Reviewer comments in bold, responses in plain text.

Reviewer 1 comments

This is an interesting and concise paper that proposes a compact method to evaluate the capacity of land surface models to represent the effect of snow inflation on the underlying soil. I have no doubt that the proposed metric, with some little changes proposed in the following, will be widely used. The paper is yet another illustration why the first author's recent passing away is a huge loss for the scientific community. The figures are all relevant and easily readable. Relevant scientific literature is appropriately referenced. No unnecessary detail clutters the simple and clear message of the paper.

We thank the reviewer for his comments. We agree that the simplicity of the metric is one of its strengths. We also appreciate the comments about Drew's passing and how his passing is a huge loss for the scientific community. We couldn't agree more.

This work should therefore be published after a few minor changes suggested below.

Specific remarks.

- Page 2, line 12: The primary motivation is certainly a good representation of soil temperatures. One could add, however, that wrong temperatures at the snow/soil interface, caused by wrong snow conductivity, can feed back on the snow pack itself via a modified snow metamorphism (in cases models do simulate snow metamorphism dependent on temperature or vertical temperature gradients).

This is a good point. We have added the following sentence: "Additionally, biases in the simulated temperature at the snow/soil interface can adversely affect the snow pack itself though the impact of these biases on snow metamorphism at the base of the snow pack."

- Page 2, line 26: There is a little incoherence that could be acknowledged: The theory presented here initially supposes a periodic (sine) air temperature signal; however, the theory is then limited to the "cooling season".

This is correct. We believe that the text is already relatively clear on this. E.g., we note: "The above theory is adapted to the cooling period of the year, defined here as October to March."

- Page 3, line 3 : "2m air temperature serves as a sufficient proxy as the two quantities tend to equilibrate towards each other, particularly in colder months of high latitude regions with low solar input": Yes and no: In some cases (strong inversion), temperature difference between the snow-air interface and the air at 2 m height can be substantial.

True. There is some error associated with differences (positive or negative) between air temperature at 2m and the temperature at the snow-air interface. We believe that the errors associated with this discrepancy have less of an impact on th metric than errors in the measurements themselves. But, we do now include this statement to acknowledge this point. "The actual land surface temperature is rarely observed *in situ*, but 2m air temperature serves as a sufficient proxy as the two quantities tend to equilibrate towards each other, particularly in colder months of high latitude regions with low solar input, though in situations with strong inversions, the temperature difference between the snow-air interface and 2m height can be substantial (this is an acknowledged, yet unavoidable limitation)."

- Equation 3: Why not use immediately A_0 and A_z instead of introducing new variables A_air and A_soil which are not really used?

Certainly, one could use A_0 and A_z directly, but we feel that it is actually easier to understand what A_norm is the way it is presented so we have elected to maintain as in the original document.

- Equation 6: The general form of this equation, in particular the numerator of the right hand side, makes sense, but the specific form of the denominator does not. The denominator (which is a constant) should be chosen such that if snow depth is constant (i.e. all snow falls in October), the efficient snow depth is equal to this constant value. Therefore the denominator should read: \sum\limits_{n=1}^{M} n (or (M+1)*M/2, which is equivalent). For the case of the blue curve in figure 1, which is apparently S(i) = i*0.1 (with i=1 for October and i=6 for March), this would yield S_eff=0.266 m, which is less than the average depth of 0.35 m. This would make sense; in Figure 1, for the same case, S_eff is higher than the simple time average, which is incoherent. By the way, I have the impression that equation 6 is not what is plotted in Figure 1. In any case, the difference is only a constant factor, so this has no important effect on the results presented in the rest of the paper. But I think that the definition of S_eff should make immediate sense for simple cases. Right now, it does not.

Agreed. We have fixed the equation and replotted Figure 1, Figure 3, Figure 4, and Figure 5

- Equation 6: What would the results look like if the time period considered would be limited to the period before substantial snow melt occurs? In southerly areas, it can already melt in March. Does this introduce noise?

We tested with various limits to cooling season and the results are qualitatively similar.

- Page 5, line 12: Would it make sense, and would it change the results, to offset the snow depths by adding a positive constant corresponding to a slab of snow with equivalent thermal insulation as 20 cm of soil?

We do not think that this would add any value. There is an offset in the thermal insulation at 0 effective snow depth that represents the thermal offset between air temperatures and 20cm soil temperature. We feel that the way the results are presented now make this clear and that doing something like 'replacing' the snow with a slab of snow would reduce clarity.

- Page 6, line 9: Some models have a vertical 'soil' axis that comprises the snow. That is, 'soil' depth is not counted from the soil-snow interface downwards, but it starts at the snow-atmosphere interface. That could explain some very far off outliers.

We agree and we have already noted this in the discussion. "Incorrect snow heat transfer curves are symptomatic of model deficiencies. As an example, the land scheme in the Hadley Center models used here [MOSES2.2; Essery et al., 2001] applies a composite snow model where the top soil layer and snowpack share the same temperature [Slater et al., 2001], hence insulation is not properly accounted for and cold temperatures easily penetrate into the soil."

- Page 6, line 20: Yes, but the initial argumentation says that the metric presented here is valid in the case when there are no phase changes. (But the argument is correct nevertheless)

That's correct. We have elected to remove this statement because it is not a deficiency that is relevant to the snow insulation and therefore is outside the scope of this paper and has been noted previously in other studies.

- Equation 7 is not particularly elegant. It must be artificially limited to exclude values below 0. A more elegant definition could be: SHTM= SUM(MIN(A_{norm,obs,i},A_{norm,mod,i}))/SUM(MAX(A_{norm,obs,i},A_{norm,mod,i})) This would automatically yield values between 0 and 1 because A_norm is always >=0. Other rather natural and coherent forms for the RHS of equation 7 can be easily defined.

True. Alternative forms of this equation could be implemented, but this is the form that Dr. Slater implemented and we prefer to leave this as is.   Alternative forms of the equation, while potentially more elegant, will not yield anything different in terms of results.