

## ***Interactive comment on “Characterizing Uncertainty in the Hydraulic Parameters of Oil Sands Mine Reclamation Covers and its Influence on Water Balance Predictions” by Md. Shahabul Alam et al.***

**Anonymous Referee #1**

Received and published: 17 July 2019

### General Comments

This paper evaluates soil hydraulic property uncertainty based upon multiple realizations of inverse modelling of soil reclamation covers. They use an innovative technique, Progressive Latin Hypercube Sampling (PLHS), to generate a large data set (statistical distribution) of optimized parameters. The most consequential finding showed that climate variability results in greater variability than uncertainty and heterogeneity in soil hydraulic properties. The results are of interest to the academic community and potentially to industry, however there are several issues with the underlying model that

C1

require further attention.

The entire study rests on the initial inverse modelling that identified a group of soil hydraulic properties that could then be used to generate a statistical distribution of optimized properties. However, any assessment of model performance is absent from the manuscript, and the only graphical representation of model fit is relegated to the appendix and only shows simulated versus observed  $r^2$ , which is not enough for the reader to judge the performance of the model. Since identifying appropriate soil hydraulic properties is so critical to the rest of the study, model performance should feature much more prominently.

Furthermore, I would like to be convinced that the inverse optimization did, in fact, find the appropriate properties. The internal HYDRUS inverse scheme used is notorious for being unable to find the global minima for each parameter, and getting “stuck” in local minima. If using this type of inverse scheme, the appropriate methodology is to vary the initial parameter “guess” to ensure that the global minima has been identified. Was this done? Also, the range over which the optimized algorithm was allowed to vary was extremely small, and could easily be excluding the global minima. Further explanation of the approach to inverse modelling, and justification of the narrow range for optimization is needed. Further clarity is also needed on how many parameters were simultaneously inverted, as there is a preponderance of evidence that demonstrates the inability of gradient-based inverse schemes to identify global minima when more than 6 parameters are inverted (the developer of HYDRUS has stated that he has never successfully optimized more than 5 or 6 parameters).

Besides the aforementioned technical criticisms, this manuscript lacks (in my opinion) a genuine discussion section that ties the results to an interpretation of the physical characteristics of the soil and its setting. I would have welcomed an expanded discussion of relevant physical processes on the reclamation landscape that the results pertain to. This could be done throughout the results and discussion section. Furthermore, while the authors honestly admit the limitation that no consideration was given

C2

to climate change, it seems a small leap to say that if the current situation illustrates that climate variability is the main cause of uncertainty, a changing climate will magnify that. What are the implications for practice? For the most part, the conclusion section summarized the findings (i.e. was a summary, rather than conclusions). Some more insight into how this information can be used would be helpful to the reader. What recommendations would the authors make to industry trying to reclaim landscapes?

#### Specific Comments

P3L5-15 Given the relatively "light" computational load of HYDRUS 1D is the latin hypercube technique necessary?

P3L5-30 Latin hypercube discussion would be of little importance or significance to most readers, perhaps some of the methodology could be moved to an appendix?

P6L7 I've seen a number of different definitions for the initialism LFH, but never leaf, folic, and humic. Perhaps look at the definitions of Naeth?

P6L6-18 This section is pretty hard to follow, suggest rewording

P6L19 Particle size distributions for peat are not common due to its fibric nature. A brief note on what was done would be appropriate

P7L1 It does not really seem appropriate to lump the peat and LFH covers into a single group

P8L4-L32 More detail is needed here. What about the Mualem tortuosity parameter? Was this inversed or set to equal 0.5 or some other value? Did each simulation include the inversing of multiple materials? If so this could be highly problematic, obtaining parameter estimates through inverse simulation is exceptionally difficult with more than 5 or 6 parameters (as stated by Simunek), let alone 15. Furthermore, the HYDRUS inverse method of Marquardt-Levenberg is highly susceptible to getting "stuck" in local minima as opposed to finding the global minima. In order for successful inverse simulation to find the global minima with this method the initial values must be varied. If the

C3

same parameter sets are found after this procedure then it can be assumed with reasonable confidence that the global minima has been identified. All subsequent findings require that you found appropriate soil hydraulic properties. I am not yet convinced that you did, I would need to see more results related to the performance of the inverse simulation.

Furthermore, as shown by the parameter values in Table 1 many of the parameters are extraordinarily tightly constrained (alpha, n, and Ks in particular). Even covers of the same (ostensible) material could easily vary by multiple orders of magnitude, yet only the lean oil sands are allowed to vary more than roughly 1 order of magnitude. The van Genuchten n parameter for the subsoil has remarkably little freedom to vary.

Also I could not seem to find any mention of root water uptake parameters, or the parameters associated with partitioning E and T from PET, or the interception constant.

P9L19 In the Discussion, I would like to have your assessment of the implications of this choice of root distribution, since it is idealized and probably incorrect.

P10L2 Unit gradient meaning free drainage?

P10L25. Are these (optimized soil parameters) the V-G parameters?

P11L11 I'm not clear on what you mean by discrete parameter distributions not being representative of the range of distributions

P12L2 Just to be clear, this is the order of the soil profile? Peat/LOS/Subsoil?

P12L6 There is evidence to suggest that some early covers generate a lot of snowmelt runoff, rather than infiltrating into the soil (e.g. Ketcheson et al., 2016), although others that suggest mostly infiltration (Nichols et al., 2016). Are there measurements or observations at this site that support your assumption. You should probably include a statement to this effect.

P12 L7 Does this method consider sublimation?

C4

P13 L9 The performance of your model should not be hidden in an appendix, and it is not acceptable to just show the  $r^2$ , if only a single model metric were chosen, show the Kling-Gupta Efficiency. Otherwise, the RMSE, should be shown as well as perhaps graphs over time of the simulated and observed water contents.

P13L13 This seems poor justification for grouping the parameter sets together.

P14L1 Why would you be interested in including temporal variability into these parameter sets? By then applying these parameter sets to long-term simulations you would be artificially accentuating the variability in net percolation and ETa.

P14L5 I don't think that you have shown enough supporting evidence to make that assertion. Did you perform this same procedure for alpha, n, theta r, and theta s?

P17L2 Does this approach assume that there is no correlation between parameters?

P21L8 Are you surprised that the LAI max occupies such a narrow range for such different soil covers? Why is the range so small? Please add a comment on this, as 4.12 to 4.50 are basically the same, as far as model precision goes. Doesn't this suggest that you can get a reasonable forest growing on basically any cover? The differences in Figure 10 are so slight it seems like the same graph copied 5 times.

P22L9 and Fig11 Are there statistically significant differences?

P27L10 I'm not really sure I understand the value of this whole objective and section. I would remove this from the manuscript.

P29L9 This seems to be a foregone conclusion since you have already admitted to lumping to very different materials into this category.

P29L19-20 "the results of this study help to highlight a wide range of cover performance risks that can occur when parameter variability is combined with climate, LAI, and cover thickness variability", this is really the first and only mention that the reader gets of cover performance risks. I am interested to know more, but it needs to come before that statement!

C5

P29L25 AET really didn't change that much in any scenario. The data seems to conflict with the interpretation.

Fig. 3 I could be mistaken but it seems that the hydraulic conductivities are outside the maximum and minimum values that are seen in Table 1. The fact that some parameters are logged and others not makes it difficult to compare. The alpha values in the figure are in different units than what is in Table 1 (not the only instance of alpha values being in inconsistent units). The saturated water content of the subsoil seems to in some cases be very low. What is the cause of the inverse scheme identifying a 0.1 or 0.2 water content? That is almost certainly incorrect.

Fig. A2 While it seems reasonable that the subsoil and lean oil sands are grouped together. It does not seem appropriate that the peat and LFH soils are grouped. Their parameters (particularly alpha and n) differ substantially.

Formatting Comments

Fig. 1 Font in legend is unclear - small and resolution too poor. Figure should be improved for readability

Fig. 2 Should be improved for readability, very difficult to understand

Fig. 9 Could probably be moved to an appendix

Fig 11 could be replaced with another line for LAI+/- SD in Table 3.

Fig 12&13 could be put into a single Table (there are a lot of Figures).

Fig 15 could be summarized in one or two sentences in the text.

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-154>, 2019.

C6