The energy balance at the soil surface used with the present study formulates the net infrared radiation loss at the surface as a balance between the outgoing and incoming infrared radiation. This is different from either Massman (2012) or Massman (2015), neither of which included the possibility of incoming environmental infrared radiation being absorbed by the soil's surface. Here the surface energy balance is expressed as*

EQUATION (16)

where the '0' subscript refers to soil surface and the term on the left hand side of this equation is the energy absorbed by the soil (and assumes that absorptivity and emissivity of the soil are the same) and the first term on the right hand side is the net infrared heat loss (where the term $\propto \epsilon_a(\rho_{va})T_{Ka}^4$ was not included in either Massman (2012) or Massman (2015)), ..."

Referee - 3) Lines 195-196: It would be appropriate to point out here that this observation differs from what Massman (2012) concluded regarding the effects of infrared radiation on soil thermal conductivity.
RESPONSE - 3) *A Minor change was made. I believe that the reviewer is really referring to Massman*

(2015) because Massman (2012) does not include R_p or the infrared component of λ_s (the Bauer term) to which R_p is essential. So I will address the results of Massman (2015).

Massman (2015) does not really conclude anything definitive about R_p except that the model did a better job at reproducing the Campbell et al. (1995) Quincy sand soil temperature observations if R_p were increased to 4000 μm = 4 mm, which is much greater than the default value of 1000 μm = 1 mm (see figure 1 of Massman (2015)). Otherwise even at the relatively large value of $R_p = 1000$ μm Massman (2015) suggests that the Bauer term does not impact λ_s or the soil temperatures very much. So at this point there is really nothing to justify revising the paper.

Nevertheless, the following sentence now appears on Line 221: Massman (2015) reached a similar conclusion.

Referee - 4) Lines 533-534: The thermal conductivity model contains no explicit dependency on bulk density, but it does include porosity. Why not incorporate the effect of bulk density on conductivity via the effect it has on porosity, as was done for the WRC, hydraulic function, and the source term? This seems rather odd to me. Is there a reason why such an approach would not (or perhaps did not)? If so, that certainly needs to be addressed in the text of the manuscript.

RESPONSE - 4) *A Change was made, in red below. The change in porosity was included in the change in thermal conductivity. My original wording was did not clearly state this to have been the case. I have altered the manuscript to indicate that the thermal conductivity also changed with the change in bulk density. And the referee is correct that it is the soil porosity that changed with the change in bulk density. Lines 602-610 now read:*

"The following model parameters were changed for this sensitivity analysis to feedbacks: soil bulk density increases from 1.30 Mgm^{-3} to 1.46 Mgm^{-3} (a 12% increase as per figure 1); simultaneously the thermal conductivity of the mineral fraction, λ_{m0}, increases from 4.42 WmK^{-1} to 8 WmK^{-1}, the de Vries shape factor, g_a, decreased from 0.123 to 0.06, the Campbell et al. (1994) parameter q_{w0} (which determines when water content starts to influence the soil's thermal conductivity) decreased from 0.03 to 0.02, the soil's volumetric specific heat increases by 10% (in accordance with the observations made by Butters (2009)), the overall soil thermal conductivity, λ_s, increases by 15%, and finally the source term coefficient, S_*, decreases from 0.1 to 0.08 (specifically chosen to be a 20% decrease). This increase in bulk density yields a concomitant decrease in soil porosity, η, which is simply carried over in a purely linear fashion to the soil thermal conductivity, the WRC, the hydraulic function, and the source term, S_v."

Technical corrections:

Referee - Line 228: What does BFD stand for?

RESPONSE - Line 228 *Yijian Zeng also asked the same question. My response to him is:*

A Minor Change was made. The revised manuscript now places quotes around the term BFD curve (i.e., Line 253 now reads "BFD"). 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.

Referee - Lines 452-453: This sentence requires clarification. To understand the context of this statement, it would useful to how much vertical structure in lambda-sub-s is included in the model.

RESPONSE - Lines 452-453 *A Change was made. The following sentence was added (Lines 524-526): This could easily be the case for MEF soils because the present model of λ_s does not include any of the observed vertical structure in soil bulk density or its relationship to the vertical structure of the soil's thermal conductivity (Massman et al., 2008).*

In regards to this last comment: The reviewer further states in a later comment (dated July 6, 2020):

Referee: Nowhere in the original manuscript does the author explicitly states that bulk density was assumed to be constant with depth. This needs to be clearly communicated in the description of the model. The sentence above should be retained, as it does indeed provide clarification, but this alone is insufficient. It needs to be made clear earlier in the manuscript (i.e., in the description of the model) that bulk density was taken to be constant with depth.
RESPONSE: *A Change was made. Lines 127-129 now read: where η (m^3m^{-3}) is the total soil porosity, assumed to be temporally constant and spatially uniform (note: η is obtained from the soil's bulk and particle densities, which are the actual model input variables, and because both these variables are assumed constant and uniform, so also is η), and ...*

***** Response to Comments from Anonymous Reviewer #3 dated 31 July 2020 *****

Reviewer - 0a The paper is well written and clearly structured. I enjoyed reading the manuscript and find much of the work is explained such that it is also understandable for those without a background in fundamental soil physics. I think it is appropriate for HESS readership.
RESPONSE - 0a *Thank you.*

Reviewer - 0b I have two major comments and several others, included below.
RESPONSE - 0b *I hope the following response should address your concerns and comments.*

Reviewer - 1 A first main comment is regarding the framing of this work to aim to model soil temperatures in fires, but being only validated in one burn, and a very specific one. The manuscript could benefit from more careful explanation of the differences between various types of fire and the implications for this for the modeling performed.
RESPONSE - 1 *OK*

Reviewer - 1a L32: Please update this sentence to clarify you are talking about slash pile burns, and cite work that supports your statement. Prescribed burns that use moving fire fronts rather than static pile burns typically have fire residence times of mere minutes and usually take place when soils are wet, usually keeping mineral soil temperatures very low or barely changed. Somewhere in this intro it would also be good to explain what slash pile burns are and how they differ from moving fire fronts in prescribed and wildfires in terms of residence time and soil heating.
RESPONSE - 1a *A Change was made. I removed the following sentence (including Line 32)* ***"Unfortunately for soils, managing fuels often takes the form of biomass burning (i.e., prescribed burns), the consequences of which for the affected area can be as bad as or even worse than wildfire."*** *I have added the following sentences (Lines 36-51) in an effort to address the reviewer's concern about how fires differ and how my model might be used to investigate the impact of both static and dynamic fires.* ***The benefit of a model lies with the ability to vary amount and duration of heating (e.g., [Steward et al. (1990)]) that characterize different fire types and to better judge how fire impacts different soil types; and just as there are a variety of different soil types with differing thermophysical properties, there are also a variety fire types, characterized by duration and intensity (energy or soil heat flux). For example, prescribed burning of understory vegetation, an example of spreading or dynamic fire used to reduce understory fuels, is usually low intensity and will heat the soil for only a few minutes. But as the fuel loading increases so does the fire's duration and intensity (e.g., [Massman et al. (2003)]). On the other hand, prescribed slash pile burns, like the one studied here, are static fires that can heat the soil with 1-10 kWm^{-2} for 10's of hours (e.g., Massman and Frank 2004; Massman et al. 2008). Wildfires, and crown fires in particular, are dynamic and often fast moving and relatively brief by comparison to slash pile burns, but they can also be extraordinarily intense (10-100 kWm^{-2}). Furthermore, this type of fire can cause burning material, e.g., tree boles or other woody material, to come into direct contact with the soil and then continue to burn or smolder for long after the fire front itself has passed. This is the worst of all possible situations because whatever impacts the dynamic portion of the fire may have had on the soil will only be made much worse (at least to the area surrounding the point of contact). With the present modeling approach it makes it relatively easy (just by***

changing the model's soil surface boundary forcing) to estimate the depth of penetration of critical temperature thresholds (e.g., Massman et al., 2010, figure 1) for differing soil types.

Reviewer - 1b L62: please discuss somewhere (probably in the discussion) how the results of your work are applicable in dynamic (not static) fires like moving fire fronts, especially given the fact that you describe that heating processes differ between static and dynamic burns (L 215) and that the model was only validated with data from static fire, but that your aim was to model soil temperatures during fires (plural, L441)?
RESPONSE - 1b *I have tried to address this issue with the changes I described in my RESPONSE - 1a above.*

Reviewer - 1c L341: What is the rationale of calibrating this model on a slash pile burn site and not in a moving fire front?
RESPONSE - 1c *No change was made at this time. The model has been calibrated against 10 other fires, two of which were moving fire fronts. All fires were instrumented with soil temperature measurements. But only this 2004 MEF fire had soil moisture measurements. One other MEF fire (2002) had soil heat flux measurements. Otherwise no other fire included soil heat flux measurements. The soil moisture and heat flux measurements are extremely important for evaluating the model's performance and formulating the upper boundary forcing. So I focused the present manuscript on the 2004 burn. I could certainly add a comparison of modeled and observed temperatures during the moving fire front experiment to the present manuscript. But I am not sure if there is anything new to be learned from doing so.* ***On a personal note: I am not sure what the reviewer's question has to do with (the original) Line 341, which just states that there is no information for modeling K_R at the MEF burn site?***

Reviewer - 1d Line 513: Discuss here whether these soil changes are likely to occur or not in wildfires and prescribed fires that have moving fire fronts. I agree with this being relevant for static burns but this paragraph needs context regarding the likelihood of this happening in other types of fires. In light of that, which types of fires do require this explicit inclusion of dynamic feedbacks? The current text makes it seem that all fire is the same, which is too simplistic.
RESPONSE - 1d *A Change was made. Lines 585-590 now read:* ***Although this statement should self-evident, it is probably more precise to say that changes to the soil result from the integrated effects of the surface heat flux (delivered to the soil), the area of soil in contact with or influenced by the fire, and the duration of the fire. That is the probability of change to soil due to fire is $\propto \int_0^{t_d} \int^{Area} G(z=0, Area, t)\, d(Area)\, dt$. This more precise approach clearly suggests that changes to the soil are more likely to occur during spatially static fires (e.g., slash pile burns or relatively low intensity but long duration burning biomass) than during a fast moving dynamic fire (grassland or low shrubland fire) of moderate or even high intensity. Intermediate between these two extremum case is the relatively fast moving, but extremely high intensity, crown fire.*** *These additional sentences are basically meant to rephrase in a more precise way the statement I added to the Introduction, i.e., Lines 36-51 as discussed above in my Response - 1a to this reviewer. I also think it is possible to infer from Lines 564-579 that I may have a slightly different view on fires and how they can impact soils than that held by the reviewer. I do not necessarily agree that the original manuscript was "simplistic [by making] all fire [seem] the same." I explained at length about how formulating the upper boundary condition was difficult*

and could easily vary with different fire types. In the section on the upper boundary condition and the surface energy balance does distinguish between static and dynamic fires. But within a modeling context some simplification is necessary and ultimately imposed by the need to formulate (as much as possible) simple, realistic and tractable boundary conditions.

Reviewer - 2 My second main comment is regarding the goal of this study to provide a tool to support management (L596). I wonder about both the inputs to this model and the ease at which others can use this model. As far as I know there is not a simple model (or model at all) available online that allows prediction of soil temperature curves given a set of soil properties and fire duration. It is good that the MATLAB code will be released but that will only be accessible to a very select group of people, also within science. Release of the model as an R package or as R shiny could considerably increase its potential use amongst scientists and R shiny may make it available to those outside of science. I think there is great potential for this that would increase the impact of this work. Related to that, it was not entirely clear to me how broad a range of input data is needed to run the model for easy use. Are all the complicated parameters needed or can the model be run just with information on readily available soil characteristics like texture, bulk density and organic matter content, and that the software does the rest? That would also considerably increase the accessibility of this work for further use.
RESPONSE - 2 *A Change was made. I added the following sentence. Lines 677-678 now read: "(In fact, a version of the HMV-model is currently available to land managers through the USDA Forest Service's First Order Fire Effects Model (FOFEM) version 6.7 [USDA Forest Service (2020)].)"*

To further elaborate for the reviewer: The model has been recoded into C when it was incorporated into version 6.7 of FOFEM. The model is available to anyone who wishes to use it. The installer is available at https://firelab.org/document/fofem-files and the direct link to the installer, which will change when the next version of FOFEM is released, is available at https://www.firelab.org/sites/default/files/images/downloads/FOFEM_6-7_Install.zip. The GUI displays all the input variables the user needs to supply. We have tried to keep most of the model's complexity hidden from the user.

But note: (1) The version of the HMV model accessible through these web links does not include the ability to switch between surface energy balance formulations as done for the sensitivity analysis discussed in the present manuscript. (2) Nor does it include the adjustment, Q_{Fin}, for the initial disequilibrium in the infrared radiation. (3) Updating the HMV model FOFEM code should be possible as long as they are compatible with any budgetary constraints. (4) A paper describing the FOFEM implementation of the HMV model is currently in preparation after about a year's delay. I had hoped that the FOFEM-HMV paper would have preceded the present manuscript. (5) At present I am unaware of any plans to re-code the HMV model in any other computer language.

Additional comments:

Reviewer - 3a L39: can you clarify here what the need is for an updated model? What has gone wrong with the previous model and what is the risk of using the previous model without the changes proposed here? Are temperatures off, or does it not work in a specific soil type or moisture conditions, and by how much are the temperatures off? I.e. can you explicitly mention the improvement that is made to modeling soil temperatures zooming out from the fact that the equations may be more accurate?

RESPONSE - 3a *A Change was made. The reviewer's question is in many ways the subject of the discussion between M Novak and myself. Without going into my detailed response to M Novak, my answer here to third reviewer is:*

The changes to S_v, see Equation (9), make only a small change to the model's performance. M Novak argues from his 2019 paper that this should be the case and I confirmed it by making several tests runs of my model. In the case of the double counting of the soil's apparent thermal conductivity, it does make a difference to how the model distributes the incoming energy between soil storage ($C_s \partial T/\partial t$) and heat conducted to deeper layers of the soil ($-\lambda_s \partial T/\partial z$). But in calibrating my model I adjusted λ_{m0} (see Lines 189-200 for a definition and discussion of λ_{m0}), which would have disguised any differences between before and after dropping the double counting. The intent of the present paper is to discuss the model performance relative to the in situ observations made during the 2004 fire. I have no intent of exploring the consequences of misusing the apparent thermal conductivity in the present study. But note: (1) a colleague and I may explore this subject in a later paper and (2) M Novak speculates a bit more on the changes in soil temperature associated with eliminating the apparent thermal conductivity.

Nonetheless, I revised the Abstract (Lines 6-11) to read: "Improvements to the model eliminate two important (but heretofore universally overlooked) inconsistencies: one that describes the relationship between evaporation and condensation in the parameterization of the non-equilibrium vapor source term and the other, is the incorrect use of the apparent thermal conductivity in the soil heat flow equation. The first of these made a small enhancement in the stability and performance of the model. The second is an important improvement in the physics underpinning the model, but had less of an impact on the model's performance and stability than the first."

So basically my purpose in the present manuscript in regards to these two changes to the model is to point out that they important because they improve the basic physics underlying the model. But their impact on the performance is small in part by "re-tuning" the model parameters to the data (S_*, λ_{m0}) and in part by the nature of the changes and parameterizations themselves (S_v).

Reviewer - 3b L49: what are these less extreme conditions, just diurnal fluctuations?
RESPONSE - 3b *A Change was made. Line 65 now reads: "also demonstrates (under less extreme conditions than during fires, **i.e.,** $0 < T \leq 60$ **C**)". Also note: I believe M Novak in his comment on the first version of my manuscript would claim that his results apply equally to conditions that occur during fires as well. I address this issue in my response to him and so will refer the reviewer to M Novak's comment and my response, if he is interested in these matters.*

Reviewer - 4 L141: How does this work in wetter soils? In temperate regions fires typically occur in Spring when soils are wet. Is the model valid then as well? If so, is there a potential error because of this assumption?
RESPONSE - 4 *No change was made at this time. I am not sure to what the reviewer's question is referring. So I will provide two answers in the hope that one is correct.*

***(a)** The source term, S_v, works the same way in either wet or dry soils because of how $A_{wa,dry}$ is defined. It is only for very dry soils that $A_{wa,dry}$ differs from $A_{wa}(\theta)$ because condensation should be able to occur with or without soil moisture being present. But, does this asymmetry in $A_{wa}(\theta)$ and $A_{wa,dry}$ have any impact on the model simulation or performance? No, the consequences are minor. Lines 6-11*

(Abstract) now read "Improvements to the model eliminate two important (but heretofore universally overlooked) inconsistencies: one that describes the relationship between evaporation and condensation in the parameterization of the non-equilibrium vapor source term and the other, is the incorrect use of the apparent thermal conductivity in the soil heat flow equation. The first of these made a small enhancement in the stability and performance of the model." I refer the reviewer to M Novak's comment and my response, if he/she would like a more detailed discussion.

***(b)** But if the reviewer is asking what are the potential consequences of water vapor that is transported to cooler parts of the soil and then condensing on wet soil surfaces, then yes there is potential for a model instability. An instability will occur whenever the modeled soil moisture, θ, exceeds the air filled porosity, η. In other words, the model cannot accommodate the possibility that the soil exceeds fully saturated, i.e., $\mathcal{S}_w > 100\%$.*

Reviewer - 5 L299: how realistic is it that the initial soil temperature is uniform with depth and how would a change in this approximation alter the model outcome?
RESPONSE - 5 *No change was made. The assumption of a uniform initial soil temperature is just a convenient way to initialize the model. It really has no significant impact on the predicted temperatures because, in general, the soil temperatures of the greatest interest during a fire are likely to far exceed the initial soil temperatures. Otherwise the overhead required to initialize the model, either from observation or simulation, with a realistic profiles of temperature and soil heat flux seems unnecessarily high.*

Reviewer - 6 L356: to make this info more accessible for a European readership please include the name of this soil type using the FAO WRB system (World reference base for soil resources, https:// . . . .
RESPONSE - 6 *A change was made. Thanks for pointing out the FAO soils document. The soil classification given in the text, "deep (> 1.0 m), fine-loamy, mixed, frigid, Pachic Argiustoll" was determined in 2003 by a US Forest Service soil scientist after she and I spend a day sampling the area within and near the burn site. I am not a soil scientist and have no first hand knowledge of soil classification schemes. But I was able to get help from Dr. Curtis Monger (National Leader Soil Survey Standards, USDA-NRCS National Soil Survey Center, Lincoln, NE). His statement is* ***"The mollic keys out before the argillic, so the argillic horizon is signified by the Luvic principle qualifier. Having a mollic, the soil might have keyed out as a Chernozem if it were dark enough or Kastanozem if not, but both of those soil types require a calcic. Therefore, the Reference Soil Group is a Phaeozem. Other than the overly thickened mollic, the soil had no special features, such as redoximorphic features, shrink-swell, etc.. Therefore, the only supplementary qualifier would be pachic shown in parentheses."***

Line 419 now reads: "deep (> 1.0 m), fine-loamy, mixed, frigid, Pachic Argiustoll (or Luvic Phaeozem (Pachic) [FAO UN (2014)])".

Reviewer - 7 L358: suggest replacing soil organic material by soil organic matter, and please indicate the depth across which this organic matter percentage is valid.
RESPONSE - 7 *A Change was made. Lines 422-423 now read: "Soil organic matter comprises about 1-2% of the soil by mass and is more or less uniform through at least the top 10 cm of soil [Jiménez Esquilín et al. (2008)]". For the purposes of completeness: The soil surface throughout MEF is patchily covered with a duff layer of varying thickness. I can add this last sentence to the manuscript if the reviewer thinks it is valuable to do so.*

Reviewer - 8 L360: I don't understand, how can grazing and harvesting disturb the soil, do you mean the very soil surface? Or were there very deep ruts or something? I can imagine this affects soil cover but not the soil. Maybe good to indicate to which depth this disturbance occurred.
RESPONSE - 8 *No change was made. My understanding is that grazing and logging largely compact the soil, therefore these activities are not just surficial disturbances. But I am afraid I cannot really quantify any potential impacts by depth. I have never sampled any undisturbed areas within MEF. The soil bulk density profiles I do have indicate that the top 20 cm or so are more compacted than the deeper layers, but I know of no study or method that could determine what might be natural background bulk density changes and what might have been caused by logging and grazing.*

Reviewer - 9 L402: I'm intrigued, could you explain how it could never have been measured during a fire? Never at all or never during the fire?
RESPONSE - 9 *A Change was made. Line 468 now reads: "this or any fire". In situ measurements of λ_s (at least those I am most familiar with) usually require delivering an electrical pulse to (and thereby heating) a 12-13 cm long stainless steel needle and measuring the temperature rise and fall during and after the pulse has been delivered. The rate of rise or fall of the temperature is related to the thermal conductivity of the medium. If the medium has a relatively low thermal conductivity (i.e., it is acting more as an insulator than a conductor), then the needle's rise rate will be steep because the heat cannot be conducted away from the needle surface very efficiently, and of course just the opposite if the medium is a good conductor of heat. During a fire I think the signal to noise would be so great that the chances of detecting the small, local and fairly fleeting temperature signal from the needle would be nearly zero. Another issue is that the electronics of most thermal conductivity probes are not designed for such high temperatures and (from our experience) the real possibility of significant ground currents occurring during fires. On the other hand, it would be fun to consider designing and building an instrument to measure λ_s during fires.*

Reviewer - 10a L452: include info on how realistic it is that the sensor was indeed installed at this other depth. And how were sensors depths determined, before or after fire? How accurate are they?
RESPONSE - 10a *A Change was made. Lines 520-523 now read: "The (relatively significant) overestimation by nearly 60 C of the 2 cm soil temperatures is not fully understood, but it may be a consequence of mis-measurement of the installation depth of the soil temperature sensors and/or poor contact with the soil, meaning that the soil air in contact with the temperature sensor would be acting as an insulator relative to the heat conducting soil particles in contact with the sensor". I don't think further discussion of this issue is really warranted in the manuscript, especially with the additional comment about poor contact with the soil as a possible explanation for the underestimation of the model predictions. But, for the sake of answering all the reviewer's queries: (1) Installing these sensors by hand using a ruler (which is how I did it) is somewhat imprecise. I estimate the error associated with this approach to measuring the installation depth is ± 0.2 cm, meaning that I never really convinced myself that I could have missed the measurement depth by over half a centimeter. (2) We removed the sensors about 3 years after the burn and, as much as we could, we did try to check the installation depths of all the sensors. This did resolve a few measurement mysteries, but nothing regarding this particular temperature sensor. (3) The precisions of these sensors is less than 0.5 C, implying that it is unlikely there is anything inherently wrong*

with the sensor.

Reviewer - 10b Related to a previously posted reviewer comment: for accessibility and ease of use it would be helpful if the model input uses bulk density rather than porosity since bulk density is more easily available from soil databases. And does the model allow for bulk density to vary with depth? That would be useful given the typical increase of soil bulk density with depth.
RESPONSE - 10b *A Change was made. Lines 127-129 now read: "**(note: η is obtained from the soil's bulk and particle densities, which are the actual model input variables, and because both these variables are assumed constant and uniform, so also is η)**". The model is coded in such a way that it is fairly easy to include variable soil bulk density, but I have not created a specific option for inputing variable soil ρ_b profiles.*

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***** Response to Comments from Michael Novak dated 15 June 2020 *****

General comments

MN - 1 This manuscript is the third in a series on modeling soil heat and moisture dynamics under extreme surface heating associated with forest fires or prescribed slash pile burns. The first two papers (Massman, 2012, 2015) compared model results with a laboratory simulation of surface fire (Campbell et al., 1995). According to the Abstract the coupled heat and moisture model developed by the author is improved in two main ways: (1) the formulation for the non-equilibrium vapor source term, S_v, is modified for extremely dry soil moisture content and (2) the apparent thermal conductivity is no longer used in Fourier's law in the soil heat flow equation because that resulted in the double counting of thermal vapor transport (de Vries, 1958). Additional changes to the Massman (2015) model not highlighted in the Abstract are also detailed. The manuscript describes a slash pile burn field experiment carried out in a pine forest in Colorado and compares the latest model version to the field measurements (showing moderate to good agreement for the variables of greatest interest). Final sections discuss sensitivity analyses, the potential for future fire studies, and a summary.

RESPONSE - 1 *A change was made. As per anonymous referee #2 I have added a sentence (Lines 11-13) to the abstract to detail the additional changes to the Massman (2015) model that are not included in the first version of the abstract.*

MN - 2.1 Explicit formulation of an expression for S_v is required in so-called non-equilibrium versions of the Philip and de Vries (1957) theory, including those with the well-known extensions detailed in de Vries (1958, 1987*) and Milly (1982*). Until relatively recently all Philip and de Vries type theories in the soil physics literature were formulated with the assumption of local phase equilibrium between liquid water and its vapor throughout the soil domain even though it is recognized that for evaporation/condensation to occur in a soil pore such an equilibrium cannot be exactly true. In these equilibrium versions S_v is determined implicitly and only one partial differential equation, with corresponding boundary and initial conditions, is required to simulate soil moisture flow compared to two for a non-equilibrium version. Recently Smits et al. (2011) and Trautz et al. (2015) claimed significantly better simulation of cumulative evaporation and soil moisture content with similar non-equilibrium versions. Massman (2015) also made such a claim when updating the Massman (2012) model which assumed equilibrium. In the current manuscript it is indicated that the latest improvements to S_v "enhanced the stability and performance of the model" although neither the current nor the 2015 paper makes a direct comparison between equilibrium and non-equilibrium predictions. Novak (2019), which followed on Novak (2012), was devoted to a detailed investigation of the non-equilibrium formulation and the results of modeling moisture and heat flow with and without it.

RESPONSE - 2.1 *A change was made. In my 2015 paper I was basically referring to the 2012 version of the model as the equilibrium model. To compare the results of 2015 non-equilibrium model with those of the 2012 equilibrium model requires comparing the corresponding figures in each paper. But the 2015 model includes soil moisture hydraulic functions and the 2012 model did not. I should have given more credit in my 2015 paper for the improvement in the non-equilibrium (2015) model's performance to the inclusion of these hydraulic functions. Finally, I also note that others (Borujerdi et al. (2019) and Ouedraogo et al. (2013)) besides Smits (2011), Trautz (2019) and myself have also reported improved modeling results with a non-equilibrium approach and all of these models include hydraulic functions. Nonetheless, after considering M Novak's comment, I made several tests with the model and have modified* *the statement in the Abstract (Line 9) to read "made a small enhancement in the stability and performance of the model".*

MN - 2.2 It was mainly prompted by the realization that if recent claims about the importance of non-equilibrium are true then all previous investigations of soil evaporation (and other phenomena in soil physics) that assumed equilibrium are invalid.

RESPONSE - 2.2 *I am not sure how M Novak arrived at this conclusion. I don't see how the non-equilibrium approach invalidates over 60 years of scientific insight resulting from the equilibrium approach.*

MN - 2.3 The relevant Novak (2019) findings can be summarized as follows: (1) in a soil drying under natural conditions or due to fire evaporation begins at the soil surface but eventually moves steadily deeper into the soil with the phase change occurring within a very narrow frontal-type evaporation zone (at most a few mm wide), (2) a dry layer in which moisture flow is dominated by upward vapor diffusion exists above the evaporation front and relatively wet soil in which upward liquid flow dominates exists below it, (3) $S_v \approx 0$ everywhere outside the evaporation zone (especially true in the dry layer; a small amount of condensation driven by the soil thermal gradient exists in the wet layer but the values are several orders of magnitude smaller than those at the front), (4) there is no discernible difference for all dependent variables (and corresponding fluxes) between the non-equilibrium and equilibrium version models everywhere outside the narrow evaporation zone, (5) the intrinsic resistance (Shuttleworth, 1975*) associated with Hertz-Knudsen dynamics is 2–3 orders of magnitude smaller than the within-pore diffusion resistance that governs a proper physically-based formulation for S_v and can therefore be neglected, (6) avoiding the imposition of equilibrium at the soil surface during drying, which in principle is required within a fully non-equilibrium model, is more difficult in practice due to numerical instability; imposing equilibrium at the bottom boundary of the soil domain, if deep enough so that little change occurs, and in the initial condition is physically realistic.

RESPONSE - 2.3a *No response seems to be required here; but (a) The simulations of [Başer et al. (2018)] (see their Figures 12b and 12c) suggest that S_v can be non-zero (i.e., $\rho_v \neq \rho_{ve}$) for several centimeters in both the horizontal and vertical dimensions, contrary to M Novak's conclusion (3); (b) thanks for reminding me of [Shuttleworth (1975)]; (c) I also prefer a "physically-based" approach to formulating S_v; and (d) I am less certain than M Novak about what constitutes a "proper" (or by implication an "improper") approach to modeling S_v.*

RESPONSE - 2.3b *A change could be made & A change was made. But I think it would be helpful to discuss how I conceptualize S_v because it seems to be somewhat different than M Novak's. But first, M Novak and I do agree that S_v should be constructed as the product of a soil specific surface area (A_{wa} = surface area within a sample volume divided by the volume of that sample) and an exchange flux between the pore volume and the surface bounding that volume, F_{pore}. Novak (2012, 2019) models F_{pore} as a diffusive flux, i.e., $F_{pore} \approx D_e(\Delta\rho_v/L_p)$, where D_e is related to the coefficient of diffusion, $\Delta\rho_v/L_p$ is the bulk mass density gradient, L_p is some characteristic pore length scale and $\Delta\rho_v$ is the difference between the equilibrium vapor density, $\rho_{v,eq}$, and the pore vapor density, ρ_v. In my view S_v should capture other important surface-mediated effects than just specific surface area, so I model F_{pore} with the Hertz-Knudsen Equation. In fact, using the Hertz-Knudsen approach to describe surface-air interfacial interactions (and therefore potentially the interfacial intrinsic resistance) is common (e.g., [Shuttleworth (1975)], [Clifton et al. (2020)]). But my approach to modeling F_{pore} does not include any* *possible diffusive boundary layer effects, which could be an important consideration for advective flows. Rather my formulation of S_v does include an adjustable parameter, S_*, which because $0.01 \leq S_* \leq 0.1 < 1$ should act to compensate for the lack of any explicit boundary layer effects. I chose not to include possible boundary layer effects to help keep S_v as simple as possible. I am willing to include this clarification in the manuscript, if the editor or the reviewers find it helpful. But otherwise comparing the magnitude of the within-pore boundary layer resistance to any intrinsic interfacial resistance is well beyond the intent of the present study. Nonetheless, this issue did prompt me to make further model test runs, which led to the following change to the manuscript on Lines 171-172: "*$0.01 \leq S_* \leq 0.1 \ldots$*" .*

RESPONSE - 2.3c *Further change could be made. But there are other aspects to M Novak's model of F_{pore} that are worth pointing out here. [Skopp (1985)] provides a good summary of the basic assumptions and logic behind the diffusive model of F_{pore} championed by M Novak. (Note: [Skopp (1985)] develops a model of the uptake of oxygen by moist surfaces of soil particles. Nonetheless, the general approach he follows can be applied to S_v – or to any gas for that matter.) For the present purposes the salient assumptions for the [Skopp (1985)] model are that (1) F_{pore} is diffusive only (i.e., no advection is occurring) and (2) the pore gas phase concentration is at a steady state (i.e., that the storage term, $\partial\rho_v/\partial t = 0$). My model makes neither of these assumptions, which already suggests that the diffusive F_{pore} is not necessarily consistent with my approach. Of course, that does not preclude incorporating M Novak's model for F_{pore} into my model, but I chose not to pursue this in the present study. Furthermore, I have already proposed including a discussion of the boundary-layer effects associated with advective flows.*

RESPONSE - 2.3d *Further change could be made. But the steady state assumption is a different story. In his comment 8.4 below M Novak counsels that storage ($\partial\rho_v/\partial t$) may not be negligible during fires. My model runs suggest that it is important during fires, which is evidenced by the vertical profiles of soil vapor density and pressure (Fig. 1 below). I included similar plots in both the 2012 (paragraph [43] beginning with "Figure 8 ...") and 2015 (pps. 3671–3672 beginning at the paragraph starting with "Figure 7 ...") papers. So it seems reasonable to include Fig. 1 in the present manuscript. My concern in the two earlier papers (and shared to a certain extent in the present paper) is that the model predictions for vapor density and pressure seem higher than I would expect or intuit. I am willing to revise the manuscript to incorporate Fig. 1 and more discussion, if the editor and reviewers should find it helpful to do so. But there is more to this story as I point out in my Responses 3.1 and 3.2 below.*

MN - 2.4 The Novak (2019) results show clearly that the numerical implementations of the heat and moisture transport equations in Smits et al. (2011) and Trautz et al. (2015) were flawed and their conclusions about the importance of and the necessity to use non-equilibrium models were incorrect.

RESPONSE - 2.4 *I certainly did not conclude after reading Smits et al. (2011) or Trautz et al. (2015) that they had suggested that a non-equilibrium approach was "necessary" for modeling soil evaporation. I also observe that Novak (2012) and Smits et al (2012) debated some of the objections that M Novak had raised about the approach Smits et al. (2011) took when exploring the non-equilibrium model.*

MN - 3.1 Therefore, while the implementations of a non-equilibrium version of heat and moisture flow in Massman (2015) and in the new manuscript are not incorrect in principle, if done correctly, there is clearly no advantage to doing so despite the author's perception of better performance in both cases (never actually shown).

RESPONSE - 3.1 *I agree that my version of S_v is "not incorrect in principle". There are two improvements in model performance that resulted from what appears to be a relatively small change in the parameterization of S_v. The model was more robust to (or less prone to instabilities with) (1) increases in the time step and (2) changes in parameter values or functional forms of the relative hydraulic function, K_R. Both these are important, even if relatively small. But I don't think they are worth discussing.*

MN - 3.2 True differences between the equilibrium and non-equilibrium models that do exist within the narrow evaporation zone (as shown in Novak, 2019) are unobservable and have no bearing on the objectives stated in the Introduction to the new manuscript with regard to effects of fire on soil.

RESPONSE - 3.2 *A change could be made. My model explicitly includes vapor storage ($\partial\rho_v/\partial t$), whereas Novak (2019) assumes that it can be neglected and therefore implicitly assumes steady state conditions, i.e., $\partial\rho_v/\partial t = 0$. Allowing for storage in my model yields vertical profiles of vapor density that can approach atmospheric density at one standard atmosphere and vapor pressures that can exceed pressures of one standard atmosphere. Further these profiles extend well outside the evaporation zone and, therefore, they could (at least theoretically) be detectable outside that much smaller region. It is still on open question (at least to me) as to whether such profiles are physically realistic, a point I made in Massman (2015: pages 3671–3672) when discussing 2015 model's predictions. On the other hand, high values of ρ_v and e_v have also been reported with non-steady state equilibrium models of intense heating of permeable medium, e.g., [Dayan (1982)]. It seems to me that the only way to truly test for possible differences between equilibrium and non-equilibrium models during fires (and maybe under less extreme conditions as well) would be to assume non-steady state conditions and a Darcian velocity that could result from any rapid changes in vapor pressure gradient associated with possible rapid evaporation rates, e.g., [Udell (1983)], [Pakala and Plumb (2012)], and Fig. 1 below. One could also test for possible differences between steady state, but otherwise identical, versions of equilibrium and non-equilibrium models. M Novak's 2019 results and conclusions not withstanding, at this point I am not entirely convinced that the case for very little difference between equilibrium and non-equilibrium models has been conclusively proven (at least with applications to fire). Again I would be happy to include more discussion on this point in the revisions, if the editor and reviewers feel it would be helpful.*

MN - 3.3 Furthermore a non-equilibrium model requires greater programming labor, is more complex, has longer computation times, and has greater potential for programming error and numerical instability.

RESPONSE - 3.3 *Yes, the more complex the model the longer the execution times and the greater the risk of a programming error.*

MN - 4 The S_v formulation actually used based on the Hertz-Knudsen Equation [line 126, Eq. (8)] is incomplete since the much larger within-pore diffusive resistance that exists is neglected, implicitly resulting in a smaller resistance which would bring the non-equilibrium model used closer to the equivalent equilibrium model (Novak, 2019). Massman (2015) described two formulations for S_v, one of which was based on Novak (2012) which assumed pore diffusive resistance alone to be limiting to within-pore diffusion but the two formulations were not compared directly. An improved and fully physically-based version of the Novak (2012) model is derived in Novak (2019).

RESPONSE - 4 *No change was be made. As I discussed above in my Response 2.3b above the only extra resistance that might be missing from my model is a soil pore surface boundary layer resistance. Otherwise for the present purposes the principal difference between Novak (2012) and Novak (2019) is an improved* *definition of the transfer function he calls h_v. From my perspective this improvement in h_v and the concomitant improvement in M Novak's S_e (my S_v) results from a better description of A_{wa}. Otherwise as I see it Novak (2012) and Novak (2019) follow the same paradigm for developing F_{pore} and therefore there really is a minimal difference between the two Novak models. But as I suggest in Responses 2.3d and 3.2 above including or excluding the storage term is an important aspect of (and difference between) my model and Novak's model that maybe should be discussed in the revisions.*

MN - 5 The importance of changes to the S_v formulation [lines 144–154, including Eqs. (9) and (10)] to model behavior is difficult to assess because no information about the depth of and water content within the modeled evaporation zone is reported. The changes were apparently confined to $\theta < 0.01$ (based on $S_w \approx 0.02$ and porosity of 0.51) which if confined to the dry layer above the evaporation front would have no effect since $S_v \approx 0$ there. The temperature measurements and model calculations (Figure 6) show values well above boiling for the upper 5 cm of soil at least and the soil in that part of the dry layer has essentially zero water content. Line 150 indicates that S_v is changed only when at most a mono-layer of water exists on the soil particle surfaces. I would expect that little evaporation can occur from such surfaces and they must have been located in the dry layer. If this is true then explanation of the better model stability and performance due to the changes in S_v indicated in the Abstract is difficult to understand because the changes would have negligible effect.
RESPONSE - 5 *No change was made. I agree that very little evaporation takes place when $S_w \approx 0.02$ (or $\theta < 0.01$ for MEF soils). But my purpose was not to improve evaporation per se, but to allow condensation to proceed more naturally by removing the constraint imposed on condensation by the condition that soil moisture had to be present for condensation to occur. This is the basic physics and logic behind the slight change in $A_{wa}(\theta)$ to $A_{wa,dry}$ when modeling condensation. The consequences of this change to model performance is admittedly slight and certainly much smaller than the variations in the model's solution associated with alterations and sensitivity analyses of other model processes and parameters.*

MN - 6 There is no recognition of the issue of not imposing equilibrium at the soil surface (lines 236–243) in the most general version of a non-equilibrium model which requires a Robin type boundary condition for vapor density (see Novak, 2019, for a detailed discussion).
RESPONSE - 6 *No change was made.* ***(Personal Note - Can the word 'which' be dropped from this sentence?)*** *I am a bit uncertain in how to respond here. Novak (2019) says (within the first five lines after his Equation (29)): "However, simulations with this condition, although they agreed with the equilibrium solution to within about 5%, demonstrated significant numerical error for ρ_v and S_e near the soil surface. There was no time to investigate this further, so a more conservative surface boundary condition was adopted, $h_r(0,t) = h_{req}(0,t)$, with h_{req} given by (5), i.e., equilibrium was imposed at the soil surface."* ***(Personal note - Part of my confusion results from the impression here that M Novak is (or at least was in 2019) mystified by the numerical instability caused by not imposing equilibrium at the soil surface. Has he uncovered a reason for this instability since the 2019 publication?)*** *Assuming this quote from Novak (2019) is the origin of his present comment. Then, yes, I am fully aware that some care must be made when imposing the surface boundary condition on ρ_v. In my model this is accomplished principally by adjusting C_U and C_E of my Equation (17) to maximize the rate of evaporation without destabilizing the model. I mentioned this in my discussion after my equation (17). I did not dwell on this in the present manuscript, but this boundary condition-related-instability occurred in both the 2012 and 2015 models and*

was resolved in a similar fashion for both. Finally, all version of the model equate E_0 of Equation (17) = the flux at the atmospheric side with at the top the sum of the advective and diffusive fluxes on the soil side of the soil surface.

MN - 7.1 It is likely that the small effect that was found upon elimination of the double counting of vapor distillation (lines 155–196), reported in the Abstract as a major improvement to the Massman (2015) model, is due to the fact that the double counting was quantitatively important only within the narrow evaporation zone. Therefore soil temperatures in most of the domain are not affected very much.

RESPONSE - 7.1 *A change was made. Dropping the double counting of vapor distillation in λ_s also changed the relative weighting between conduction (λ_s) and the heat storage (C_s), so the effect extended throughout the model domain. But obtaining a comparable solution after dropping the double counting also required recalibrating λ_{m0} (Response 7.2 below). This then minimizes the difference between the model's performance before and after dropping the double counting. In the Abstract (Line 10) I now say that eliminating the double counting improved the physics underpinning the model.*

MN - 7.2 Other changes to soil thermal conductivity include accounting for a large effect (as much as a 70% decrease) of temperature on the thermal conductivity of the mineral component based on that of α-quartz although there is no discussion about the mineral fractions in the soil and whether they are dominated by quartz sand. The soil is reported to be 60–65% sand, 20–25% silt, and 10–15% clay. Do any other soil mineral components behave similarly to quartz? In Massman (2012) the large effect of temperature on volumetric heat capacity (due to both mineral and water component variations) is included. For thermal conductivity apparently only the dependencies of the water and air components on temperature are accounted for but not that of the mineral component. This is maintained in Massman (2015). For normal conditions these dependencies on temperature are usually small enough to be neglected but under fire they appear to be significant especially given that the thermal regime is of greatest interest. Modification of R_p in the Bauer term is reported but it is also indicated that the thermal infrared contribution to heat flow is negligible anyway.

RESPONSE - 7.2 *A Change was made. Except for his question, "Do any other soil mineral components behave similarly to quartz?", M. Novak's comment here are largely descriptive and I basically agree with his conclusions, except possibly for "For normal conditions these dependencies on temperature are usually small enough to be neglected".*

To answer his question Lines 209-215 of the manuscript now read: ***"At the Manitou Experimental Forest (MEF) burn site, and more generally throughout the region surrounding MEF, sand quartz is likely to be the dominant soil mineral, but other crystalline mineral forms, such as granite and feldspar are also common (e.g., [Retzer (1949)], [Mathews (1900)], [Smith et al. (1999)]). Although the thermal conductivities associated with these other mineral forms also decrease with increasing temperature ([Heuze (1983)], [Mottaghy et al. (2008)], [Miao et al. (2014)]), overall they decrease with about half the slope ($\partial\lambda_m/\partial T$) of the sand quartz parameterization used here. They also tend to show a much lower λ_{m0} than quartz sand. Here $\lambda_{m0} = 4.42$ Wm^{-1}K^{-1}, which should help accommodate these other non-sand quartz mineral forms."***

Regarding M Novak's supposition about the temperature dependencies of the soil's thermophysical properties at normal conditions. The sensitivity ($\partial/\partial T$) of this dependency can be estimated. From a review of papers and reports (some published, some unpublished, and including Massman (2012)) and *depending on soil type the volumetric specific heat for dry soil can increase by between about 7 and 25% when the temperature increases from 0-50 C, while λ_m decreases between about 10 and 25%. So for relatively dry soils these temperature dependencies may well be significant under more normal temperatures, especially as they act synergistically to widen the difference in how incoming energy is partitioned between storage (C_s) and conduction (λ_s). Therefore, I cannot conclude a priori that their combined effects can always be neglected.*

MN - 8.1 The large model complexity in this paper, which often requires referral to Massman (2012, 2015) to fully understand, makes it difficult to read and the meaning of the results somewhat incomprehensible.

RESPONSE - 8.1 *I have tried to eliminate some of this by including more references and discussion. But I also want to keep repetition to a minimum. Possibly I have been too aggressive with this. But as I have said in several places throughout this response that there are other revisions I am willing to undertake (but have not done so at this point) that may also help address M Novak's concern here.*

MN - 8.2 The physics of the various components is generally not new except for perhaps the non-equilibrium part which has been shown to be unnecessary.

RESPONSE - 8.2 *I agree that the physics is not new. But at this point I am not convinced that the "non-equilibrium part is unnecessary" (also please see my* ***Response - 2.3a*** *regarding the modeling results of [Başer et al. (2018)]). In fact I think exploring the differences between equilibrium and non-equilibrium models has brought further insights into the dynamics of soil evaporation, as well as an improved understanding (and performance) of models of coupled soil heat and moisture flow during fires (and maybe even to such models under less extreme heating). I think there is still some research that needs to be done on the role that the storage term, $\partial\rho_v/\partial t$, plays in modeling coupled heat and moisture flow in soils before concluding that "the non-equilibrium part has been shown to be unnecessary".*

MN - 8.3 I wonder whether including every process imaginable in such a model is warranted and even helpful given the uncertainties of fire under field conditions and associated measurement difficulties.

RESPONSE - 8.3 *I do not want to assume, a priori, that just because a particular physical process or parameterization is negligible under more normal conditions that it is equally negligible at very high temperatures. Unfortunately, instrument limitations and the associated uncertainties are always a hazard with observational field work. But this does not preclude me from comparing the model simulation with observations in an effort to evaluate the importance of physical processes that may be important under these extreme conditions.*

MN - 8.4 Despite the many processes the model still required a number of tuning parameters. Under normal field conditions many processes, e.g., storage of water vapor, advection of heat by air and water flow and water vapor by air flow, and heat of wetting are known to be of negligible importance and do not have to be included in coupled heat and moisture flow models (Grifoll, 2013*; Novak, 2010*, 2016*). This may not remain true under more extreme fire conditions but order-of-magnitude calculations could be used to determine this.

RESPONSE - 8.4 *Yes the model does include several tunable parameters. And I concur under more normal field conditions many processes are known to be negligible. But as I discuss in my Response 2.3d the storage term ($\partial\rho_v/\partial t$) may not be negligible when the soil is being heated during a fire. Otherwise I include many of these processes in order to test for their significance to the solution and because it is physically reasonable to do so.*

MN - 8.5 The upper boundary conditions are especially uncertain in this paper since they require proper understanding of how the slash pile burns including analysis of air flow within and above the pile. I am skeptical about the temperature boundary condition used in the paper. According to Figure 4 sensible heat flux was positive throughout the fire experiment while the picture in Figure 2 suggests a pile with internal temperatures of about 1000° C so that there likely was a steep temperature inversion above the soil surface and therefore the sensible heat flux must have been negative (downward). Despite this the modeled soil temperatures are reasonable, presumably a testament to empirical parameter tuning.

RESPONSE - 8.5 *A Change was made. Yes, the upper boundary conditions (in general) and the temperature boundary (in particular) are uncertain. And I emphasized this at length in Section 4.1 (sensitivity analysis for the upper boundary condition). But that said, I also pointed out in section 2.5.1 (complexities of formulating the surface energy balance and the upper boundary condition) that the upper boundary condition and the formulation of the surface energy balance may depend on the type of fire one is trying to simulate. Overall, I think there is more than enough evidence to support that this singular fire and data set cannot represent all possible fires and all possible boundary conditions. Nevertheless, I can still test the performance of these boundary conditions using this pile burn data set. (Note: Please see my Response - 11 below because I did add two more photographs of the fire and some further discussion on formulating the upper boundary conditions for this fire.)*

But for further clarification, I have added the following two sentences (Lines 296–300) to the manuscript: ***"Further complicating a proper formulation of the soil surface IR and convective heat flow terms are the structure and porosity of the pile, which will influence the IR impinging on the soil surface, the air flow ventilating the pile interior, and the size and efficiency of any convective eddies that may act to transfer heat to or away from the soil surface. In this study the upper boundary condition for temperature ($T_a(t)$ and C_H) is chosen to insure that the convective heat flow is away from the soil surface."***

MN - 8.6 Actually since surface temperature was measured in the experiment it could just as well (or even better) have been used as the upper (Dirichlet) boundary condition to the heat flow equation.

RESPONSE - 8.6 *A Dirichlet upper boundary condition has always been a possibility. But to date I have chosen not to explore this approach.*

Specific comments

MN - 9 What is the physics underlying Eq. (5) (line 117)? Is it Stefan flow? Grifoll (2013*) included not only Stefan flow in his heat and moisture transport study but also the mechanical dispersion associated with it. He found that this dispersion affected vapor flow in the dry layer although the effect on evaporation rate from the soil was small. The physics he described was sound but the effect of dispersion was probably exaggerated because the assumed longitudinal dispersivity was likely too large. Eq. (5) is similar to a differentiated form of Eq. (12) in Grifoll (2013*).

RESPONSE - 9 *A change was made. The physics underlying Eq. (5) (line 117) is (as stated in the present manuscript) that "u_{vl} (m s^{-1}) is the advective velocity induced by the change in volume associated with the rapid volitalization of soil moisture". This is not necessarily a diffusive or Stefan flow advective velocity. Fundamentally it is more like an explosion. The current u_{vl} is an update of the model parameterization used by Campbell et al. (1995). In Massman (2012, 2015) I referenced [Ki et al. (2005)] for further details. I have added this reference (Line 133) to the present manuscript, which now reads "u_{vl}*

($m\ s^{-1}$) is the advective velocity induced by the change in volume associated with the rapid volitalization of soil moisture ***(e.g., [Ki et al. (2005)])***, *...". That said I am not completely satisfied that this is the best or most appropriate model for u_{vl}, which is why I included the discussion of a different formulation for u_{vl} in Section 5 "Improving physical realism: ...".*

MN - 10 It is well known that the greatest limitation to evaporation from soil is soil hydraulic conductivity (Hanks and Gardner, 1965*; Grifoll, 2013*). For the standard environmental temperature range (10–60° C) both the hydraulic conductivity and soil water retention curves vary significantly with temperature (the former through the viscosity of water and the latter through surface tension; however measured retention curve variations are usually larger than expected from surface tension alone which is not yet understood) Accounting for these temperature variations in actual modeling is not always important (Milly, 1984*). There is no indication whether the temperature dependencies of the hydraulic properties (lines 303–341) are included in the model. For the larger temperature range under surface fire it might be important.

RESPONSE - 10a *A change was made. With regards to the hydraulic functions, the temperature dependency of the viscosity of water (μ_w) is included in the present model. So the hydraulic functions are temperature dependent and I noted this on the line following Equation (3) of the manuscript, which states: "where $\mu_w = \mu_w(T_K)$". The Line 118, which follows Equation (3) now reads "$\mu_w = \mu_w(T_K)$ (Pa s)* ***[Huber et al. (2009)]***". *But unless film flow is included in the hydraulic functions (see section 2.2.5 (page 3666) of Massman (2015)) the hydraulic functions do not include surface tension (σ_w) effects. The inclusion of film flow had little impact on the present model's performance or any resulting simulation. This is the same thing I found in Massman (2015), so I did not discuss film flow in the present study.*

RESPONSE - 10b *A change was made. With regards to the WRC, I agree that it is worthwhile to investigate the possible influence of temperature on the WRC. So I have added the following paragraph to address this.*

Lines 384-396 now read: ***Additionally, temperature can also have significant effects on the WRC (e.g., [Olivella and Gens (2000)], [Salager et al. (2010)]), a consequence of the influence temperature has on the surface tension of water ($\sigma_w = \sigma_w(T_K)\ Nm^{-1}$) [Vargaftik et al. (1983)]. Massman (2012) found that these effects were relatively small, even at very high temperatures, but his 2012 model did not include any possibility of soil water movement (i.e., no hydraulic functions). [Milly (1984)] includes the temperature effects on both the WRC (σ_w) and the hydraulic functions (μ_w) and likewise found little effect, but his study was restricted to relatively normal soil temperatures (i.e., $T \leq 60$ C). Here the impact this temperature dependency (σ_w) has on the WRC and the model solution was investigated by multiplying the model variable ψ_n of Equation (19) by the function $\phi_n(T_K) = \sigma_w(T_{K,in})/\sigma_w(T_K)$, which ensures that $\partial\theta/\partial T_K < 0$ in accordance with observations (e.g., [Olivella and Gens (2000)], [Salager et al. (2010)]) and that ϕ_n is consistent with theoretical considerations and similar to other model parameterizations (e.g., [Milly (1984)], [Zhou et al. (2014)]). It also ensures that $\phi_n = 1$ at the beginning of the model simulation. The results suggested that temperature has only a minor influence on the WRC and the model solution and because the model's sensitivity to ϕ_n is small compared to the model's sensitivity to changes in other parameters and parameterizations, this issue will not be considered any further in this study.***

MN - 11 No qualitative indication is given as to how the fire evolved over time (lines 361–374), i.e., for how long the fire burned intensively as in Figure 2. Was there a period in which just glowing ashes existed?

It would be useful to the reader to have a feeling for what happened. A time series of photos would be ideal.

RESPONSE - 11 *A change was made. I added two more photos of the burning pile: Figs. 3 and 4 of the revised test. Lines 433-436 now read:* ***Figures 2, 3 and 4 are a sequence of photos taken during the first 3 hours or so of the burn. Figure 2 shows the burning slash pile about 25 minutes after the fire was initiated and shows the deployment of the data loggers, CO_2 pumps and analyzers, and the supporting infrastructure. Figure 3 shows the burning slash pile about 75 minutes after initiation and Figure 4 was taken about 195 minutes after initiation.*** *I also changed the caption on Figure 2 of the present manuscript to read "Layout of the data system and slash pile of the Manitou Experimental Forest 26 April 2004 experimental burn* ***about 25 minutes*** *after ignition."*

A related change was also made. Lines 499-506 now read: "These apparent limitations of the simplified surface energy balance, Equation (18), do not lessen the argument that it may be appropriate for some slash pile burns ***or any time burning material is in direct contact with the soil and the concomitant soil heating is overwhelmingly conductive****. Rather these present comparisons suggest that a hybrid of Equations (18) and (16) may be more appropriate. Such a hybrid would employ Equation (18) in the early part of the burn (before significant loss of mass from the slash pile due to combustion) and Equation (16) later after the fire intensity has peaked (i.e., sometime after t_d) when the soil surface or possibly an ash surface is more exposed to the ambient environment.* ***The time course of the experimental burn shown in Figs. 2, 3 and 4 are consistent with a hybrid formulation of the boundary forcing. Nonetheless, it is beyond the intent of the present manuscript to explore this concept any further.****"*

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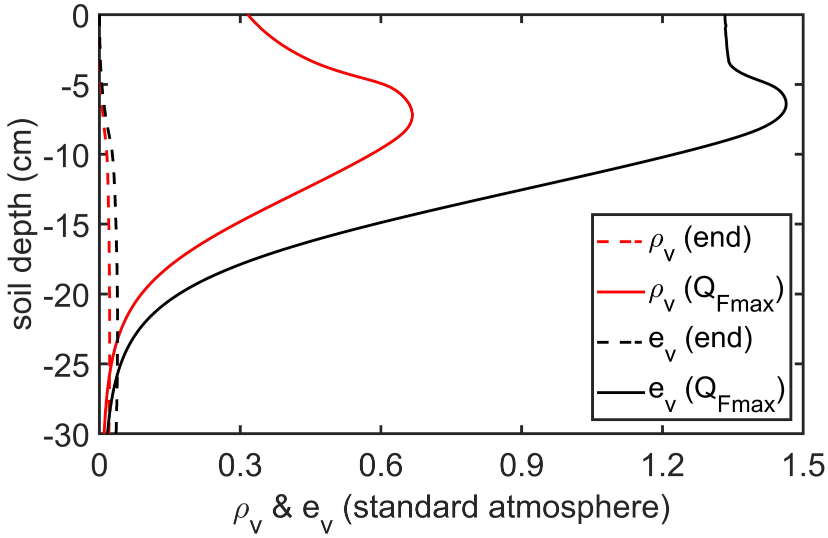

Figure 1: Vertical profiles of modeled soil vapor density ($\rho_v/1.292$) and pressure ($e_v/101325$) in units of standard atmosphere. Profiles correspond to the final profile at the end of the simulation and at the time, t_m, that the forcing, $Q_F(t)$, reaches its maximum.