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Geoscientific Model Development Discussions

Interactive comment on "Stoichiometrically coupled carbon and nitrogen cycling in the MIcrobial-MIneral Carbon Stabilization model (MIMICS-CN)" *by* Emily Kyker-Snowman et al.

Anonymous Referee #2

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General Comments:

This is a substantial effort to incorporate microbial controls into a modeling framework to simulate soil C and N dynamics over broad spatial and temporal scales. The MIMICS model follows the well-known CENTURY model structure used in many previous ecosystem and global scale models. It incorporates the CENTURY litter allocation scheme, but identifies two microbial guilds that each primarily processes one of the two litter pools, and allocates microbial products (and some litter) to three SOM pools reminiscent of- but more ecologically defined than the active, slow and passive pools in CENTURY. This model replaces first-order decay rates with Michaelis-Menten

functions based on microbial biomass, to calculate principle material fluxes, with kinetic parameters representing composite, empirical attributes of microbial pools. Thus decay rates are regulated by microbial biomass with stringent controls on microbial biomass constraining maximum rates. The results showed general correspondence with observations at least comparable to earlier results of more empirical models (e.g., DAYCENT) but generating potential insights (or at least insightful questions) to underlying microbial controls. The authors state (line 401) that MIMICS-CN is a "first step towards representing" a more comprehensive and realistic soil biogeochemical model, but I urge the authors to be bolder. In fact, there is little need to make so many comparisons to DAYCENT other than demonstrating the capacity of a more modern, biologically defined model to produce more insights. That argument seems to be generally accepted.

Specific Comments:

The influence of the CENTURY formulation on MIMIC's structure is apparent, but is there a rationale independent of convenience or of comparing behaviors of similarly structured models having different functional equations? For example, what is the justification for the two particular pools of microorganisms? Is it an extension of the litter quality definition?

What is the rationale for the three pools of SOM? This allocation isn't entirely consistent with other comparable models, like MEND, Millenial Model, etc. Although described in other papers, the central importance of this SOM scheme to MIMIC's behavior requires more explanation to understand simulations. Adding the dichotomy of physiochemical (mineral associated) and chemical (recalcitrance) is a positive step, but it seems to describe SOM as particulates and adsorbed organics without mention of soil aggregates that are important and tend to mix different qualities of dead organic matter. Is the active pool comprised of dissolved compounds? In addition, what empirically observed data were used for comparisons (e.g., Fig. 6)? Were these various fractions of soil extractions?

A novel aspect of this model is the dual "spilling" mechanisms for C and N, depending on the balance of supply vs. demand of C and N between substrate and microorganisms, and based on reasonable stoichiometric constraints. However, could this mechanism contribute to the excess loss of C (and N) over the long term? Also, it seems that the pulse of litter for the 10-year simulation represented the sole input for those years, correct? Could this also be a reason why simulated soil C and N were lower than observations at 7-10 years? Finally, what is the justification for nominal N-leakage (i.e., NUE = 0.85) and the N-leaching rate (eq. A33)? Could this N-loss be another factor contributing to lower C and N at late stages of simulation? I think the model has several features that could explain that discrepancy.

Why did the authors choose only 6 of the possible LIDET litter types, and these 6 in particular?

Lines 138-9: Was microbial biomass at Harvard derived from Xu et al. (2013) or observations?

Line 173: Not all previous SOM models simply used cascading pools of progressively more recalcitrant materials. The value of some of these final explorations isn't clear.

Line 188: How much of the similarity between microbial biomass estimates and observations (Fig. 5) can be ascribed to the density-sensitive turnover rate for microorganisms? Overall, the microbial values were the most tightly constrained within the model to reflect studies similar to those included within comparisons, especially as percent of soil C and N; if so, lines 235-237 might be overstated.

Lines 193-5: What was the rationale for the changes in (fi) and microbial turnover parameters other than to fit observations at Harvard Forest? Is there an interpretation of these adjustments?

Line 314 (and elsewhere): The authors seem surprised that MIMICS can reasonably match basic characteristics of these systems, but not only is the model largely constructed parallel to previous models that already did so, but additional model parameters and flexibility has been incorporated (and constrained). It would be a surprise if it didn't. Again, the authors could speak more boldly about their work.

Lines 353+: How do these pools compare to the particulate, aggregate protected, and mineral-associated organic matter pools that more realistically represent SOM (cf. Abramoff et al. 2017)? I don't understand how the pools defined in MIMICS were compared to observations.

Line 358: How do the first-order kinetics of the physiochemically-protected SOM compare with adsorption-desorption kinetics of mineral-associated organic matter (cf. Wang et al. 2013 Ecol Appl 23:255-272)?

Lines 354+: Soil clay content was important in MIMICS and obviously in the real world. This is a mechanism needed for broad scale modeling, but how does MIMICS' responsiveness differ from earlier models that explicitly included soil texture as a control on SOM pool dynamics?

Line 370: Wouldn't the relationship between soil C:N and litter C:N be strongly influenced by soil mineralogy and chemistry? Not that microbial processing wouldn't be important, but stabilization is likely impacted by the nature of the stabilizing medium.

Technical Suggestions:

Fig. 3: It seems that the individual R2 values for C and N by ecosystem in Table 2 represent the scatterplots in Fig. 3b and d, so it would be helpful to mention the biases reported in Table 2 when interpreting differences between simulations and observations by biome.

Are the simulation outputs in Fig. 4 red triangles rather than the dots mentioned in the legend? Also, I don't recall how the mass of N in decaying litter could increase above initial values; was this a result of immobilization from the soil DIN pool?

I don't think that section 4.3 adds much to the paper. If necessary, it could be tightened to focus on the subset of topics that are the immediate objectives for future work by this group. Otherwise, it is so broad that it distracts from the important results of this work.

I suggest that most of lines 424-430 and 433-end could be omitted and the authors focus more explicitly on the key contributions of MIMICS-CN's to modeling soil C & N dynamics across broad scales. Again, I think the rest detracts from the interesting results of this work.

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