Final Author Response to Referee Comments on "Implications of carbon saturation model structure for simulated nitrogen mineralization dynamics"

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We thank both anonymous referees for the generally favorable reviews and for their constructive comments and critiques. Both referees identified that we lacked a more basic introduction of the mechanisms of carbon saturation as well as a more robust discussion of how our results relate to the work of others. We also recognize the need to revise our hypotheses to be more explicit and to more clearly articulate many aspects of our work. We look forward to incorporating these comments into a revised manuscript. We respond in more detail to each referee comment below.

Anonymous Referee #1
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The manuscript by White et al. compares four different models, leading or not to the saturation of the soil stock of stabilized C, and the impacts of these models on N dynamics. The subject is important and seems suitable for Biogeosciences. The general presentation of the paper is suitable and the writing in itself is overall clear. The results are interesting. My two main concerns remain that (1) somehow, the paper is too much focused on the models so that in some parts the underlying mechanisms or the "real world" are not enough emphasized, and that at the same time (2) some aspects of the models could be better presented for the readers to fully understand them.

Response: Thank you for the favorable review and we appreciate your constructive comments. We look forward to improving our manuscript based on your suggestions. We provide a more detailed response to each item below.

1 I think the introduction should start by some general information on the mechanisms that could lead to saturation and the type of evidences we have for carbon saturation (there are already some information, but in my opinion not enough).

Response: We will add an introductory paragraph that describes the mechanisms and evidence for C saturation. Carbon saturation theory suggests that soils have a limited capacity to stabilize organic C and that this capacity may be regulated by intrinsic soil properties such as clay concentration and mineralogy (Hassink, 1997; Six et al., 2002). Clay mineral surfaces stabilize organic C through mineral organic complexes, leading to reduced C decomposition rates (Baldock and Skjemstad, 2000). As mineral surfaces in a soil become saturated with C, C decomposition rates increase, and the rate of soil organic C storage per unit of C input declines. This phenomenon results in an asymptotic response of soil organic C stocks to increasing C inputs (Stewart et al., 2007; Gulde et al. 2008; Heitkamp et al. 2012).

I think the discussion should be clearer about which of the four models is the most realistic according to the results or, more generally, about the impact of the results of the four models on what we think about the likelihood of the different mechanisms of saturation.

Response: We will attempt to clarify this in a revised discussion section. However, our intent was not to identify a single model that was most realistic, but rather to investigate the features of C saturation models that impose significant controls on N mineralization dynamics. It is likely that different aspects of each model will need to be combined in order to create a model that best represents reality. It appears that inclusion of a microbial pool is necessary to constrain the critical C:N ratio for N immobilization to realistic values for model applications where temporal dynamics of N immobilization-mineralization turnover are important. Whether or not C saturation should be modeled as a process that controls transfer rates or a process that controls transfer efficiencies remains an open question and will require experimental work. We do note, however, that using the saturation ratio to control the C transfer efficiency results in a soil texture control on N mineralization that is consistent with currently used non-saturating models such as RothC and Century.

The four models tested could be compared to other published models of C and N dynamics, at least in their formalism and structure. I think it would be interesting to refer for example to: Wutzler, T. & Reichstein, M. (2008) Colimitation of decomposition by substrate and decomposers – a comparison of model formulations. Biogeosciences, 5,749–759. Perveen, N., Barot, S., Alvarez, G., Klumpp, K., Martin, R., Rapaport, A.,Herfurth, D., Louault, F. & Fontaine, S. (2014) Priming effect and microbial diversity in ecosystem functioning and response to global change: a modeling approach using the Symphony model. Global change biology, 20, 1174-1190. Smith P, Smith JU, PowlsonDS et al. (1997) A comparison of the performance of nine soil organic matter models using datasets from seven long-term experiments. Geoderma, 81, 153–225. Even if these papers address other issues than C saturation. Somehow, this would also help replacing C saturation among other theories about C dynamics.

Response: This is an excellent suggestion, and we thank you for providing these important references. Carbon saturation is but one of many mechanisms that may control C and N cycling in soil. Interestingly, the microbial priming effect, which is the focus of two of the references above, is one mechanism that may have important relationships to C saturation dynamics. In fact, the definition of priming effects in a modelling context offered by Wutzler and Reichstein (2008), that 'decomposition of one soil carbon pool is influenced by the dynamics of another soil carbon pool,' extends to certain aspects of the C saturation models we present in our paper. While our C saturation models do not directly include the microbial biomass pool size as a control on decomposition rates, the behavior of our models in some ways mirrors that of the priming effect models. For example, the behavior of the priming effect model SYMPHONY (Perveen et al., 2014), where an increase in C inputs led to an increased mineral N content, N leaching, and a decrease in the rate of soil C storage are consistent with our findings of how certain saturation model structures can behave. Because priming effects and C saturation are not mutually exclusive processes, a logical next step in the harmonization of biogeochemical models would be to test for interactive and synergistic effects when both C saturation and priming effects are included in a model formulation.

2 The models are sometimes difficult to follow. It would be helpful:

+ to have a table defining all variables and constant parameters and giving the corresponding symbols

Response: Done

+ in my opinion the C/N ratios should be referred to in the equations by a single symbol and not something as "C:N". Just for the sake of clarity.

Response: In a revised manuscript we will replace C:N with the abbreviation ' r ' and use a subscript to reference different pools.

+ the introduction starts too quickly with equations and symbols that are not really defined. And I think Fig. 1 is notquoted. I think it would be useful either to leave the same information in the introductionbut in a less mathematical way, with more explanations on the underlying mechanisms,or to move the information or its mathematical part in the Methods. In the same vein,the introduction could describe more clearly the type of carbon pools involved, to give the reader a clearer idea about the real pools modeled in Fig. 1.

Response: We will add a new paragraph to the introduction that explains more clearly the mechanisms of carbon saturation and the pools involved. However, we feel the mathematical formulations are still an important part of the introduction, and are necessary to understand the rationale and hypotheses of our study. We will add additional references to Figure 1 for clarity and a separate table with all symbols.

+ I have difficulties understanding how the stoichiometric constraints are modeled. There is of course some information on this issue but I do not manage to make something consistent with it. Are the C/N ratios fixed (apparently not) or the results of the parameter determining fluxes? Potentially microbes could be N or C limited which should change the equations and lead to different sets of equations. Maybe, Table 2 should also give the equations for the N compartments. These aspects (formalism of the stoichiometric constraint) are likely to be critical for the coupling of C and N dynamics and are not fully discussed in the Discussion. What is the C/N ratio of the inputs? Could it be useful to test the effect of this ratio on C and N dynamics?

Response: The C:N ratios of all pools are fixed except for the plant residue inputs pool, which is a variable input parameter. We will clarify this in the revised manuscript. In the case of N limitation to microbes, we specify that decomposition rates will decrease according to Eq. 11 on page 9675, line 15. However, because the investigation of N limitation and feedbacks to C cycling was not the expressed purpose of this study, we prevented N limitation from occurring in any of our modelling exercises by providing a sufficiently large mineral N pool at the beginning of the simulation run. We will make this point clearer in the revised manuscript. The question of N limitation and feedbacks to C cycling and the implications of C saturation models on these feedbacks is certainly an interesting question, and we briefly elaborate on this topic in a response above and will include it in a revised manuscript. Further detailed analysis and discussion of this topic is best left to a different study, however.

We understand the request to include equations for the N compartments in Table 2. However, the N fluxes are already specified in Figure 2, allowing readers to infer the equations for each N compartment if needed. Therefore, we do not think it is necessary to add the equations to Table 2.

The C:N ratio of the residue inputs is variable and can be specified in each run of the model. For example, in the residue addition simulation, we specify the C:N ratio of the inputs to be 60. Testing the effect of different C:N ratios of the input residues on C and N dynamics is certainly useful. We did this through an indirect approach by analytically solving for the C:N ratio of inputs that would cause N immobilization for each model (the 'critical C:N ratio') and explored how varying the saturation ratio and clay concentration of a soil affected the critical C:N ratio in each model structure. We think this is a more efficient approach than testing the effects of different C:N ratio residue inputs directly. The critical C:N ratio is a useful threshold that indicates how residues with different C:N ratios would behave in each model (eg, residues with C:N above the critical C:N would immobilize N while residues below the critical C:N would mineralize N).

+ As far as I understand there are constant inputs of organic matter so that C and N are constantly entering the ecosystem (but the equations for the Cr compartments are not given in Table 2). There are C outputs via respiration and CO2, but there is no N output (denitrification, leaching). This leads necessarily to an increase in the ecosystem content in N and impacts C dynamics. Is that realistic? Could it not bias the results?

Response: In a full ecosystem model, it would not be realistic to have an inorganic N pool with no N transformations or losses. For the purposes of our theoretical investigation, however, N transformations and losses are beyond the scope of the study, so we use the inorganic N pool simply as a holding place to represent the cumulative N mineralization and immobilization during the course of the time series modeling exercise which was only run for 365 days. Because the feedbacks of N cycling on C cycling were not the primary purpose of our study, we did not include any of these potential feedbacks in our model. We will add a paragraph describing these points in the revised manuscript. We will also add the equation for Cr to table 2 for clarity.

+ the different compartments of Figure 2 could be labeled / named

Response: We will attempt to add the full compartment names to a revised figure 2. However, if the labels cannot be fit or make the figure overly busy, we may need to omit full labels and favor clarity. We note that the compartments are currently labeled with abbreviations that are consistent with the abbreviations in the text. The abbreviations are also specified in the figure caption.

Anonymous Referee #2
Received and published: 30 July 2014
About the Reviewer: I am a plant-soil ecologist with a broad experience in field experiments, not in modeling. The Editor asked me to look at this article from a modeling-'layman' point of view,

since the trans-disciplinary scope of the journal requires modeling papers to be accessible to both modeling as well as non-modeling scientists. My background has probably coloured my suggestions for improvement of your article. I have full confidence that you yourselves will be the best judge of which comments are useful to implement and which are not. All the best.

General comments:
In this article the implication of ’carbon saturation model structures’ for simulated nitrogen mineralisation dynamics are discussed (the title is very appropriate). Three C saturation models of increasing complexity are compared to the non-saturating RothC model. All models are coupled with an N The paper fits well into the scope of the journal although the interdisciplinary aspect could be worked out more (link between the modeling work performed in this paper and empirical field-research).The approach seems mostly straightforward and the results are interesting and relevant, but unfortunately this did not become entirely clear to me when reading the abstract, introduction or discussion. I strongly suggest the authors to spend more time on restructuring these chapters, since I believe this could be a much better article than it currently is. Providing clearer hypotheses that cover all the performed work (or excluding the work that does not directly follow from the hypotheses) and structure the article accordingly would improve the readability.

Response: Thank you for the generally favorable review of our manuscript. We appreciate your constructive comments and look forward to implementing them in a revised manuscript.

p. 9668 Abstract:
Weak points: Unbalanced (some results explained, others not at all); structurally hard to follow; unclear what exactly the approach was, i.e. unclear what ‘existing models’ they are referring to and how exactly they adapted these; the main approach seems to be that they added a second pool to an existing model, yet the rationale behind this adding of a second pool is not introduced at all, nor how this relates to their main aim(improving understanding of how C saturation affects N cycling) and why this is new; and last but not least: no clear hypotheses formulated (start with that one).Please formulate a clearer main aim, the phrase in the text closest to a main aim is:’How C saturation affects N cycling only weakly understood.’

Response to comment above and all abstract related comments below: Thank you for pointing out the weaknesses of our abstract. Given the lengthy list of suggestions, and the necessary restructuring of our hypotheses, it is clear that our abstract will need to be completely rewritten. We will use your comments above and below as guides to writing a new abstract for the revised manuscript.

l. 6 ‘C&N cycling tightly coupled in biogeochemical models’: specify (in which?) From the abstract it is not clear that non-saturation models are prevailing in literature.

l. 7. It’s not entirely clear to me why this is ‘Thus’?

l. 9 ‘Saturation models ‘proposed in literature’ Vague. Name them?

l. 10-11 ‘such as clay content’, what intrinsic properties are was explained before (l. 3-4), not necessary to repeat.

l. 11 What is meant by ‘current’, available? actually present?

l. 11-12 Can be written more clear and conscise, e.g ‘The C-saturation ratio of a C-pool (actual C : max Cstorage)(Cs/Cx)’ Note: Cs/Cx is mentioned for the 1st time on p 9673. Would be clearer if this notation is given already in abstract, or at least in the introduction.

l. 14 It’s not clear to me in which (type of a) base model you propose to ‘implement’ C-saturation in; or do you mean ‘how Cs/Cx is used in the model’?

l. 14 It’s not clear to me where ‘number of pools’ came from, what is the rationale for doing this (has not been mentioned earlier in the abstract) - Hence it’s not clear to me what is new about this and how it relates to the main aim.

l. 15 To make it easier for the reader to understand where the results section starts please use a signal phrase? After explaining the theory and rationale behind your study and your (now missing) research questions/hypotheses, please add something like ‘Our findings show that. . .’

l. 16 ‘C-saturation affected N mineralization’ please indicate direction

l. 25 ‘to represent short-term storage and turnover of C and N in microbial biomass’ -> this sentence could be used earlier in the abstract where the ‘adding a second pool concept’ and the rationale behind it is introduced (now completely missing) Transfer rates result explanation is missing, unbalanced abstract. Please give hypotheses in the abstract and cover also the comparison with RothC.

p. 9669 Introduction:
l. 1 C-saturation theory not explained, it’s only stated THAT it has fundamentally changed our understanding of C storage in soils, but not HOW.

Response: We will add a new paragraph that briefly describes the mechanisms of C saturation in soils and the implications for C storage. Briefly, clay mineral surfaces stabilize organic C through mineral organic complexes, leading to reduced C decomposition rates (Baldock and Skjemstad, 2000). As mineral surfaces in a soil become saturated with C, C decomposition rates increase, and the rate of soil organic C storage per unit of C input declines. This phenomenon results in an asymptotic response of soil organic C stocks to increasing C inputs (Stewart et al., 2007; Gulde et al. 2008; Heitkamp et al. 2012).

l. 5-7 In models that couple C & N cycles, C fluxes drive N mineralization – maybe provide names of models that do this? - is Manzoni and Porporato 2009 the only paper in which such models are described?

Response: Manzoni and Porporato (2009) is certainly not the only paper in which the mathematical formulations for the coupling of C and N are reviewed. However, it is a current *and comprehensive analysis of the different manners in which C and N cycling have been coupled by a wide range of models in the literature. To prevent any misunderstanding that Manzoni and Porporato (2009) is not a model per se, but a review of the modeling literature, we will amend the citation to state 'reviewed by Manzoni and Porporato, 2009.'*

l. 8- the C-model? the C&N model of Manzoni and Porporato? Which model are you referring to exactly?

Response: We are referring to a C model in the generic sense. To clarify this, we will change the wording from 'the C model' to 'a C model.'

l. 10 'Little attention': is more than no attention: please give the reference for the little attention or be more explicit about being the first to ever do this.

Response: Done

l. 13 'The majority of biogeochemical models that couple C & N cycles use linear C models with no saturation' - please give some examples

Response: We again prefer to reference the review by Manzoni and Porporato (2009), which indicates slightly more than 50% of contemporary C models are linear in structure. We will also add references to Rothamsted C (Jenkinson, 1990) and Century (Parton et al., 1987) models to the revised manuscript.

l. 21- what is meant by 'transferred' here? the transfer of C from decomposing C pool (e.g. fresh litter?) to receiving C pool (e.g. SOM?) or decomposing pool: fresh litter; receiving pool: soil microbes? or decomposing pool: soil microbes; receiving pool: respiration? or decomposing pool: soil microbes A; receiving pool: soil microbes B?

Response: We will clarify the meaning of this statement to indicate that transfer efficiency refers to the proportion of decomposed C from a given pool that is transferred as organic C to a receiving pool rather than being respired as CO_2. We will also introduce language to indicate that the concept of transfer efficiency can apply to any C flux that occurs between pools in a model. The specific fluxes between pools (eg fresh litter to microbial pool or fresh litter to soil pool) will depend on the structure of any given model.

l. 25- (Fig.1a) regulating eta as a function of the C-saturation ratio (the ratio of the current C to that of a putative maximum C level of the saturating pool) Could you give a clear definition of eta in the abstract and stick to it throughout the text?

Response: We will clarify the definition of ε where it is introduced on line 21. Briefly, ε is the proportion of decomposed C that is transferred to a receiving pool as organic C as opposed to being respired as CO2.

p. 9670

l.3-4 please shortly explain how/why eta and k decrease when saturation increases. Eta and k expressed as functions of C-sat ratio? Otherwise the text is unbalanced, since for the ‘transfer efficiency C models it is well-explained (eta as a function of Csatratio). Additionally, shortly explaining this for the models where C-sat ratio is used to regulate transfer rate as well would clarify the differences between the two model types, which seems essential in the text.

Response: We will briefly clarify these points in the text, and also add an extra reference to Fig. 1, which we believe provides a satisfactory illustration of the concept. Briefly, when the saturation ratio increases, ε and k effectively decrease because they are regulated multiplicatively by the function (1 – Cs/Cx). In the case of ε, the concept is that a given molecule of organic C is at a given time susceptible of being stabilized as organic matter (e.g. in an organo-mineral aggregate) or metabolized by microorganisms. As the saturation ratio increases, the probability of it being metabolized by a microorganism increases and therefore the "effective" ε decreases because anytime that microbes are conceptually involved in the process the assumption is that there is a respiration cost. In the case of k, transfer to the stabilized soil C pool is slowed as the pool approaches its saturation capacity.

l. 19 ‘It is important..’- important to specify which transfer? - is this not also the case for Eq. 1? (and everything else discussed in this paper?)

Response: Here we are referring to a transfer in the generic sense, in that it could apply to any individual transfer between two pools in a model. The same also applies for Equation 1, which is a generic description of N mineralization during the transfer of matter between any two pools in a model.

l. 25 ‘In only one case is the microbial pool explicitly represented..’ - maybe nice to mention other models in which a microbial pool is included (and what the effect of this is) even when these are not saturation models?

Response: This is a valuable comment, but rather than list other models with a microbial pool, we will provide additional context by citing the review of Manzoni and Porporato (2009) which found that 60% of contemporary C models include one or more microbial pools.

p. 9671
line 4-19 This whole paragraph is a bit unclear to me: it starts with how the clay factor ‘implicitly links’ non-saturating and saturating C-models. Then it continues about how the clay factor is used to calculate the saturating pool. It continues about the Csaturation ratio as a better way to predict C-retention than fclay. . .but then it suddenly ends with ‘whether non-saturation and saturation models differ in their representation of N-cycling has not been fully explored’. I don’t see how exactly this last sentence follows from what is explained in the paragraph.

Response: We agree that the final sentence of this paragraph requires revision to improve the clarity and logic of our argument, and we will do so in the revised manuscript. To briefly clarify, despite the commonalities in how f_{clay} *controls N mineralization in both saturation and non-saturation C models, the behavior of N mineralization in these two types of C models has never been formally compared in the literature.*

l. 20 'In summary, linking of N dynamics and C saturation theory is relevant'- Please state more clearly what will be improved by this linking.

Response: We will thoroughly revise this paragraph in the final submission. We will more clearly state that what will be improved by our work is the understanding of how different methods of implementing C saturation in a model (whether through regulating ε or k) affect N mineralization dynamics, how inclusion of a microbial pool in a C saturation model affects N mineralization dynamics, and how N mineralization coupled to a C saturation model compares to N mineralization coupled to a non-saturation model.

Hypotheses: l. 23 'The structure and the parameterization of different C models will affect the dynamics of a coupled N mineralization model.' - 'a' coupled N model? Which coupled N model? Coupled to what, to the different C models? But then they are not C models but C&N coupled models?

Response: In the methods section of the paper we describe in detail the N mineralization model that is coupled to each C model. We do not think that the hypotheses statement is necessarily the place to further elaborate on the details of the coupled N model. Nonetheless, and in line with the reviewer point, in our revisions we will be more explicit that this is a study about the coupling of C and N models, not just C models.

l. 24 'We propose that each model will have characteristic N mineralization immobilization dynamics that will reflect both the model structure and the consideration or not of C saturation.' - How is this different from the previous? - What exactly is meant with 'will reflect both the model structure and the consideration or not of C saturation. Do you mean that the N mineralization dynamics will be different depending on which model structure was applied(specify options) and whether or not C-saturation (the C saturation ratio?) was taken into consideration? The hypotheses do not cover all work presented in the article. Please be more precise.

Response: We clarified and sharpened the three specific hypotheses of the study. First, we hypothesize that the method used to implement C saturation in a model, either through regulation of transfer efficiency (ε) or transfer rate (k), will affect N mineralization dynamics. Second, we hypothesized if including or not a microbial pool through which C and N must pass during fresh inputs decomposition will affect N mineralization dynamics in models with C saturation. Finally, we hypothesized / analyzed if using ε to implement C saturation in a model results in soil texture controls on N mineralization that are similar to those currently included in widely used non-saturating C and N models.

p. 9672 Methods:
2.1 Structure of the carbon models - slightly restructure paragraph with suggestions below - it would have been more clear to me if a phrase like 'we focused on three C-saturation models with increasing complexity' was used in abstract, hyps, methods.

Response: Thank you for this suggestion. We will add a phrase such as this to the methods section and elsewhere.

l. 18 'We parameterized . . .' -Why? Because this is a much used model and generally accepted as giving realistic values? please shortly elaborate.

Response: Yes, we used parameter values from RothC as defaults in the three C saturation models when possible because RothC is a widely used and well calibrated C model. We will elaborate as such in the revised manuscript.

l. 19 'The turnover rate of soil C (ks) in the single-pool saturation model and that of microbial C (km) in the microbial and 2 pools microbial are [also] taken from RothC. - add 'also'? or if possible, merge sentences, start this subparagraph with: 'all values for turnover rates of all pools in all three C-saturation models were based on RothC'. In the two complexer C-sat models only, ks was derived. - Why the exceptions? Shortly elaborate on the reasoning behind them. The differences between similar looking (but differently calculated) elements of the different models could be shortly highlighted in this section (Cmicrobial; eta); that would make the differences between the models more clear and the results easier to understand.

Response: Thank you for identifying the lack of clarity in this paragraph. In considering your comments, we realize that it is premature to introduce a discussion of how the turnover rates were determined for each model when the model structures and parameters have not even been fully introduced yet. We think this topic can be more clearly described in sections 2.1.1 through 2.1.4 where each model is described in detail. We hope that this change will improve the clarity of the manuscript.

2.1.1 Single-pool saturation model - saturation ratio is defined here for the first time (after having been mentioned already very often in the preceding text) - Nicely put into context (l.10-16) - whole paragraph is very clear.

2.1.2 Microbial saturation model - paragraph is well-written

2.1.3 Abiotic saturation model -l. 12 desportion (desorption) - all clear

2.1.4 Rothamsted C model - paragraph very clear

Response to the 4 comments above: Thank you.

2.2 Modeling N mineralization I don't understand what you actually did? 'We coupled' suggests all models were C-only and the authors coupled C&N in all 4 models used? Added eq. 1 to all used models? Is this correct? If so, please say more explicit? If not, please specify. For clarity, would it help to add a phrase similar to: (p. 977 l.16) 'Analytical solutions to C:Ncrit were calculated for each model by substituting the parameterization of eta for each model into Eq.2' to this paragraph?

Response: Yes, all models were C only to begin with, and we coupled a simple N mineralization model to the 4 C models using the convention described by Eq. 1. We will clarify this point in the text, and also add a reference to Fig. 2 where the coupled C and N models are diagrammed in detail.

Just wondering: is it realistic that there is no N leaving your system? No plant N-uptake or leaching? Does this not mean that the N stock of the soil of the soil will increase continuously?

Response: In a full ecosystem model, it would not be realistic to have an inorganic N pool with no N transformations or losses. For the purposes of our theoretical investigation, however, N transformations and losses are beyond the scope of the study, so we chose to use the inorganic N pool simply as a holding place to represents the cumulative N mineralization and immobilization during the course of the time series modeling exercise which was only run for 365 days. We will add a paragraph describing this point in the revised manuscript.

2.3 Modeling exercises 2. Calculated the C:Ncr for a range of clay and saturation ratios- ranges based on what? (and, can 2 values be considered 'a range'?) The Ni pool was initialized to a size of 0.05 Mg N ha-1 - based on. . .?

Response: The range of clay concentration spanned from 5% to 80% and the range of saturation ratios spanned from 0.01 to 0.99, both in a continuous fashion (See Fig. 4). We are not sure which '2 values' you are referring to in your question about the range, but it is possible that you have confused the two clay content values used in modeling exercise (iii) with the continuous range of clay contents we used in modeling exercise (ii). In modeling exercise (iii), the N_i pool was initialized to 0.05 Mg N ha^{-1} in order to prevent N immobilization from drawing the N_i pool to 0 and slowing C decomposition as a result. We will clarify the reason for this initial value in the revised manuscript.

p. 9676 3. Results:
l. 14-15 I got confused here about whether Cm is saturating or non-saturating. (also on p. 9677: l.4 Is it only the abiotic saturation model in which Cm is non-saturating but the Cm is saturating in the microbial C-sat model)?

Response: Yes, in the abiotic saturation model C_m is non-saturating while it is saturating in the microbial saturation model. We will add a clarifying sentence.

l. 15'in all other instances' meaning: the Cs pool in the RothC model? Or in the RothC model AND in the [C-sat 2 pools] Cun pool? Or all pools in the RothC model and the [C-sat 2 pools] Cun pool? Or something else? Please specify. - Why does the [C-sat 2 pools] Cun behave different than the Cm in the Csat models? Because it is also a non-saturating pool? Please shortly elaborate. Maybe just say: 'all saturating C pools (in the saturation models: Cs and Cm) saturate but all non-saturating C pools respond linearly (Cun in the C-sat models and all C-pools in RothC?) to increasing C inputs.' Like this it seems to be stating the obvious (which it kind of is) but at least it is not stating-the obvious in an untransparant manner.

Response: We will clarify the text to be more obvious and transparent with the results.

- ‘depending on the model structure’ alone is not clear enough to me because it does not explain why Cun does not saturate in the Csaturation models.

Response: We will add some clarifying text to explain the specific component of the model structures that control the saturating vs. non-saturating behavior of each pool. Briefly, the saturating pools behave as such because the C transfer efficiency (ε) to each of these pools is regulated by the C saturation ratio. As C saturation increases, more C is respired as CO_2 in the transfer and less is retained by the receiving pool. In the abiotic saturation model, C_m and C_{un} are non-saturating and respond linearly to increasing C inputs, as do all the pools in RothC. This is because the ε to these pools is a fixed value.

l. 18-22 belongs to methods section?

Response: We agree and will delete the methods content and more clearly articulate the effect that f_{clay} had on the various pools.

p. 9677
l. 2 (and rest of the text) I would prefer ‘RothC’ model was used consistently instead of ‘non-saturation model’ and ‘RothC’ interchangeably. Just be very clear in abstract, intro and methods that the RothC model was chosen as a representative non-saturation model for comparison with the C-sat models and then stick to calling it RothC in the rest of the text. Possibly with the adjective ‘non-saturating’ fo clarity every now and then.

Response: Done.

What seems to be missing from the design is a model with an extra C pool but in which C-sat is still used to regulate eta? Because in the design as it is, the effects of: a) using C-sat ratio for calculating k instead of eta; or b) the addition of an extra pool (e.g. the two modifications in the ‘abiotic’ C-sat model as compared to the two less complex C-sat models); or even three if C) the Cm pool is non-saturating in the ‘abiotic’ model but not in the ‘microbial’ model. Hence as far as I can see no clear conclusion can be draw about which of these two (three?) factors caused the differences/similarities between the C-sat model outputs? Please convince me otherwise.

Response: There are certainly additional model structures that could be added to this paper which would allow readers to observe the effects of controlled changes among the models. However, adding additional model structures comes at the risk of increasing confusion as readers would need to keep even more model structures organized in their minds. We think that four model structures illustrate the main features of each model and the differences among them. Furthermore, we think the detailed illustration in Fig. 3 of how the C pools in each model behave as a result of the model structure provides a suitable level of detail to interpret the factors that lead to differences and similarities between the model outputs in terms of C storage. Finally, in our analysis of N mineralization, which is the primary topic of the paper, the solution to the $C:N_{cr}$ involves only ε, and is independent of the number of pools in the model. So the suggestion of adding a new model with an extra C pool where C-sat still regulated ε would have redundant results to the models currently included in the paper.

3.2 Structure of the section could be clearer, maybe add a sentence at the start e.g. ‘Both C:Ncrit as well as mineralization rates were affected..’

Response: Done

l. 24-25 this is the 1st time eta in the abiotic C-sat model is called ‘growth efficiency of the microbial pool’. It’s nice, maybe do this already earlier in the text?

Response: We will add the term microbial growth efficiency in the introductory text where ε is first described.

l. 27 I understand that this factor fclay features in both model structures but I would like to see the underlying question formulated as a hypothesis in the introduction, with a more biological rationale that ‘it’s a factor in both models’.

Response: This is an excellent suggestion. We will reformulate our hypotheses statements and include a hypotheses that using ε to implement C saturation in a model results in soil texture controls on N mineralization that are similar to those currently included in widely used non-saturating C and N models. We will also revise the structure of the results section to more closely align with our revised hypotheses.

l. 15-25 – The C:Ncrit for RothC should be discussed shortly (otherwise unbalanced) before moving on to fclay

Response: Based on the comment below, we will clarify the linkage between the paragraphs where $C:N_{cr}$ of the saturation models is compared to the $C:N_{cr}$ of RothC. This should address the concern about the results section seeming unbalanced.

l. 26 Maybe start this paragraph with a clarifying linking phrase, e.g. ‘After finding the analytical solutions, the C:Ncr were used to compare C:Ncr for all models at different fclayÂ´l

Response: Done.

p. 9678
l. 11- 13 ‘For all models the total N mineralized at the end of 1 year was equal to ..’ Why is after 1 year all N mineralized equal to the quantity of organic N inputs? Why does that not depend on the rate? If the rate is very small could it not take longer than a year?

Response: The reason that N mineralization equals the quantity of organic N inputs is because all pools were initialized to steady-state levels prior to the simulation. Under steady-state conditions, N inputs will equal N outputs because the size of each pool returns to its original starting size at the end of the 365 day simulation (with the exception of the Ni pool, which in our model is a collector for N outputs). The implication of the pools returning to their starting sizes *is that outputs from the pools will equal the inputs to the pools. The rate certainly does affect the amount of time that N from any cohort of litter additions resides in a given pool, and this directly affects the size that the pool will reach under steady state conditions. Within the 365 day timeline that we run our simulation for, N will mineralize from the cohort of litter added at the beginning of that year as well as from the litter still remaining in the pool from previous year's additions.*

Discussion:
Maybe follow a standard structure for the discussion in which each hypothesis is discussed individually. Please formulate (precise) hypotheses for all addressed questions (covering all modeling exercises, discussed in this article, including all 4 models and all factors) at the end of the introduction and refer to this structure in your discussion. This will improve the paper: as it is, it is unclear which hypotheses are being tested with your 'abiotic' C-saturation model and why; the 'clay factor' does not feature in the introduction but plays a large role in the results; which hypotheses are being tested (and the rationale behind them) with the comparison between RothC and the other models is in the current state of the article too vague. - Please check your text for inconsistencies like this.

Response: This comment led us to entirely revise our hypotheses statements as follows. First, we hypothesize that the method used to implement C saturation in a model, either through regulation of transfer efficiency (ε) or transfer rate (k), will affect N mineralization dynamics. Second, whether or not C saturation models include an explicit microbial pool through which C and N must pass will affect N mineralization dynamics. Finally, using ε to implement C saturation in a model results in soil texture controls on N mineralization that are similar to those currently included in widely used non-saturating C and N models. We will revise the structure of the discussion section to align with these hypotheses.

I would enjoy a (very short) overview in the discussion of which other C-N coupled/+ microbial pool/+ carbon pool models exist and how these are different from the models discussed here.

Response: We appreciate this comment and also recognize that it is similar to a comment made by reviewer #1. We will add this to our discussion. Briefly, our model illustrates that controls on ε have important implications for N mineralization. Carbon saturation is only one of several controls on ε that have been used in models (Sinsabaugh et al., 2013). For example, temperature effects on microbial physiology (Allison et al. 2010), N content of litter residues (Manzoni et al., 2008), and microbial N limitation (Schimel and Weintraub, 2003) are all controls on ε. In some cases these models have been coupled to an N cycle and the implications of the controls on ε have been assessed (e.g., Schimel and Weintraub, 2003). In other cases, the implications of the control on ε for a coupled N cycle remain untested (e.g. Allison et al., 2010). Our work illustrates the importance of testing changes in a C model on a coupled N cycle. In order to advance the use of new C model structures in coupled ecosystem models, the accuracy of N cycling needs to be considered.

p. 9679
l. 2-3 'can misrepresent' –shortly explain how

Response: The misrepresentation manifests itself as N mineralization from high C:N ratio litter inputs, which would be expected to cause N immobilization rather than N mineralization. We will clarify in the revised manuscript.

l. 8 These findings 'suggest appropriate ways to structure' Vague. Which ways. Please rephrase.

Response: Thank you for pointing out the vagueness of this statement. Because these are introductory remarks to the discussion section, we did not think it was the appropriate spot to elaborate on the appropriateness of different model formulations. Rather, we will rephrase our statement to indicate that our work revealed important implications about how the structure of C saturation models affects N mineralization.

4.1 Temporal scale & N mineralization dynamics The addition of a microbial pool addition is logic, and could follow from a clear hypothesis. The 'abiotic' C-saturation model does not seem to follow-up on the microbial model entirely logically, couple of steps seem to be missing: too many factors changed to be able to draw conclusions on differences between 'abiotic' and 'microbial'? Which hypothesis is tested with the 'abiotic'?

Response: We will revise our hypotheses such that the logic of testing the different model structures is more evident. Briefly, testing the abiotic structure falls into the hypothesis that asks whether there is a difference in N mineralization between models that implement C saturation by regulating the C transfer efficiency vs. models that implement C saturation by regulating the transfer rate to the saturating pool.

l. 15-16 What is meant by 'three cycles of microbial predation'? Three generations of microbes? Three time steps?

Response: By three cycles of microbial predation, we mean microbe A being eaten by Microbe B, microbe B being eaten by microbe C, and microbe C being eaten by microbe D. In models designed to operate at short time steps (daily to monthly), each cycle of microbial predation could be approximately equated to a time step of the model. In models operating at longer time steps (1yr or greater), this cascade of microbial predation must be lumped into a single value for C transfer efficiency.

l. 13-23 Although most of this paragraph is well-written and clear (it's obvious why this was a useful exercise), the paragraph starts with ' the 4 models we compared' and subsequently only discusses the single-pool model and the microbial C-sat model. Please correct this inconsistency. E.g. 'temporal scale and N mineralization dynamics' in the other 2 models are not mentioned. Clear formulation of hypotheses and structuring the discussion accordingly would have prevented this.

Response: Done.

p.9680
A step-wise approach would have made more sense for drawing of conclusions? e.g. single-pool model single pool model + microbial pool single pool + microbial pool + extra C pool (C-sat still

regulates eta) single pool + microbial pool + C-sat regulates k single pool + extra C pool single pool + microbial + extra C + C-sat regulates
k (I might be misstaken!)

Response: We think that the clarification of our hypotheses statements and the corresponding reorganization of the discussion sections will address the critique posed here.

With the above in mind, could you explain a little more how you can conclude from your exercise that 'The influence of C-saturation on Nmineralization dynamics depends on whether C saturation is modelled as a process regulating eta or k.'? E.g. how can you exclude the effect of the extra C-pool?

Response: Our conclusion that the influence of C saturation on N mineralization depends on whether C saturation is implemented by regulating ε or k is illustrated by the analytical solution to $C{:}N_{cr}$ plotted in Figure 4. Importantly, $C{:}N_{cr}$ is the critical ratio for N mineralization from plant residue inputs, therefore the analytical solution is calculated based only on the C:N ratio of the receiving pool for decomposing residues and the C transfer efficiency to this pool. The number, type, and parameterization of the pools downstream of the receiving pool do not affect the analytical solution to $C{:}N_{cr}$, so we are safe in excluding the effects of those pools.

p. 9681
l. 15-17 'The findings of . . ..abiotic saturation model.' - Please discuss this statement

Response: We will clarify this statement to state that the findings of these studies are consistent with the behavior of a C saturation model where the C saturation ratio regulates ε. In such a model, increasing C saturation would reduce ε, resulting in less N immobilization (as in Castellano et al., 2012) or greater N mineralization (as in McLauchlan, 2006).

l. 21-22 - As discussed earlier: rephrase - 'more fundamental' : vague.

Response: We will clarify this statement to state that C saturation theory provides a mechanism which could explain the findings of earlier studies where soil texture affected N mineralization.

l. 24 'may well be used': vague

Response: We will clarify this statement to indicate that the behavior of some of the C saturation models in our study is consistent with the early research that soil texture affects N mineralization.

l. 26 'Although. . .' - Please provide references

Response: Done

p. 9682
l. 3 'This level of saturation requires. . .' - what is easily achieved, the level of saturation or the high C inputs?

Response: The level of saturation can be achieved. We will clarify in revisions.

l. 5 ‘ Clearly, it will require experimental work . . .’ - One reference (Mazzilli et al 2014) for experimental work to validate your results seems quite meager, please elaborate.

Response: The Mazzilli reference is used in the prior sentence to illustrate a study that documents soils with a high saturation ratio, not as a reference to experimental work that validates the results of the models. Rather, we referenced 5 studies in the previous paragraphs to this which suggest that C saturation might affect N mineralization.

l. 8 ‘Given the limited. . .’ You are talking about this generation of hypotheses in plural throughout the text but you provide only one.

Response: In the revised manuscript we will clarify and elaborate on multiple potential hypotheses generated by our modeling study. Briefly, the hypothesis that C saturation is a mechanism that controls N mineralization is an overarching hypothesis that needs further testing. More specifically, we identify the hypothesis that C saturation controls the $C{:}N_{cr}$ *of decomposing residues. We also illustrate a potential hypothesis for an applied field experiment that follows from our study, that C saturation patterns in a soil profile with stratified organic matter, such as occurs in no-till agricultural soils, affect N mineralization dynamics.*

l. 10 Are you sure this hypothesis has never been tested in the wide range of litter decomposition studies? Effect of surrounding soil clay content on litter decomposition?

Response: There are certainly many decomposition studies that have addressed the role of soil clay content on decomposition and N mineralization. However, we are not suggesting to test the idea that soil texture controls N mineralization, but rather that C saturation is a control on N mineralization. In the literature that is currently available to assess this hypothesis, soil texture gradients can create a proxy for C saturation gradients. However, the effect of C saturation gradients on N mineralization within a constant soil texture remains an open question, to our knowledge. We will clarify the importance of testing C saturation gradients directly, rather than soil texture gradients, in the revised manuscript.

I would like to see a broader discussion, both for the modeling part as well as for the place of your findings in the field of soil-plant interactions. e.g. how does your work relate to ideas expressed in articles such as: Mycorrhiza-mediated competition between plants and decomposers drives soil carbon storage (Averill et al. Nature 2014) Persistence of soil organic matter as an ecosystem property (Schmidt et al. Nature 2011) Variable effects of nitrogen additions on the stability and turnover of soil carbon (Neff et al. Nature 2002)

Response: We appreciate the request to place our work in the context of broader work in soil-plant interactions. This is similar to a request from referee #1. We will add a discussion about the parallels between C saturation dynamics and priming effect dynamics (see response to referee #1). While the Averill et al. (2014) and Neff et al. (2002) papers suggested here are certainly interesting and important works, they deal primarily with N cycle controls on C *cycling, whereas our work focuses on C cycle controls on N cycling. Therefore, we do not think that these two articles are necessarily the best examples of soil-plant interaction studies to discuss in our paper. Schmidt et al. (2011) is a good reference to support the need for a new generation of ecosystem models which take into account advances in our understanding of C cycling mechanisms, such as physicochemical stabilization of organic matter on mineral surfaces, and we will add it to our introductory remarks.*

A discussion of results involving the ‘Abiotic model’ and RothC model is lacking. Please correct this.

Response: Done

Conclusions: All three c-sat models can produce similar predictions of C-storage, but not of N-mineralization. ‘Inclusion of a microbial pool in the C-model led to reasonable predictions of N-mineralization’ l. 23 Specify in which ‘C model’

Response: Done

p.9683 l.1 ‘offer a clear pathway’: vague, please specify.

Response: Done

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