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Discussions

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

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I read the paper by Dixon et al. entitled "Seasonally frozen soil modifies patterns of boreal peatland wildfire vulnerability" with considerable interest as it purports to couple frozen soil dynamics with fire vulnerability in boreal ecosystems. Without a doubt, this is an area requiring further understanding as the coupled heat-mass transfer in organic soils is vexing.

Unfortunately, I found this paper fundamentally flawed on a number of grounds. It has a large number of boundary and initialization conditions that do not make physical sense. The parametrization of frozen ground is unrealistic on many fronts. While I do not want to be overly negative, I present the authors a number of concerns and believe that this

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type of paper would do more harm than good and be misleading to others as it is more an example of running Hydrus-2D than realistically simulating and understanding this system.

1) Hydrus-2D does not simulate freeze/thaw dynamics. There are several models that do. Having frozen ground simulated as static 'blocks' that do not move for up to 5 weeks is completely unrealistic, misleading, and ignores decades of research on heat and mass transfer in frozen soils. What the authors do is introduce and aquitard at some specified depth and keep it static for the period of simulation. There is no consideration of frozen ground physics, two-directional thaw, etc., despite dozens of papers on this regard. I think most people realize the role of aquitards in limiting water.

2) The paramterization and boundary conditions are incredibly unrealistic. How can we understand anything about this dynamic and coupled system when the appropriate thermal setup has not been accounted for? If in the spring the frost table was at the surface and allowed to descend, and the heat/mass transfer be accounted for, that would be of value and interesting results related to changing moisture conditions with thermal evolution (which would be heterogeneous because of variable soil thermal properties). There are models that do this computationally intense work.

3) The peat properties do not vary with depth. The authors cite one of their own studies to say this is true, but this ignores dozens of papers, and many in this environment that state otherwise. The Quinton et al. reference is misquoted and misinterpreted. Depth-dependent hydraulic and thermal properties are critical in understanding this process and the role of ice on moisture contents. This is ignored.

4) The upper boundary of 4.5 mm/d evaporation is unrealistically high. For 5 weeks? With no change in the geometry of frozen ground? At this point, I am unsure if this model has any grounding in all in reality the good fieldwork that has likely occurred in this area.

5) No rainfall for 50 days? This is unreasonable and likely imposed to 'prove a point'

with the model.

6) The rationale for setting conserving and productive peats below different hummocks/hollows is not justified.

7) The moisture recharge rate of 1.8 mm/d from the frozen soil layer is strange. Where is this moisture added to the domain? At the lower boundary? The upper boundary? At the aquitard. This is unclear and regardless incorrect without a moving boundary.

8) The 'continuity of frost layer being only important for productive peat' is an artefact of the model setup, not a real conclusion.

9) Is there any model validation?

10) Figure 5 and 6 looks like a screen cap and the quality should be improved.

In the end, I could comment on the interpretation of the results, but the results of this modelling exercise are not validated, the model setup is unrealistic, and all conclusions are dubious and speculative. I caution the authors to carefully consider these comments and whether this research is to 'prove' a clever point with an inappropriate model setup or truly trying to understand this system and its vulnerability to fire.

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