

Interactive comment on “Seasonally frozen soil modifies patterns of boreal peatland wildfire vulnerability” by Simon J. Dixon et al.

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Review; Seasonally frozen soil modifies patterns of boreal peatland wildfire vulnerability. Abstract: Submitted Interactive comment: GENERAL COMMENTS The discussion paper considers the role of evaporative drying of peatlands with an underlying shallow frost layer. The focus of drying is upon surface peat soils becoming dry enough that they can reach a smouldering threshold moisture content. The paper explores this using modeling of planar and hummocky peatlands. The subject matter is of interest and does have value with understanding hydrology of any frozen soil system. That of just the frost layers potential role in limiting moisture transfer from deeper parts of the soil and resulting in drier conditions is of interest (unless it has been explored by others for organic soils). Although this is a modeling approach it can help explain and

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further direct interpretation of field data. The paper does require changes in organization, method explanation, and technical corrections to properly present and understand their findings. Overall I view these changes as major as it is difficult to assess the paper's outcome at this stage. To improve the paper the following need to be considered: use of terms needs to be standardized; assumptions need to be clearly stated at the beginning; the methods section describing models needs to be reorganized and information added; why a flat frost layer is used beneath hummocky terrain rather than a layer that follows surface contours; why both volumetric and gravimetric water contents are used; why it is necessary to use 'probability' instead of just water content; and addressing possible issues to do with numerical boundary errors between PR and WC domains.

Title is a bit misleading as the focus of the paper is upon how frost layers affect surface moisture contents. Low surface moisture contents in turn affect smouldering vulnerability. It is suggested that somehow the title have more a focus upon moisture contents.

Description of the model is scattered throughout first part of paper making it difficult to follow and understand. Some of this scatter was made necessary by having to first define terms, especially 'water conserving' and 'productive'. It would be more clear for the reader if the hydraulic properties (2.3.4 lines 191- 231) were first given then the model description (dimensions (section 2.3.2 Lines 133 to 154) and design (2.3.5, lines 234-248) were combined), then followed by model boundary and initial conditions (2.3.3, lines 155-231). Also use of terms describing model, especially regular model is confusing and it is suggested that the term 'planar' be used replacing 'regular' and 'topography excluded'. It is also not clearly understood what the initial moisture contents of the models are. It appears that the peats are initially saturated throughout – this is critical for the reader to understand – and that 'water table', as used by the authors, is synonymous with 'saturation'.

Description of where frost layer is located and type of frost layers tested is entirely missing in the methods section. Rational for using a 'planar' frost table (shown in Fig

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4) rather than a frost table that is of constant depth beneath the ground surface must be given.

364-366 and 386-392: not clear how lateral water transfer results in zones of low GWC at the example A interface, which is the boundary between the PR and WC peat types. If water transfer was occurring then water would move from low soil water suction (high water content) to high soil water suction (low water content) due to an hydraulic gradient. A vertical 'tongue' of very low GWC extending downward in the WC peat type would not be expected. The authors must discuss that this is not an artifact caused by numerical calculations at model boundary conditions between the PR and WC boundary. If the density of peat is constant with depth and type then the hydraulic gradient has to have flow going from 'wet' to 'dry' thus flow could not occur from the WC side of the hummock to the productive hollow.

Assumptions and information on evaporation stated in lines 463 to 482 is informative. Some of the assumptions should have been clearly stated within the methods section

MORE SPECIFIC COMMENTS Initial confusion with effect of frost layer. At first it is implied to the reader that the frost layer can limit upward capillary flow from the water table (Line 66), but later it appears that it might also include the recharge of soil water to the evaporatranspiration needs of the layer above as the frost layer itself thaws and frees up water (Lines 175-179). In discussion of the results it is apparent that the frost layer plays both roles. These roles should be clearly stated within the first part of the paper.

Frost layer relative to water table and peat moisture content. It is not clear what the moisture content of the frost layer is. It appears that it forms within the water table and thus would be saturated. This needs to be clearly stated within Methods. If it is saturated then one can assume that it is 'relatively impermeable'.

Surface water tension. The use of this term is confusing, however if this term is preferred by the journal then ignore this comment. First 'tension' implies water potential

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relative to atmospheric and thus in the context of unsaturated soils 'tension' should be accompanied by negative values (e.g -400 mb). The second source of confusion is the use of 'surface' which can be misleading with 'ground surface', 'evaporating surface' or 'water table surface'. It is recommended that the authors use the term 'soil water suction' in its place as 'suction' has the negative term within the word. This will also avoid further confusion for such instances as line 335 "near-surface water tensions"

Use of VWC, VMC, and θ to represent volumetric water content. It is suggested that where possible only θ be used, however as this is a symbol and for sentence structure purposes VWC could be used.

A definition of 'smouldering' would help understanding of article by readers not familiar with peatlands (lines 41-51). I had to look it up to understand the issues see Rein et al 2009: "Smouldering is a flameless form of combustion, deriving its heat from heterogeneous reactions occurring on the surface of a solid fuel when heated in the presence of oxygen [3]. The fundamental difference between smouldering and flaming combustion is that in smouldering, oxidation of the reactant species occurs on the surface of the solid rather than in the gas phase. The characteristic temperature, spread rate and heat released during smouldering are low compared to those in the flaming combustion of a solid. Smouldering fires in forest biomass propagate on average at around of 10-30 mm \cdot h⁻¹ and the peak temperature is around 550-650 °C [4]." (p2).

Smouldering threshold. A threshold GWC of 250% is given (line 263). As this is a key value to discussion a reference or two supporting this threshold would strengthen its use.

GWC as a % or a ratio. As GWC is defined by equation 5 (252) as a ratio its use throughout the paper should be as a ratio and '%' values avoided.

Density value used for peat in models. As both VWC and GWC are used interchangeably throughout the paper it would help the reader if the dry bulk density of peat was given. Clarify whether one value was used for all models and depths or several or a

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range. For equation 5 (252) clarify that this is the dry bulk density used. Also correct conversion should also include the density of water in the equation and in the calculations – especially if kg m⁻³ are being used as units.

Use of terms to describe the ‘regular’ or ‘planar’ peatlands. This changes throughout the paper and makes it very confusing to follow. Objective ii) (lines 87-89) does not state the type of peatland topography however throughout the rest of the paper refers to objective ii) as being the ‘regular domain’ (134, 156, 237), a ‘planar surface model’ (157), and as ‘topography excluded’ (294). It is suggested that ‘planar model’ be used as ‘regular’ and ‘topography excluded’ are not descriptive enough. This term should also be used in objective ii).

Use of term ‘microtopography’ to represent hummocky peat terrain. This is less confusing however as it represents objective iii) (89) it needs to be clearly stated in objective iii) and defined so that ‘peatland microforms’ is understood by the reader to represent ‘microtopography’, or hummocky terrain variations of 0.4 m in amplitude and 2 m in wavelength. If ‘microtopography’ is to be the term of choice then it should be used consistently throughout and not other terms (e.g., ‘topography included’ line 348).

Consistency with use of other terms throughout paper; e.g., ‘seasonal ice dynamics’ (70), ‘seasonal frost layers’ (76), ‘near-surface frozen soil thaw dynamics’ (79). ‘depth to ice’ meaning ‘depth to frozen soil’ (86) 105: ‘no ice’. Is ‘ice layer’ different from ‘frost layer’?

139: soils tested in McCauley et al 2002 were mineral soils (highest om content for one was 9.5%) and hydraulic conductivity was measured with liquid fuel. A reference with peat soils would be more appropriate or mention the limitations of this reference. Of interest with McCauley et al (2002) is that K was measured at different frozen moisture contents thus showing K was a function of degree of ice-saturation. If the frost layer of the models was saturated or near-saturation then permeability can be assumed to be very low.

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Does using the metal probe to indicate frost layer only work if the peats are above a certain moisture content?

140-142: clearly state that ‘rectangular geometric objects’ and ‘inactive objects’ refer to the frozen layer. This is confusing as the terms ‘layer’, ‘cells’, and ‘objects’ are used to mean the same thing

143, 146, 158: upon first use define evaporating surface (rather than at line 169) and locate in z plane. Does it always coincide with the ground surface? If so then perhaps state this

158: for the starting water table depth would the evaporating surface always coincide with the ground surface?

188: by ‘planar’ water table depth within the microtopography model this means it is ‘flat’ and given a depth of 0.04 m from base of hollows it would thus be 0.44 m deep from hummock tops? If so then add phrase “..0.04 m from base of the hollows and 0.44 m from hummock peak and higher..”

Consider showing a figure (with dimensions) that show the planar and microtopographic domains with labelling demarcating water conserving hummocks and hollows from productive hummock and hollows and the different frozen soil geometries. This would help readers better visualize the models.

204: try to improve upon definitions or terms used to describe WC and PR. WC: ‘peat profile displayed high surface tensions under evaporation’ high surface tensions = high suctions meaning the peat dried out??. PR: ‘able to maintain low surface tensions during evaporative stress’ meaning low suctions and the peat remained wet?? I did find the terms ‘water conserving’ and ‘productive’ not intuitively representing what these peats. Why ‘water conserving’ if the peat dries out? Why ‘productive’ if the peat conducts water and remains wet?

Do not understand the need to show probability of ignition vs time (eg fig 5) and Mean

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GWC vs time figures when they are both the same (shapes are the same)?

TECHNICAL DETAILS

45-46: Prat-Guitart et al., 2016 should be cited last in the list citations to keep with chronological order established in rest of article

131-132: Šimůnek et al., 1999?

154: space between 1.4 and 'm'. Check throughout paper as there are other instances.

167: add a reference for 'Environment Canada, 2017'

199: citations as part of the sentence (as opposed to within brackets) are now italicized where as the few before (e.g. lines 101, 121) were not. Also square brackets [] are now used rather than rounded (). Check throughout paper

203: Kettridge et al. missing a period and the 2015 is missing an 'a' or a 'b'.

216: Quinton et al missing a period

219-221: explicit definition of 'water conserving' and 'productive' should be before use in lines 204-205

231: perhaps table should clarify what the models are with each material; it is not clear if 'mean', 'water conserving', and 'productive' are associated with the planar model or not?

253: specify whether ρ refers to particle density or bulk density (and if bulk density whether it is wet or dry). By unit analysis it will have to be dry bulk density. Also specify that θ is VWC. To make Eq 5 correct with regards to dimensions the density of water term must be included: $GWC = \theta w/b$, where b is dry bulk density

261: clarify symbols in Eq 6 and 7 by providing definitions in text of μ_p (mean peat density?) and σ_p (standard deviation of peat densities)

263: should use GWC as a ratio throughout paper not a percentage. This gets confus-

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ing for use of 'x' in equation 7.

265: is x expressed as GWC in kg/kg?

281: to avoid confusion it should be stated that 'moisture recharge' is referring to moisture released from the ice as it melts (lines 175-177) as opposed to other sources of recharge; i.e. rainfall (line 187) or capillary rise.

299: as figure uses hours then use hours in text to help reader; e.g., "after 72 hours (3 days)..."

300: "...after 168 hours (one week)..."

306: "day 7 (hour 168)..."

299: 307: Figure 2: difficult to visually differentiate 'productive' from 'mean' lines at 100% screen magnification.

308: The 0.20 m depth refers to top of the frost layer? Why is this different from 0.15 m depth used for the planar model (line 141)?

314: as figure is in hours suggest the following phrasing 'After three weeks (504 hours) of ...'

308-309. Is this figure just for the microtopographic model? Or does 'mean' refer to the planar model? Clearly state within Figure title and within text discussing figure.

316-332: not clear whether this section is this referring to planar or microtopographic models.

322, 325, 328 what is meant by Figure 3b/c, 3bc? Figure 3b,c?

324: as θ is VWC just state "... $\theta \approx 0.50 \text{ m}^3 \text{ m}^{-3}$."

342: Figure 3. State whether the figure is for planar or microtopographic model. As model dimensions are 5 m why is 4 m used here?

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342: Figure 3. Shading used makes it very difficult to discern θ values between 0.3 and 0.5. Could use 3 colors in shading (as done with Fig. 4) rather than just 2. Why is Fig 3 as volumetric whereas the rest of the paper and figures discuss gravimetric?

351-352: remove the 'and' in the sentence to improve sentence structure.

353: Figure 4. Gravimetric color bar in figure should be expressed as a ratio not a %

353: Figure 4. Ice layer depths and configurations are not described in the text. Within figure it is apparent that they are all planar regardless of surface topography – needs to be discussed why this was assumed as opposed to having a set depth beneath the surface, which would be more reflective of field conditions.

353-362: to avoid confusion change the top titles of the figures from 'after 2 weeks' to '2 weeks' and '5 weeks'. Then for line 358 remove the 'after' so it reads "scenarios at two weeks. For the results at five weeks,..."". Otherwise the reader is considering a time period following 2 or 5 weeks.

367: as there are no figure sublabels within Figure 4 it is difficult to discern what 'Figure 4a/b' refers to.

383: Figure 4 does not show ice scenarios at three weeks.

394: use the same terminology throughout the paper for 'discontinuous' frost layer. Figure 4 used 'discontinuous' thus Fig 5 should not use 'blocks' 395: figure is titled 'probability of ignition' which refers to equation 8 (line 269). This is misleading and that of what the equation represents 'probability of the gravimetric water content being lower than the threshold for smouldering' (lines 262-266) would be more accurate.

402: might have missed this before but state what peat density (or densities) were used in the model – or at least state if only one peat density (by depth and peat type) was used or a range.

424-431: specify that this is for the planar model. This series sentences are confusing

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as it starts by stating for the 'presence or absence of a frost layer', however fig 2 is just for a solid layer frozen conditions and peat DID NOT remain saturated over multiple weeks of evaporation for the productive domain (difficult to tell the lines apart)

431: do you mean Figure 1?

435: sentence is awkwardly phrased

463-473: some of this information; fixed evaporation rate of 4.5 mm/d should be stated in methods.

540: '2015a' should be cited before '2016'

627: volume and page numbers needed for Kettridge et al., 2015a and 2015b. Kettridge et al 2015a was published in Ecohydrology in 2016.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-678>, 2018.

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