Review #3

We appreciate the supportive comments from Reviewer #3. In our revisions we have provided greater clarity and detail in our model assumptions & rationale. For clarity specific reviewer comments are numbered for each reviewer (in italics). Our responses (in bold) follow each comment Specific changes in the revised manuscript are also noted with the same numbering scheme to clearly note how we incorporated reviewers' suggestions.

R3.1 [P2015, L5-6: I think you misrepresent the results of Sulman et al. (2014) a little bit. Their CORPSE model also includes different litter qualities and protection on soil minerals. MIMICS obviously also includes different microbial functional groups (copiotrophs and oligotrophs), but Sulman et al. (2014) distinguish into rhizosphere and non-rhizosphere soil which could be interpreted as the preferred habitat for copiotrophs (rhizosphere soil) or oligotrophs (non-rhizosphere soil).]

We agree with this assessment of the CORPSE model, and have removed the Sulman et al. 2014 reference from this part of the text, replacing this reference with Harakuk et al. 2015

R3.2 [P2015,L12: With the term "soil biogeochemical model" you imply that you model more than SOC dynamics (e.g. nutrients or weathering). SOC turnover model or SOC decomposition model might be more fitting.]

This is an accurate assessment. We have accordingly modified the revised text to state that, "MIMICS is a soil C model…".

R3.3 [P2016,L17: You state that "microbes only assimilate C from the available SOM pool", but according to your model structure microbes also directly assimilate metabolic and structural litter.]

This too is accurate. The revised text has been modified to say, "…microbes only assimilate C from litter and available SOM pools."

R3.4 [It might be worth to discuss how fungi fit into your framework of copiotrophs and oligotrophs.]

We contend that the copiotrophic / oligotrophic framework represented in MIMICS applies to archea, bacteria, and fungi. For example, fungi have a diversity of physiological characteristic that range from extremely copiotrophic (*Saccharomyces sp.*, yeasts) to extremely oligotrophic growth strategies (see Parkinson et al. 1989). We acknowledge that quantifying the relative abundance and physiological characteristics of these growth strategies is an answered challenge for soil scientists; however, the model assumes that the physiological characteristics and ecological function of these organisms has a greater bearing on soil C processes than their location on the phylogenetic tree.

Structure of the paper

R3.5[I think a flow chart is needed to illustrate how the various comparisons against observations are linked: For example, is the calibration to leaf litterbags informative for the comparison with site-level SOC stock?]

As a visual learner, I (W. Wieder) completely agree; however, we feel text in the manuscript clearly lays out this workflow (see the last paragraph of the introduction, Section 1). Moreover, revisions to the methods (Section 2) clearly delineate how calibrations from one suite of simulations were (or were not) carried on to subsequent experiments.

Model structure

R3.6 [Your definition of the "chemically protected pool" deviates from the conventional use of this term in SOM research. To my knowledge "chemical protection" is usually used to describe stabilization on iron or aluminium oxides or the edges of clay minerals via ligand exchange or anion exchange (Six et al., 2002; Conant et al., 2011). The more appropriate term would be "chemical recalcitrance" or "selective preservation". Within your model framework the "chemically protected pool" could be called "structural microbial residues".]

This is an astute observation noting when imprecise language within a discipline produces unnecessary uncertainties. Our concept of a (bio)chemically recalcitrant (SOMc) pool in MIMICS corresponds to this later definition (see also Six et al. 2006) and text in the revised manuscript now reflects this perspective.

R3.7 [You state that you normalize the microbial turnover rate with site-level NPP (P2018,L6-8). What is the mechanistic justification for this? Is this normalization the reason for the strong correlation of the global microbial biomass with NPP (P2023,L14)?]

Reviewers 1 and 2 raised similar concerns, which we have tried to clarify in the revised text (see expanded section on model structure and assumptions in Appendix A1). The correlation between microbial biomass and NPP is a feature of forward Michaelis-Menten models (Wang et al. 2014), which is also supported by observations (Bradford et al., 2013; Fierer et al., 2009). Soil food web literature also supports the idea that sites with higher microbial biomass (i.e., more productive sites) may support greater top-down control over total microbial biomass (Thakur & Eisenhauer 2015).

R3.8 [For the decomposition of SOMc to SOMa you introduce the parameter KO (Eq. A10). Could you elaborate what this parameter represents and why microbes have a harder time decomposing SOMc compared to LITs. In other words: Why is Vmax the same for SOMc and LITs, but more microbes are needed to reach this Vmax for the decomposition of SOMc compared to LITs?]

We assume the V_{max} of chemically recalcitrant SOM (SOMc) is approximately similar to structural litter (LITs); however, in mineral soils enzymes have a harder time accessing these substrates. Thus, the parameter KO (eq. A10) increases the half saturation constant (K_m) for oxidation of SOMc. Theoretically, KO could also function of soil texture or

mineralogy, but for now we isolate mineralogical controls to the uptake of SOMa (eq. A3 & A7) through the Pscalar parameter (Table B1). [This text has been added to Appendix A1]

Model-data comparison

R3.9[You compare the MIMICS model with the LIDET leaf litterbag data. It is unclear to me why the clay content of the different LIDET sites should be considered. To my knowledge the LIDET leaf litterbags were not in contact with the mineral matrix. Adair et al. (2008) state "leaf litterbags were placed on the ground surface". Hence, the protected pool should be zero when modelling leaf litterbags.]

This is an excellent observation. When MIMICS explicitly represent above- and below-ground processes (or has explicit vertical resolution) we will certainly omit soil texture modifications from litter and/or organic soil horizons.

R3.10 [It might be a good idea to use both litter types of LIDET - the root litterbag and leaf litterbag data. This would make it possible to evaluate the effect of physical protection (leaf litterbag VS root litterbag).]

Again, we completely agree. Future studies should look at root vs. leaf litter, as well as C-N dynamics in microbial-explicit models. Yet to date this is the most rigorous examination of litter decomposition simulations in a microbial explicit model across such broad ecoclimatoligical gradients.

R3.11 [You keep the microbial biomass constant during your modelled litterbag experiment (P2017,L22-23). What is the mechanistic reasoning for this approach? Does this mean that microbes outside of the litterbag decompose litter inside the litterbag? I would guess that microbial biomass and the ratio of oligotrophs to copiotrophs changes during a litterbag experiment according to how much of the metabolic litter is remaining. Please clarify why the ratio of oligotrophs to copiotrophs should not change during a litterbag experiment.]

These ideas would be a fascinating application of MIMICS that may provide insight into evolution of microbial community composition in a litter cohort, presumably as the chemical quality of substrates declines. As with many first order model, like DAYCENT, here we assume that initial litter quality sets the stage for microbial dynamics and litte quality effects on litter mass loss. Indeed, the focus of the LIDET papers we cite (Parton et al. 2007, Adair et al. 2008, Harmon et al. 2009) assume litter bags don't change inherent ecosystem properties, but are used to broadly characterize climate and litter quality effects on rates of litter mass loss – and not the changes in internal litter-bag chemistry or in temporal shifts in microbial community composition. As such, we held microbial biomass constant in our LIDET simulations. This text has been clarified in section 2.1.1

R3.12 [For the comparison against site-level SOC stocks you modify "the microbial turnover and growth efficiency parameters" (P2018,L13-14) because surface and subsurface dynamics are not explicitly modelled in MIMICS (P2018,L12-13). In my opinion surface and subsurface dynamics are included in MIMICS: For the litterbags (surface dynamics) SOC cannot be

stabilized on clay minerals (there is no clay in the leaf litterbags), while for the comparison with site-level SOC stocks (subsurface dynamics) the influence of the soil matrix comes into play.]

These are valid points, however, the microbes decomposing leaf litter are spatially separated from those decomposing SOM (with the exception of multi cellular fungi with hyphae in the litter layer and mineral soils). Presently the same microbial communities are acting on LIT and SOM pools in MIMICS. [see also response to this same reviewer's comment #9, above]

R 3.13 [For the comparison against data from the N-enrichment meta-analysis you force MIMICS with an increased aboveground NPP. What about belowground NPP? Did the meta-analysis discuss if plants might invest less into roots because the uptake of mineral N was easy?]

This may be likely, although it changes in fine root inputs have observed in the meta-analysis used for this study (Liu and Greaver 2010), or others (Jannsens et al. 2010).

R 3.14 [For the global simulations I do not understand why you have to adjust τ , fmet and Pscalar (P2019,L18). Has this to do with the influence of the mineral matrix which is absent in the leaf litterbags?]

These adjustments were necessary for several reasons, which we also describe in Appendix A3. In moving from cross-site to global simulations we used different estimates of plant productivity, taken from CLM4.5. We also simulated soils 0-100 cm (rather than 0-30 cm). Parameter changes we made in global simulations served several functions including to: maintain both microbial functional groups in most gridcells (Supplementary Fig. 2b), simulate appropriate ratios of MIC:SOC (Supplementary Fig. 2f), and simulate reasonable steady-state SOM distributions (Fig. 4).

Model parameterization

R 3.15[You state that you "parameterized MIMICS with leaf litter decomposition simulations" (P2016,L24). How did the parametrization work? You should clearly state which parameters are literature-derived or from previous studies, and which parameters were calibrated to the leaf litterbag data.]

We feel Appendix A1 addresses this concern. We've added the following sentence to the text already presented in response to this comment: "Building on our previous work (Wieder et al. 2014), the LIDET decomposition study presented here was designed to facilitate parameter estimation (Table B1), however we note many of these parameter values the are poorly constrained by direct observations."

R 3.16 [What is a "Tuning coefficient" (Table B1). How was this coefficient determined?]

This was a term and value taken directly from the paper from which we obtained the temperature sensitivity parameters for Vmax and Km (German et al. 2012), see also the

footnote on Table B1, "[#]From observations in (German et al., 2012), as used in (Wieder et al., 2013; Wieder et al., 2014c)."

R 3.17 [Why is Vmax_K = {3, 3, 2} and Vmax_r = {10, 2, 10} (Table B1)? Or in other words why do the r-strategists decompose the structural pool slower than the available SOM pool, while K-strategists decompose the structural pool faster than the available SOM pool?]

Assumptions and references describing these decisions are described in Appendix A1. Briefly, we've added this line to the revised text. Parameter values chosen here reflect the greater enzymatic capacity for depolymerization in oligotrophic communities (higher V_{max} and lower K_m), but copiotropic communities possess a greater enzymatic capacity for assimilation of SOM_a.

Technical corrections

P2020,L23: I think you wanted to refer to Table 1 not Table B1

This text actually refers to Table C1, and the revised text has been modified accordingly.

P2021,L18,L20: MIMCS

Corrected

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