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Interactive comment on "A vertical representation of soil carbon in the JULES land surface scheme with a focus on permafrost regions" *by* Eleanor J. Burke et al.

Anonymous Referee #1

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The paper by Burke et al describes the results of including a vertical dimension to the soil carbon cycling in the JULES model, with particular application to the cycling of carbon in permafrost-affected soils. Including some representation of the vertical carbon dynamics is an essential prerequisite to consideration of permafrost carbon feedbacks and thus represents an important evolution in the terrestrial carbon models used in ESMs. The only real downsides to doing so are (a) the increased complexity of the model, with new uncertainty on things like vertical soil carbon transport and an increased sensitivity to soil climate below the surface, and (b) some more complex logistics with respect to spinup and how to treat the initial conditions which may contain remnant carbon from prior climate states. Since both of these represent real uncer-

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Discussion paper

tainties in the high latitude system, and since the single layer approach that has been almost universally used until recently leads to a biased set of outcomes to the carbon-climate feedback prediction, formally including these uncertain processes is warranted in the models. This manuscript is a nice description of the methods that have been followed by the JULES group–one of the more widely-used ESMs globally–in making this change to vertically-resolved soil carbon schemes, and I thus recommend its publication.

Some specific comments questions below:

equation 6 and related text: if s is the unfrozen soil moisture, does that mean that, when soils freeze, s drops below s_{min} and therefore $F_s = 0.2$? If so, then that would contribute a fairly strong indirect temperature sensitivity via the soil moisture mechanism in frozen soils, which would be multiplicative of the direct temperature sensitivity, and so this ought to be noted here. This is how the freeze inhibition works in some other models as well, e.g., CLM, and is a reasonably mechanistic way of including the direct freezing inhibition. Though if that is the case here, I'd suspect that the minimum $F_s = 0.2$ parameter exerts reasonably strong control on the permafrost carbon stocks, at least in the Q10 case where respiration rates are otherwise nonzero in frozen soils, and so you may want to include that in your sensitivity analysis.

Intro to section 2.3: Does the prognostic soil carbon described in this manuscript feed back on the physical parameters from the Chadburn et al organic soils treatment? I am suspecting not, as you state there are no feedbacks from the soil onto the rest of the ecosystem model on line 20 of page 8, but want to confirm that that is the case.

Figure 3 and associated text: Could you change the color bar to give some sensitivity at the lower range of the NPP spectrum as appropriate to the tundra? In the current figure, it is unclear how and where the modeled and MODIS tundra NPPs differ, but this is crucial in understanding the soil carbon predictions from the model.

Figure 4: Whys isn't the NCSCD shown here as it is elsewhere? I'm not understanding

exactly the distinction in usage that is being made between the different soil carbon maps here.

Page 13, line 1: is this partitioning happening per soil level as well as per gridcell?

Figure 11: I am a little confused about what we are supposed to learn from this. If you track soil carbon that is initially in the system, then it will tend to decrease in time under steady conditions as the old carbon leaves the system and is replaced by new carbon. So there is information about the timescale of exchange between the deep carbon and the atmosphere here, but that isn't how you are presenting the data. E.g., if so, then you'd want to separate out the changes due to the transient boundary conditions. So I suggest clarifying what this result means.

The authors point out that an issue with initializing the deep carbon is that the model will tend to drift back to an equilibrium state. I'd point out a second issue, which is that doing so requires a much more careful treatment of how we go about benchmarking the models. Since a dataset cannot logically be used for both initialization and benchmarking, if we initialize the model with observations then we lose one of the few constraints we have on whether the model's soil carbon scheme is reasonable or not. Note that I am not accusing the present manuscript of circularity, as the authors do a nice job of separating out the comparisons of models when uninitialized versus the dynamics of the model when it is initialized later in the manuscript, but I'd recommend being very clear, e.g., in CMIP-type activities if this version is used in them, to specify when the model is initialized using specific soil carbon datasets versus allowed to find its own equilibrium, so that users of the model output do not fall into this kind of circularity trap.

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