Anonymous Referee #2:

General Comments:

2.1 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 paper 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.

We appreciate these general comments and the reviewer's clear understanding and appreciation of the work presented here. We are grateful for the encouragement to be "bolder," and have made substantial changes to the discussion that we hope will address this feedback (see especially section 4.3 to the end). We still find the DAYCENT comparison helpful in contextualizing the parameterization and evaluation of MIMICS results, but will take care to avoid unnecessary or lengthy comparison in the discussion of the text.

Specific Comments:

2.2 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?

We clarified the intent and assumptions of the microbial functional groups represented in MIMICS-CN in the methods. The microbial functional groups are intended to broadly capture tradeoffs in microbial growth rates and growth efficiency, with rapidly-growing – low efficiency, r-strategist (MIC_r) and slower-growing – higher efficiency K-strategist (MIC_K; Wieder et al. 2015). In MIMICS-CN we extend these microbial physiological traits to include microbial stoichiometry and assume that the higher metabolic capacity of MIC_r also require more nitrogen and, thus a lower microbial biomass C:N ratio.

2.3 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?

We consider the SOM_p pool to be largely derived of low C:N organic matter that is largely composed of microbial necromass that is adsorbed onto mineral surfaces (e.g. Mineral associated organic matter, MAOM; Grandy and Neff, 2008). By contrast, the low-quality SOM_c pool consists of decomposed or partially decomposed litter that has more structural C compounds, such as lignin, and a higher C:N ratio (e.g. particulate organic matter, POM). Finally, the SOM_a is the only SOM pool that is available for microbial decomposition; it contains a mixture of fresh microbial residues, products that are desorbed from the SOM_p pool, as well as depolymerized organic matter from the SOM_c pool. Under these assumptions we do not specifically consider soil aggregates, but we recognize their importance in maintaining organic matter persistence in soils. We added text to the first section of the methods to describe the structure and reasoning behind MIMICS-CN in more detail. Figure 6 only illustrates model assumptions, not observations across sites (clarified further in R2.11, below).

2.4 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.

We appreciate these suggestions and assume they are focused on trying to understand why our results also show higher than observed rates of litter C mass loss in deciduous and coniferous forest (Figs 2a, 3b; Table 2). This suggests that the partitioning of plant detrital inputs into litter pools that are chemically defined works well for initial stages of litter decay, but may not consider the changes in substrate chemistry or microbial community succession that occur in later stages of decomposition that slow rates of mass loss (Berg, 2000; Bradford et al., 2017; Melillo et al., 1989). Models that implicitly represent microbial activity capture this phenomena by using a three pool structure (Adair et al., 2008), and future studies can consider how to more mechanistically understand interactions between initial litter quality, decomposer communities, climate, nutrient availability and late-stage litter decay rates (e.g. Craine et al., 2007; Hobbie et al., 2012; Wickings et al., 2012) in models like MIMICS-CN.

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

We focus our analysis on six leaf litters that were simulated across all sites that have been used previously to evaluate litter decomposition dynamics in terrestrial models (Bonan et al. 2013, Parton et al. 2007, Wieder et al. 2015). Root litter types included in the original LIDET experiment were not included.

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

The value we used as a target for microbial biomass was estimated at 1% of soil C based on Xu et al. (2013).

2.7 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.

We agree this sentence was distracting and have removed it from the text.

2.8 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.

This is a very astute observation. Among other parameters, the density-dependence turnover rate is an important control on microbial biomass that we used to parameterize the model for Harvard Forest prior to our simulations at other sites. We used the same value for density-dependent turnover for the rest of the LIDET sites and produced a range of values for microbial biomass that did reflect a similar range to observations (Fig. 5). In general, we think the indicated lines reflect an accurate reporting of our results. However, we understand the concern and changed the language to be less definitive.

2.9 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?

In section 2.2 of the methods, we discuss how we used observations at Harvard Forest to help parameterize the model before evaluating the model's performance at other sites. The purpose of these adjustments was to fit observations at Harvard Forest, with the expectation that making these adjustments would help the model to perform more realistically at other sites.

2.10 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.

We appreciate the encouragement to speak more boldly about our work and recognize that MIMICS-CN simulates microbial stoichiometry, microbial growth and turnover, and microbially-mediated decomposition, rather than using prescribed values as in models that

lack explicit representation of microbes. This increases the power of MIMICS-CN to explore the microbial and biogeochemical processes underpinning model predictions. Following these suggestions, we have made substantive changes to the text of the manuscript. We have added more discussion about Figure 6 and the implications of the patterns illustrated there, while refining other parts of the discussion to give caveats and limitations a less outsized impact in the manuscript relative to a discussion of the model structure, implications, and future directions.

2.11 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.

Results in Fig. 6 are intended to illustrate patterns in model results that *could* be compared to observations, but we don't know of data available across environmental gradients that could be used to sufficiently evaluate these assumptions at this time, and we also feel that such an evaluation of all these variables would fall outside the scope of this manuscript. Nonetheless, this exercise provides an opportunity to explore how model-defined assumptions about pool stabilization mechanisms drive potential responses of SOM pools to environmental variables. For example, the chemically-protected and available SOM pools in MIMICS-CN turn over based on temperature-sensitive Michaelis-Menten kinetics and litter chemistry (the later controlling allocation to litter pools the relative abundance of microbial functional groups). Therefore, in our simulations, SOM_C pools (analogous to light fraction or POM pools) were negatively correlated with MAT and positively correlated with litter lignin content (Fig. 6d, 6e). Turnover of the physicochemically-protected SOM pool, on the other hand, occurs via first-order kinetics with a rate constant modified by clay content, and the equilibrium values of this pool in MIMICS-CN are determined by inputs that largely come from microbial biomass and biomass turnover rates (Fig. 1). Therefore, the equilibrium values of simulated SOM_p (analogous to heavy fraction or MAOM pools) were positively correlated with the product of ANPP and clay content (Fig. 6c). We added text to the discussion to clarify the purpose and interpretation of the results shown in Figure 6.

2.12 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)?

This is an interesting question that we would like to explore in the future, but addressing it in the text here falls outside the scope of the discussion about environmental controls over SOM pools in MIMICS.

2.13 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?

We point to previously published work here. In global simulations with the carbon-only version of MIMICS, these assumptions result in MIMICS projecting longer turnover soil C

times and larger soil C pool in the tropics than other models (Koven et al. 2017, Wieder et al. 2018) and a higher vulnerability of high latitude soil C stocks (Wieder et al. 2015; 2019).

2.14 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.

We agree; the text now reads "is SOM stoichiometry correlated with litter quality, or is it better explained by climate, edaphic, and mineralogical gradients that impact soil microbial community composition, microbial activity, and mineral-mediated mechanisms of SOM persistence?"

Technical Suggestions:

2.15 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.

To avoid redundancies, we have removed the biome statistics from the text and refer to Table 2 (see also response to R1.4).

2.16 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?

We changed "dots" to "triangles" in the legend and text, see also response to R1.5. The increase in N in litterbags above 100% of initial values was the result of immobilization from the soil DIN pool; we added a sentence to the methods to make this clearer.

2.17 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.

Following this suggestion, we edited the section referenced here (now 4.4) to give it a narrower focus and refine the broad discussion of potential next steps with MIMICS. Our goal with these changes was to highlight more specifically the next steps with MIMICS that we feel are the highest priority. We hope you find these changes clarified this portion of the discussion into something less distracting and more in line with the rest of the text.

2.18 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.

We omitted most of the section referred to here and integrated it with the section above.