

Interactive comment on “Weichselian permafrost depth in lowland Europe: a comprehensive uncertainty and sensitivity analysis” by J. Govaerts et al.

Anonymous Referee #1

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Govaerts et al. conduct a study of potential future permafrost aggradation in the Netherlands in the context of nuclear waste repository installation. They go beyond most previously published studies in this area of research by conducting a sensitivity analysis to a number of factors including subsurface parameters as well as the forcing climate conditions. In general, this is a fairly clear, succinct paper that should garner some interest in The Cryosphere. I only have a few very minor comments for the authors to consider. Since these are all minor, I haven't structured the comments.

P1, L10, delete 'being'

P1, L14, insert 'the' before 'engineered'

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P2, L3 Here and elsewhere, the authors interchange 'permafrost' with 'frozen ground'. Permafrost is only defined based on temperature (cryotic conditions) and does not necessarily imply the ground is frozen. This should be rewritten. Also in section 3.1 (first paragraph), the authors use -0.25C as their permafrost boundary. This makes no sense. By definition, the 0C isotherm is the permafrost boundary. The -0.25C level might be an indicator of frozen ground.

The first two sentences in the introduction need citations.

P2, L25, insert 'the' before 'engineered'

P2, L25-27, Kurylyk et al. (2014) review how permafrost separates surficial and deep subsurface water flow systems. They also discuss the role of advection in terms of the interactions between permafrost and climate. This would be useful in the authors' discussion on these topics later.

P3, L3 and elsewhere, is 'OPERA-project' an acronym? If so, define.

Equation (3). I am not used to seeing two derivatives (change in moisture content with temperature) in the effective heat capacity function including freeze-thaw. How do the authors reconcile this equation with Equation (14) in Kurylyk et al.?

Section 2, The authors don't really present the soil freezing curve (relationship between temperature and unfrozen water). They state on P5, L31 that they use a smooth Heaviside function. Heaviside function is not smooth, so this seems contradictory. Is it a linear function between 0 and -0.5C? If so, they should state that. If not, they should present the equation for it.

P5, L5 and L8, heat capacity (in this paper) is volume based, so why do the authors present it in mass-based terms (J/(kg K))

P5, L19, it is a bit silly to say that the thermal properties of the geologic material agree with the values chosen for similar material in past studies within the same order of magnitude. Surely one can be more precise than that given that thermal conductivities

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of ALL geologic material only vary by about one order of magnitude.

P5, L22, those are not units of thermal conductivity

P6, L31, I'm confused by the term 'unit thickness' followed by 250x250. I guess the authors mean the geologic unit, but in modeling, unit thickness usually means a thickness of 1.

P7, L2, change 'which dimensions range' to 'with dimensions ranging'

P7, L12, porosity also affects the latent heat, not just the bulk thermal properties

P7, L22, perhaps it is stated elsewhere and I missed it, but what is the lower boundary condition? Is it specified flux or specified temperature? The authors could add a figure showing their domain and boundary conditions. I think that would be helpful. What is the time step size?

P9, L15, these are called 'thawing and freezing n-factors' not just 'thawing and freezing factors'

P9, L19, see beste type

P9, L30, insert 'of' after 'average'

P11, L10 delete 'a'

P11, L16, delete 'e.g.'

P12, L2 should 'en' be 'and'?

P15, L17, Was Govaerts et al. (2011) only for one site? If so, this should be stated here. If not, the distinguishing factors between the present study and the Govaerts et al. (2011) study should be more clearly outlined in the introduction.

P16, L4-7, wouldn't it make sense for the authors to include the fact that they ignored surface glaciation as another one of their 'conservative assumptions' that they list in two other locations? Maybe that's not relevant for the Netherlands

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In general, I would assume the authors are familiar with the depths of the proposed nuclear waste repositories in the Netherlands. How do those design depths compare with the depths of maximum simulated permafrost? Surely this would be of interest to most readers.

Figure 4, why do the authors present results for these two specific polygons?

Figure 5, why do the authors show a binned color scale? Shouldn't this be a gradient color scale, or do the authors actually bin their results?

Figure 6, I think it would be advantageous for the authors to present the location of the transect for this figure in Figure 1

Figure 7, does this only show the maximum permafrost depth across the nation at any point in time, or is the spatial variability in permafrost included in the percentile calculations?

Figure 8, I'm confused by what T2-T19 refer to.

Table 2, is the porosity in Table 2 only the porosity of sand (Table 2 implies this). There are commas where there should be periods for the decimals.

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