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Geoscientific Model Development Discussions
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Interactive comment on "SHIMMER (1.0): a novel mathematical model for microbial and biogeochemical dynamics in glacier forefield ecosystems" *by* J. A. Bradley et al.

Anonymous Referee #1

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This study presents an original contribution to the modeling of microbial dynamics in glacier forefield ecosystems, especially for the early stage of microbial colonization and soil development. The model predicts fairly well the observed phenomena, introduces quantitative insights, and may serve as a useful tool in future studies. Key carbon and nutrient cycling processes are resolved in a simple and generalizable way. Therefore the model appears to be adaptable to similar sites or ecosystems. I recommend this manuscript to be published with minor revisions.

On model construction:

- In terms of microbial respiration, why do you use the Q_{10} model, not an Arrhenius

equation model? If Q_{10} is not a constant but depends on soil temperature (Line 26, Page 6169), then it's probably not a representative parameter for the system. But if the reaction rates are modeled based on energetics, for example, using a temperature-dependent Gibbs free energy of activation, temperature effects on the metabolic state may be better represented.

- This model is a 0-D model, which does not resolve transport driven by spatial gradients or advection. While this is probably sufficient for in situ soil processes because of the presumed homogeneity and shallow soil depths, it may have neglected vertical aqueous transport of carbon and nutrients if there is surface runoff. Would this bias the model results?

On model results:

- What is the mechanism for the oscillations in the biomass of soil autotrophs and nitrogen fixing autotrophs in Figure 6? Is it possible that they are artifacts from numerical evaluation?

- Why is the seasonal amplitude in simulated total microbial biomass at the Damma Glacier (Figure 10) much larger than that at the Athabasca Glacier (Figure 11)?

- For the Damma Glacier, the model tends to stabilize in terms of the total microbial biomass and DIP, and increase slowly in C substrate and ON, while the data show roughly exponential increases for all the variables (Figure 10). This discrepancy is attributed to vegetation onset. But why doesn't it happen at the other site? What would explain such difference? Does it mean that the model is not applicable to the later stages of soil development in such ecosystem when vegetation occurs?

Technical comments

- Line 6–8, Page 6148: "Many of their parameters cannot be constrained on the basis of information available for glacier forefield ecosystems. . ."

I wonder what those parameters are. Are physical, biochemical or physiological pa-

GMDD

8, C1981–C1985, 2015

rameters that are difficult to constrain for glacier forefield? Please name a few of them.

- Line 17, Page 6148 to Line 17, Page 6150: I think there are a lot of overlapped information between these two paragraphs and Table 1. Please consider abridge them and try to be concise. Or maybe move some information to the model description section. It's not necessary to be overly elaborate on the model construction in the introduction.

- Line 25, Page 6152: Define "EPS" here. You don't wanna your readers to look it up from other papers.

- Line 12, Page 6153: Please define "L" in Eq. (4) as PAR.

- Line 27, Page 6154: "Nitrogen fixation in the SHIMMER model is sensitive to many of the environmental factors often cited in the literature, including surrounding DIN concentrations, temperature, and carbon and phosphorus limitation (Liu et al., 2011)."

I haven't seen an equation describing such dependence in the paper. The production term in Eq. (13) does not have DIN dependence or phosphorus dependence. So which equation does this sentence refer to?

- Line 7, Page 6156: typo, "dependant" –> "dependent"

- Line 10, Page 6156: Why do you assume that the loss terms are proportional to the square of the biomass? I don't see a citation here. Please justify your assumption with one or two sentences.

- Eqs. (11) and (12), Page 6157: Please define the v_{sub} parameter here, though it has been described in Table 5.

- Line 19, Page 6157: What are the N/C and P/C ratios used here? If the values are only shown in Table 5, then you need to guide the readers to Table 5.

- Line 7, Page 6158: Could you specify which version of the R language you were using? It may not make a difference between versions, but it'll be good to provide such

GMDD
8, C1981–C1985, 2015

information, just in case.

- Line 7, Page 6159: The extinction coefficient has a unit m^{-1}, so it has to be 6 m^{-1}.

- Line 19, Page 6161: "The calculation of may yield a 'false-negative' result (i.e. a value close to zero) when the variation in model output either side of the nominal value has an opposite sign (i.e. a parabolic relationship between the parameter value and model output)."

I'm not sure I understand what you try to mean by this sentence. If you have a parabolic shape relationship, does it not mean that the parameter has an optimum value within that range? Why would it be an unwanted behavior? And if you want to detect the "false-negative" behavior, why not look at the second-order derivative?

- Line 22, Page 6164: "high plat abundance"

Is there a typo? Do you mean "plant abundance" or "microbial mat abundance"?

- Line 4, Page 6165: "and initial substrate bioavailability is assumed to be 40% labile and 60% refractory"

The initial substrate bioavailability for the other site is assumed "30% labile and 70% refractory". Is there any explanation for the difference?

- Line 27, Page 6165: "When considering a 1 cm deep soil profile, 1 g dry soil occupies a surface area of 0.869 cm2."

This means exactly that the dry density of the soil is 1.15 g cm^{-3}. Then why repeat the information if you have stated it in the preceding sentence? Also, you may need to say "the typical *dry* density of the soil. . .", because "density" usually means bulk density not dry density.

- Table 1 on Page 6196: This table looks a bit too wordy since many aspects are already explained in detail in the main text. Better abridge the description of each entry down to two sentences/lines.

GMDD

8, C1981–C1985, 2015

- Table 2 on Page 6197: I think it's better to call A2 and H2 "generic soil auto-/heterotrophs" or "non N2-fixing soil auto-/heterotrophs".

- Figure 4 on Page 6207: The unit of the ordinate in panel (a) should be shown as "W m^{-2}"

- Figure 7 on Page 6210: It is very hard to see clearly the variable names on the ordinate, unless zoomed to 200% and above. For better visualization, I suggest the authors replot this figure as a heat map. Juxtapose the nine parameters as the abscissa, and choose a color bar that is distinct enough and colorblind-safe for the variation in λ.

- Figure 9 on Page 6212: This figure has the same technical problem as Figure 7. Better replot it as a heat map, and perhaps use a logarithmic scale colorbar for ø given its highly variable range. Also, please specify that ø is a percentage value by adding "%" as its unit.

Interactive comment on Geosci. Model Dev. Discuss., 8, 6143, 2015.