Interactive comment on “JULES-CN: a coupled terrestrial Carbon-Nitrogen Scheme (JULES vn5.1)” by Andrew J. Wiltshire et al.

Anonymous Referee #3

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This manuscript explains the N cycle in the JULES-CN model which forms the land component of the UKESM. Simulations from the UKESM have contributed to the CMIP6 effort. The N cycle component of JULES, as explained, here is very simple compared to existing models out there. This is completely acceptable as long as it is clarified that the model parameterizations are simple, their limitations acknowledged, and the implications discussed. I am afraid, however, that the manuscript doesn’t appear to do so and in my mind requires substantial work to address this and other concerns I raise below.

1 Major comments

I have several major concerns.

It is well known that leaf N content is related to its photosynthesis capacity (Field and Mooney, 1986). When CO2 increases, photosynthesis increases but this rate of increase is slowed if enough N is not available. This process is referred to as photosynthesis downregulation (McGuire et al., 1995). So, it is clear then, that N limitation acts on photosynthesis and thus on the gross primary production (GPP) flux. However, the approach used in the manuscript, in contrast, reduces the NPP (without adjusting the GPP) which is equivalent to reducing carbon use efficiency (CUE = NPP/GPP). Since there is no biological justification for this provided, I am struggling to understand the reasoning behind this. Also, if that framework is still used, TRIFFID models Vcmax as a function of leaf N content (eqn. 51 in Cox 2001) so it makes sense to adjust Vcmax.

Related to this concern, is the fact, that I am not able to find in the manuscript in detail how this reduction in NPP is implemented or how it results and because of the interaction of which processes. Unless I missed it, the only reference to this important process is made on line 78 as "... and then reducing plant net carbon gain to match available nutrients".

It is well known that current observation-based CUE is around 0.5. This is also seen in Figure 10. The CUE for the JULES-CN model is lower than that for the JULES-C model because that’s how JULES-CN is designed - to lower NPP and hence CUE as CO2 increases and N supply can’t keep up. I am wondering what happens in a future simulation for RCP 8.5 scenario. Will your CUE drop down to something like 0.25 by year 2100 which seems totally unrealistic? This will be one implication of your model design since you have chosen to reduce NPP and not GPP.

Agreed, it is well established that tissue level N concentrations correlate with photosynthetic capacity and metabolism. It is also established the first order effect of N fertilisation is enhanced growth. However, it is less clear on the mechanisms, for instance field experiments of enhanced N fertilisation have found increases in growth but no change in photosynthetic rate (e.g. Brix et al., 1969, Wang et al., 2012). Other analysis, looking at climatological gradients in N deposition found no dependency between foliar N and N deposition. It is, however, fair to say other analyses (e.g. Mao et al., 2020) do establish this link. In general, models to date make differing assumptions about these coupling mechanism between C and N cycles leading to substantial uncertainty in their projections (Zaehle and Dalmonech, 2011).

Our approach here is to capture the established first order emergent response of N addition on growth which translates into leaf area and biomass without the complex and uncertain impacts on leaf physiology. As we use a fully dynamic vegetation scheme Nitrogen availability can drive changes in plant level C:N ratios through competition. This is the first implementation of a coupled C-N scheme in the UK model and we fully expect to develop this aspect further including assessing flexible stoichiometry.

Further to the points raised above, not of all the CMIP6 models only UKESM and MPI-ESM 1.2 (Mauritsen et al., 2019) include a coupled fully dynamic vegetation model with nitrogen scheme. In both cases, the schemes assume fixed plant stoichiometry. In which case, if CUE dropped very low there would be a dynamic vegetation response leading to a dieback. In our CMIP6 experiments we do not see a strong reduction in CUE (over the course of ssp585 reduction from 0.53 to 0.48). Analysis of CMIP6 runs as part of C4MIP demonstrates we have a strong and robust representation of carbon feedbacks (Arora et al., 2020).

The second big assumption in the model is that of fixed C:N ratios of plant tissues. The implications of this assumption are not discussed. Since C:N ratio of plants varies in space (as indicated by different values of nl0 in Table 1) this indicates their ability to adapt to different environmental conditions in space. Assuming, plants can do the same in time as CO2 increases doesn't this imply that the assumption of fixed C:N is too strong and your model will limit NPP perhaps more excessively than it in the real world (with the caveat that in the real world GPP is constrained).

It is common for DGVMs to parameterise a top-leaf nitrogen content per PFT as part of the process of capturing diversity and functional traits. This is the case whether a full nitrogen cycle is included or not. This is common with the point raised in 1. It is likely that foliar N varies in space independent of nutrients as was found in the Aber study. This is not captured in the typical approach to dynamic vegetation modelling. It is entirely plausible that with increased nutrient limitation plants limit their foliar N and therefore GPP and NPP. In our approach, with fixed stoichiometry we may excessively constrain the model but through the dynamic vegetation response we might see a shift towards a plant with a lower C:N.

In context of model evaluation, it would have been extremely helpful to include a simulation in which N deposition is turned off. This simulation would have allowed to see if the effect of N deposition is indeed to increase NPP as would be intuitively expected.

This paper is just an initial description of the JULES-CN model which alongside other additional land surface processes has been implemented in UKESM. The JULES-C run implicitly gives an signal of the effect of N deposition. It has also been used within Davies Barnard et al. (2020, https://bg.copernicus.org/articles/17/5129/2020/bg-17-5129-2020.pdf) who explored the response of NPP to N and CO2 fertilization from perturbation experiments. More detail of results from the Davies Barnard paper have been added to a new discussion section.

In addition, the TRENDY model simulation S2 doesn't take into account land use change and the fertilization of crops. Crop fertilization is a major source of leaching and gaseous emissions of N2O and NOx. I am wondering if this is the possible reason that the simulated leaching in Figure 4 is so low compared to other estimates.

Yes, it is quite likely although we haven't explicitly quantified that. We have added a discussion about N fertilization and leaching into the leaching section.

Also, does the model simulate the realistic sign of response when driven with climate forcing only. Typically, a model's response to various forcings allows to see at least if the sign of the response is consistent with expectations.

These biogeochemistry only/ radiative only/no N deposition runs are available in TRENDY and C4MIP and will be assessed as part of our future work. However, we think it is beyond the scope of this paper to include a comprehensive assessment of these results. We will refer to these studies as part of the new discussion.

4. I realize that there are very few observation-based estimates available for N re- lated pools and fluxes. However, still there are plenty of quasi-observation and model based estimates against which model results could have been compared. For example, in Figure 4 there are no quasi-observed or model estimates for sev- eral quantities. Model estimates are, however, available for immobilization and mineralization (von Bloh et al. 2018), plant N uptake (Zaehle et al. 2010; Xu-Ri and Prentice, 2008; Wania et al., 2012), and inorganic N mass (von Bloh et al. 2018; Xu-Ri and Prentice, 2008; Wania et al., 2012). These estimates will allow to put your model results in some context.

These numbers have been added to figure 4 and it has been noted in the text that some of these comparisons are from other models rather than available observations.

5. Model parameterizations are not compared to other models, and the conceptual basis of parameterizations and their implications, are not discussed (as men- tioned above for the choice to reduce NPP and use fixed C:N ratios) .

We have significantly revised the model description section. We think our new description will address these concerns in sufficient detail.

For example, biological nitrogen fixation (BNF) is modelled as a straight-forward function of NPP. This is okay but the manuscript doesn't note that meta-analysis studies have found that BNF increases with increasing CO2 (Liang et al., 2016) but decreases with increasing N deposition and fertilizer application (Ochoa- Hueso et al., 2013) both of which apparently result in increase in NPP.

This has been added to the BNF section.

In addition, BNF is typically higher over agricultural areas.

BNF is, in effect, very high over agricultural areas in JULES, as nitrogen is not limiting for cropland areas and the source (fertilisation or BNF) does not affect the model outcome.

Similarly, all gaseous losses are expressed using Nturnover but in nature there are several pathways using which gaseous losses occur. N2O and NOx losses occur during nitrification (via nitrifer denitrification) and N2, N2O, and NOx losses occur during denitrification.

Not interested in losses to atmoshere but removing the appropriate amount of N

It would be scientifically beneficial for the manuscript, and for a reader, if sim- plifications made are clearly highlighted and their limitations discussed, because then it is possible to interpret the model results in light of these limitations.

We have significantly revised the model description section. We think our new description will address these concerns in sufficient detail.

6. The majority of the results shown in the manuscript focus on the ability of the new model to reproduce all the aspects of the C cycle as the previous model did. As a result, the N cycle module is not evaluated rigorously. The manuscript doesn't report N demand, how it changes over time, what part of the N demand is not met, what part of N demand due to increasing CO_2 is met by N deposition, time series of mineral N pool, time series of plant N uptake, time series of C:N ratio of whole plant and other plant components, and geographical distribution of simulated C:N ratios (even though I realize they are specified). Since this is the first time JULES' N cycle component is being published it is reasonable to expect that such a manuscript will rigorously assess the new N cycle module.

We have added a figure with time series of N demand for growth and spreading, N uptake for growth and spreading alongside the unmet N. Net N mineralisation, C to N ratio of litter,

7. There is no mention of phosphorus cycle at all. It is well know that in the tropics phosphorus limits photosynthesis and not nitrogen. How is this accounted for? My guess is this is somehow built into the Vcmax rates which are function of leaf N content (eqn. 51 in Cox 2001). If the model can reproduce correct zonal distribution of GPP it must take phosphorus limitation in the tropics somehow into account.

The effect of Phosphorus has been discussed as part of a future direction and understanding paragraph.

8. Finally, the lack of units, the lack of rate change equations for several pools, and unclear statements make it difficult to understand the model parameterizations as noted below in minor comments. In its current form, there is no way a reader can fully understand and reproduce the parameterizations reported here in some other model.

Units have been added to the relevant variables, additional rate change equations have been added. This has involved quite a few changes which are apparent in the document but hard readily document here. I think we have significantly improved the readability of the document. We have also added a nomenclature section as an appendix.

2 Minor comments

9. Abstract, line 8, "It represents all the key terrestrial nitrogen processes in an efficient way.". The word "efficient" here is misleading.

Change to parsimonious

10. Abstract, line 9, I find it extremely confusing that BNF is mentioned as an external input. BNF is how N enters the coupled vegetation and soil system. Consider the case, if we were to refer GPP as an external input since that's how C enters the coupled vegetation and soil system. N deposition and fertilizer, on the other hand, can be called external because they are not natural just like fossil fuel emissions.

Changed to “Biological fixation is dependent on productivity, with nitrogen deposition as an external input“

On page 2, in addition to BNF, leaching is also referred to as an external (loss). This also seems strange since on the carbon cycle side we don’t refer to heterotrophic respiration or dissolved inorganic C in runoff as external losses.

Changed to “Nitrogen leaves the vegetation and soil system via leaching and a bulk gas loss parameterisation”

11. Page 2, line 34, "Internally organic N is lost ...". Here "internally", perhaps is much better described as "cycling of N within the coupled vegetation and soil system".

Changed to: “Within the system organic nitrogen is transferred from the vegetation to the soil through the production of litter and disturbance”

12. Page 2, line 36, "Both inorganic and organic nitrogen may become available for plant uptake". Since organic N uptake is very small and therefore not even mod- elled (including in your model) perhaps it would be better if this is clarified.

Changed to : “, although the amount of inorganic N uptake by plants is small and typically not included in models”

13. Page 2, line 39. "In a changing climate, rising atmospheric CO2 drives an in- crease in the terrestrial carbon cycle and Gross Primary Productivity (GPP)." This is a vague sentence. What does "an increase in the terrestrial carbon cycle" means?

Changed to: "rising atmospheric CO2 drives an increase in the land carbon uptake and hence an increase in the gross primary productivity. This results in an extra demand for nitrogen which could potentially limit the increase in future carbon stocks”

14. Page 2, line 56, " ... are between a reduction of 39 % and a slight increase of 1% ...". Please consider rewording this sentence/phrase. It is somewhat hard to follow.

Changed to: “For example, \cite{doi:10.1111/gcb.15114} used a perturbed model ensemble to show that N limitation reduces both the projected future increase in land carbon store due to CO$_{2}$ fertilisation and the projected loss in land carbon due to climate change”

15. Page 3, line 65. " ... and a new managed land module ...". Please consider rewording to "and a new module for land management ...".

Done

16. Page 3, lines 72-74. "This is achieved by extending the implicit representation of nitrogen in the existing dynamic vegetation and plant physiology modules TO EN- ABLE A MORE COMPREHENSIVE NITROGEN CYCLE WITHIN THE LAND SURFACE". Please consider deleting the text in capitals given N cycle framework used here is extremely simplified.

Done

17. Page 3, Lines 74-75. "Nutrient limitation operates through two mechanisms; the available carbon for growth and spreading is reduced and the decomposition of litter carbon into the soil carbon is slowed". The word spreading at this point in the manuscript is unclear. Only after reading

the rest of the manuscript it is clear that "spreading" means changes in the spatial extent of vegetation. Please consider using another phrase/word to replace "spreading".

changed to “the available carbon for vegetation uptake is reduced”

Please also consider not using the phrase "decomposition of litter carbon into the soil carbon" here and elsewhere. Technically litter doesn’t decomposes into soil carbon. As litter decomposes it releases CO2 and the dead organic matter is broken into smaller more recalcitrant materials, which the models consider as soil carbon. In reality, of course, there is a continuum.

changed to “the decomposition of litter carbon is slowed”

18. Page 4, lines 114-115. "As standard, JULES-C includes an implicit representation of nitrogen which has been extended to be fully interactive.". A sentence or two about how nutrient constraints on photosynthesis are implicitly modelled in JULES-C will be helpful.

Additional text to explain the implicit scheme added - “The philosophy behind the developments described here is to produce a parsimonious model to capture the established first order emergent response of N addition on growth which translates into leaf area and biomass without the complex and uncertain impacts on leaf physiology. Our approach is therefore to simulate the large-scale role of N limitation on vegetation carbon use efficiency (CUE - ratio of net to gross primary productivity) and soil carbon turnover. This is achieved by extending the implicit representation of N in the existing dynamic vegetation and plant physiology modules to be fully interactive. At the core of surface exchange in JULES is a coupled stomatal conductance photosynthesis scheme parameterised in terms of the maximum rate of Rubisco carboxylation, V_{cmax} (mol CO$_2$m^{-2}s^{-1}). V_{cmax} has a dependency on the leaf N concentration. Similarly, plant maintenance respiration has a dependency on leaf, root and stem N concentration \citep{cox1998canopy,cox1999impact,cox2001,clarketal2011}. Implicit within JULES, even in simulations excluding the carbon cycle is the parameterisation of plant tissue level N concentrations and associated allometry \citep{gmd-13-483-2020}. Simulations with an interactive carbon cycle therefore assume that enough N is available to meet vegetation growth and turnover. Here, we simply limit growth if not enough N is available. To do this requires a full representation of the N cycle in the land surface including a coupled soil carbon-nitrogen and inorganic N scheme.”

19. Page 4, line 120. "The vegetation nitrogen component captures the nitrogen limitation on the C stock, and ...". As described here the N limitation acts on NPP which is a C flux and not on the C stock.

changed to “nitrogen limitation on the net primary productivity, and includes retranslocation”

20. Page 4, last sentence, line 126. " ... it slows the rate of litter decomposition INTO SOIL ORGANIC MATTER." Please consider removing the phrase in capitals.

Done

21. Page 5, lines 129-135. I felt, it is little too early to introduce the seven JULES- CN parameters given that at this point in the manuscript, the parameterization themselves haven’t been introduced.

Moved to the end of the model description section to summarise.

Also on line 130, Does " ... the effective solubility of nitrogen", refers to solubility in water.

Changed to “the effective solubility of nitrogen in water”

22. Section 3.1. It seems the model's roots are in fact fine roots (since Rc = Lc in eqn(3)), and coarse roots and stem are included in the Wc term. Please make this clear.

Done

23. For eqn (1) please specify the units of all terms. I suspect these are KgC m−2.

Added

24. For eqn (2) what are the units of σl and Lb.

Done

25. What are units of the individual terms in eqn (5) through (9) and the remaining equations.

Added

26. Page 6, lines 160-178. This entire section is based on Figures 2 and 3 which form the backbone of specified C:N ratios and their variation with canopy height. It would be extremely helpful to know the basis of these relationships.

Section 3.1 has been updated to make clear these are the existing relationships that are implicit in the JULES model. The basis for these is now given in the text. "TRIFFID employs fixed allometry such that the split between leaf, root and stem carbon are defined by a single state prognostic variable that defines the total biomass. Biomass density increases via growth and is reduced by litter production and competition. Biomass can also increase by spreading through an increase in covered area. Nitrogen is implemented to limit growth and spreading such that the change in vegetation nitrogen cannot exceed that available. This section documents the vegetation model starting with the vegetation carbon and nitrogen structure (\ref{sec:struc}) including the additional complexity of labile nitrogen (\ref{phen}). The following section describes how growth and spreading is limited by nutrient availability (\ref{sec:allocup}. The final section describes how vegetation carbon and nitrogen is turned over by disturbance and competition (\ref{sec:litter})."

27. Page 6, lines 180-181. "Biological nitrogen fixation (BNF) is ASSUMED TO BE THE largest natural supplier of nitrogen to the terrestrial ecosystem". Consider re- moving the words in capitals and including the word in bold. Fertilizer application is the largest anthropogenic N flux and BNF is largest natural flux.

Done

28. Page 6, line 181. "Following Cleveland et al. (1999), the nitrogen fixation is determined as a proportion of the net primary production before nitrogen limitation (NPPpot)". This is incorrect. Cleveland et al. (1999) parameterized BNF as a function of actual evapotranspiration (AET) not NPP.

While we concur that ET is the primary parameterization described by Cleveland et al. (1999), we refer the reviewer to page 637 of Cleveland et al. (1999): "NPP could also relate to N fixation; NPP may be a proxy for carbon potentially available to fixers. The relationships between N fixation and modeled NPP are depicted in Figure 2…". It would be remiss to not cite Cleveland as this parameterisation is directly related to that work. We have clarified in the text that NPP is the secondary model from Cleveland et al. (1999).

Also, NPPpot is not defined anywhere close to this equation where it is intro- duced the first time. The first definition of NPPpot occurs on page 9, line 242, as "NPPpot supplied to TRIFFID represents the potential amount of carbon that can be allocated to growth". Then a somewhat different definition occurs on page 19 which defines NPPpot as the NPP when nitrogen is unlimited. Isn't NPPpot just the NPP from the original framework without any reduction. I don't think, you do a calculation with unlimited N applied, per se.

Changed-We have revised the definition of NPP_pot to "potential amount of carbon that can be allocated to growth and spreading of the vegetation" and the response ratio to "the ratio of the potential amount of carbon that can be allocated to growth and spreading of the vegetation (NPP_{pot}) compared with the actual amount achieved in the natural state (NPP)" We have also added a sentence saying "the NPP_pot is defined in the same way as the net primary productivity in JULES before the explicit nitrogen cycle was included"

In context of BNF, and eqn (9), the parameter ζ is not listed in Table 1.

ζ is not dependent on pft so I don't think it is necessary to add it to table 1.

29. Page 7, Table 1. It would be extremely help if nl0 is inverted and written as 1 in units of Kg C/Kg N so that the values are easily comparable to C:N ratios reported in literature.

Changed – this statement has been added to the caption: "n_{l0} is the N concentration at the top of the canopy but is shown here as 1/n_{l0} so that it is comparable to expected C:N ratios from the literature."

Also, nl0 is listed twice in Table 1 and please consider rewording "Top leaf N concentration" to "N concentration at the canopy top".

Changed

30. Page 7, lines 188-189. "However, in JULES-CNlayered the vertical distribution of the fixed nitrogen in the soil depends on the root distribution ...". What does "fixed" refers to in this context. Also, at this stage in the manuscript it is not clear what does "depends on root distribution" means?

Changed to "However, in JULES-CN$_{layered}$ the BNF is distributed vertically in the soil depending on the fraction of roots in each layer. If a soil layer is frozen there is no BNF into that layer."

31. Page 7, lines 201-203. "This distinction is inconsequential in the carbon only mode but is more critical when considering nitrogen interactions as the implication is that at all times the plant has enough nitrogen in reserve to maintain full leaf". From here on it becomes difficult to follow the logic used in the model. I am not able to understand what does "the plant has enough nitrogen in reserve to maintain full leaf" means?

This section has been restructured and clarified.

32. Page 8, eqn. (10). I am confused here. Lb is introduced as a variable called balanced leaf area index but not explained what actually it means. In eqn (2), leaf C, LC is a function of Lb. In equation (9), leaf area index (LAI) (L) is also related to Lb through p. Somewhere here, there is the split of LC into labile and non-labile (the one which determines the actual LAI). Did I get this correct? How are L in eqn. (9) and LC in eqn. (2) related? Are they related through specific leaf area (SLA)?

This section has been updated. Now Lb is clearly defined, and it has been made clear this variable is the main mechanism that changing vegetation structure affects surface exchange. Units are now explicit.

33. All through up to this point in the manuscript, the rate change equations for the vegetation N pool are not presented. At this point in the manuscript, I am still unclear what "retranslocation" means. Is this the transfer of resorbed N from leaves before they are shed. If yes, to which plant components?

The whole section has now been updated and clarified. Retranslocation is nitrogen being moved from leaves to the labile pool prior to leaf fall.

34. page 8, lines 251-216. "During leaf-off the labile component is the equivalent of the retranslocated leaf nitrogen plus an additional store of nitrogen in preparation for the following bud burst". This sentence introduces yet another pool. It would be really helpful if all the pools and their rate change equations are properly introduced.

The rate change equations are included. The structure has been updated to clarify the pools and the implementation of rate changes.

35. Page 8, line 29. "The mean canopy nitrogen content is described by ...". Please reword this to "The vertical distribution of leaf N content in the canopy is described by ...".

done

36. Page 9, line 235. "Canopy Leaf C:N ratios are resultingly 44% higher than top leaf ratios". I am unable to understand this. Does "canopy leaf C:N ratios" refers to mean canopy leaf C:N ratio or the vertical profile of C:N ratios along the canopy depth starting from the top.

The mean canopy C:N ratio. Text clarified.

If leaf N content in the leaves at the top of the canopy is higher and decreases exponentially, and if C content is uniform than it implies that C:N ratio of leaves is lower in the leaves at the top of the canopy and higher at the bottom. Integrating eqn (12) over LAI yields ∫ L nl0 exp(−knz) dz = nl0 1 (1 − exp(−knL)) which implies that the mean C:N will depend on the LAI, L. So I am unclear where does the number 44% comes from.

Agreed. However, in the Mercado implementation there is no dependence on total LAI. z is the fraction of the canopy above a point in the canopy and is therefore independent of LAI. The implication of this is being explored elsewhere.

37. Page 9. Section 3.1.4. The term Λlc in eqn. (13) is not defined and only when the reader reaches eqn. (21) it is clear what this term is. Similarly for Λln.

Updated and clarified.

38. Page 10. Line 263. Λln is defined as the retranslocation of nitrogen from leaves and roots into the plant labile pool. I am not sure how does it relate to p in equation 10 which is also related to retranslocation.

Clarified in the text. Here, retranslocation is used to define the flux of carbon. In eq 10, the retranslocation coefficient is used to parameterise the labile store. Under the assumption that higher retranslocation corresponds to a greater store.

39. Where is Ψc from equation (13) defined? Is this what Ψ is in eqn. (17)?

Apologies, this was a typo and should be Ψg.

40. Page 10. Line 271. "The nitrogen available for growth is the total available nitrogen multiplied ...". Please reword this as "The nitrogen uptake used for plant growth is the total nitrogen uptake multiplied ...". I think, that's what is meant here. Available N sounds like the N available in the soil inorganic pool that can be potentially taken up by plants.

done

41. Page 10. Line 272. "Equations 13 and 15 are then solved by bisection such that the nitrogen uptake for growth (Φg) is less than or equal to the available nitrogen...". Do you mean the bisection method to find root of an equation? This and remaining part of this paragraph is difficult to understand since there is no Φg term in either equation (13) or (15).

This section has been updated to explain more clearly the solution to the equations presented.

In addition, since units of the various terms are not provided it is difficult to follow the equations on page 10.

42. Page 10, line 282. "... and Nv/Cv defines the whole plant C:N ratio ...". You mean Cv/Nv?

Now given as the inverse of the whole plant C:N ratio

43. In the absence of the competition module of the TRIFFID model properly de- scribed it is difficult for a reader to know what does "density-dependent litter production" and "density-dependent componennt for intra-PFT competition for space" means in Section 3.1.5. Please consider introducing this in a sentence or two.

Done

44. Page 11, please define Λc and Λn in words explicitly where the are first intro- duced. Λc was introduced in equation (22) but not defined until next page near eqn. (28).

Done

45. Page 11, lines 310-311. "The effect of nitrogen limitation on the litter carbon flux is captured in the excess carbon term Ψi". Throughout the manuscript there is no expression for Ψi so it's difficult to understand it. I do understand based on what is written in the manuscript that it the excess C that cannot be used. So it must be related to N uptake, allocation fractions for C, and specified C:N ratio of the three C pools.

Made it clearer in the text that the subscript, i, is used to indicate PFT levels and is defined in previous equations.

46. Page 12, line 339. "βR depends on soil texture". I don't think, this dependence can be too strong. Can you please mention the typical value of βR.

This line has been added: $(1-\beta_R)$ is the fraction of soil respiration that is emitted to the atmosphere - this depends on soil texture and ranges from 0.75 for a clay soil to 0.85 for a soil with no clay content

47. The rate change equations for litter and soil C pools are helpful. Similar equations for vegetation C and N pools would be so helpful.

The rate changes are in Eq 12 and 14. This section should be a lot clearer now.

48. Page 13, line 349-350. "Input into the BIO and HUM nitrogen pools comes from the total immobilisation of inorganic nitrogen into organic nitrogen where Itot = IDPM + IRPM + IBIO + IHUM ". Itot is divided into BIO and HUM pools. Since BIO is the microbial pool shouldn't all immobilization end up there.

In reality, carbon (and therefore also nitrogen) should go from litter pools -> microbe pool -> Humified pool (since HUM is made of microbial necromass). But in RothC, carbon can go straight from litter to HUM. Therefore the nitrogen fluxes must follow this as well. This sentence now says: "Following the framework of the RothC model, input into both the BIO and HUM nitrogen pools is from the total immobilisation of inorganic nitrogen into organic nitrogen where $I_{tot} = I_{DPM}+I_{RPM}+I_{BIO}+I_{HUM}$ (in kg\,[N]\,m^{-2} s^{-1})"

49. Page 13, eqn (33). Does the subscript i still refers to PFTs?

Changed i to p so soil carbon pools are represented by subscript p and vegetation pfts are always represented by subscript i.

50. Page 13, line 365. " ... the respired fraction (βR) and the C to N ratio of the destination pool ...". This is confusing since on line 339 (1-βR) was referred to as "the fraction of soil respiration that is emitted to the atmosphere".

This was a mistake in line 365 - the fraction respired to the atmosphere is (1-beta_r)

51. Page 14, line 371. " ... where i is one of RPM or DPM." Please use a different subscript here since you have used i previously to represent PFTs.

Changed i to p so soil carbon pools are represented by subscript p and vegetation pfts are always represented by subscript i.

52. Pages 13 and 14. The FN terms in eqn (36) limits the respiration of the DPM abd RPM litter pools. So it is unclear to me why FN would depend on IBIO and IHUM in eqn. (37).

Respiration is carried out by microbes so they won't decompose as much of the DPM/RPM pools if they haven't got enough nitrogen to convert that carbon into BIO/HUM. The total amount of nitrogen they have available depends on I_BIO and I_HUM because M_BIO - I_BIO (and similarly M_HUM-I_HUM) is the net mineralised nitrogen from the turnover of BIO and HUM. This ahs been added: "Respiration is carried out by microbes who require sufficient nitrogen to convert the RPM

and DPM pools into BIO and HUM pools. This nitrogen is available from the net mineralised nitrogen from the turnover of BIO and HUM pools."

In this same equation, I am also unclear what is Navail at this point in the manuscript. As with several other terms, the terms are introduced but their expressions are mentioned or the terms clarified much later which makes it very difficult to follow the logic. It is only further down in eqn. (51) that Navail is clarified.

A pointer to Navail which has been redefined as Ninorg for the bulk case has been added to help the document flow better. Fn has also been added to the vertically resolved case because it has a slightly different definition.

Also, in eqn. (37) what happens if DDP M or DRP M are negative? Is this possible, since minrealization can be more than immobilization?

They are always positive because the values for CN_soil are << CN_dpm/rpm. If they were negative, Fn should just be 1 because there would be more mineralisation than immobilisation from **all** pools. A sentence to address this has been added to the paper: "The demand is always positive because the C to N ratio of soil is very much less than the C to N ratio of the DPM and RPM pools"

53. Page 14. Similarly in eqn. (39) can Itot be more than Mtot making Ngas negative.

Fluxes will have been limited by Fn to make sure this isn't negative. If it hits the minimum pool size, it calculates a correction term (neg_n) and that correction term is then included as a negative gas flux. But that is applied just as an 'extra' gas flux and not applied to minl and immob. So Eq 39 is never negative, but gas flux can be, if that makes sense! This has been added: ". f$_N$ limits the nitrogen fluxes so that (M$_{tot}$ - I$_{tot}$) is always positive. However, if pool sizes become too small N$_{gas}$ could become negative to ensure nitrogen is conserved."

54. Page 15. Eqn.(41). Is fdpm used here different from fDPM used in eqn. (25).

Changed – they are the same.

55. Page 15. Lines 416-417."The litter inputs are distributed so that they decline exponentially with depth, with an e-folding depth of 0.2 m". With this parameterization can litter enter a soil layer even if there are no roots in that layer.

This is correct and added "This means that litter can enter a soil layer even if there are no roots in that layer":

56. Page 15. Line 423. Please consider using "bulk" or "single layer" instead of "non-layered".

Changed

57. Please consider using another term for gaseous losses rather than turnover.

Changed "additional inorganic gas loss term"

58. Page 16, Lines 433-434."Without this additional turnover available N may in- crease excessively, potentially due to excessive biological fixation in regions that are generally unlimited". What does "regions that are generally unlimited" means?

Changed "Without this additional gas loss term available N may increase excessively, potentially due to excessive biological fixation in regions where the NPP is very close or equal to the NPP_{pot}"

59. Page 16. Line 434-435. "In the current model configuration this parameter is set to 1.0 (360 day-1) such that the whole pool turns over once every model year". Do you mean 1.0 $year^{-1}$ which would translate to (1/360) day^{-1} and not 360 day^{-1}? Also, since the time step of the biogeochemistry is the same as for TRIF- FID (i.e. 10 days) there has to be ΔT somewhere. And, I suspect, 360 is used and not 365 since the calendar year in the UKESM model is 360 days. Correct?

Changed to “1/360 (day^{-1})”. Indeed 360 days represents a year in UKESM.

60. Page 16, line 436. "This results in an effective saturation limit of 0.002 KgN m^{-2}...". Not clear - saturation limit of what?

Turnover is limited by typical fluxes in and out of pool. In practise it never gets bigger. This line has been deleted.

61. What are the units of β in eqn. (47). Just above eqn. (47) β is said to be assigned "a value of 0.1 based on sorption buffer coefficient of Ammonia although here it represents the sorption of all inorganic nitrogen species". Note here that typically only NO_3^- leaches into the runoff and not NH_4^+ so please consider modifying this sentence.

Beta is dimensionless (added) and the sentence is changed to: ‘β is assumed to have a value of 0.1 and in JULES-CN represents the combined sorption of all inorganic nitrogen species \citep{wania2012carbon}.’

62. Page 16. Eqn. (48). Isn’t f1 simply the fraction of roots in each soil layer. And again, f2 is not defined or described here but further down in eqn. (53).

We have reformatted this section in an attempt to make things clearer. It now includes the following straight after the equation to act as a better signpost. Each of the modified components of Equation \ref{eq:ninorg} are discussed in detail below. The additional parameters required are $f_{R,i}(z)$ - the fraction of roots in each layer (Equation \ref{eq:norm_root}); $f_{I,i}(z)$ - the fraction of available inorganic nitrogen in each layer (Equation \ref{eq:frac_avail}) and N_{flux} - the transport of inorganic nitrogen from the layer by the soil water fluxes (Equation \ref{eq:layer_leach}).\\

63. Page 16. Line 453. "where froot(z) is the volumetric root fraction at a given depth". You mean "for a given soil layer" as opposed to "at a given depth". And, an i subscript seems to be missing here. Although, I wouldn’t suggest using i which has been used for PFTs, DPM or RPM, and now soil layers. Very confusing!

Definitely confusing! i is actually for pft here. We have clarified and change the soil carbon pools to a p! Also added the I where required on the froot term.

64. Page 17, eqn. (50). Is the parameter τ_{resp} tuned so that Nturnover is similar in the "bulk" and "layered" versions.

This has been added: Here τ_{resp} was tuned to give a realistic estimate of soil carbon in a vertically resolved version of JULES-C as in \cite{burke2016gmd}

65. Page 17. Eqn (51). Assuming, the subscript i represents the PFT shouldn’t there be (z) term here to indicate the nitrogen availability in each layer.

Added the z to this equation. Ive tried to clarify the i represents PFT.

66. Page 17. Eqn (52). I am unable to follow eqn. (51). Looking at eqn. (51) the term in parantheses in eqn. (52) should be zero since $f_{root,i} N_{in} = N_{avail,i}$ from eqn. (51).

Indeed this is correct – Equation 51 is for an equilibrium state whereas equation 52 is for a transient state.

The value/units of γ_{diff} is also confusing. I am not sure what $100\ [360\ day]^{-1}$ means.

Units of this parameter have been changed so it is per day instead of per 360 days.

67. Page 17, lines 471-474. "Any fixation goes directly into the available pool, and other fluxes are simply added according to the ratio of the available to total inor- ganic N pools at equilibrium (thus the available pool would always follow Equation 51 were it not for the fixation and uptake by plants)". I am sorry but I am unable to follow this sentence.

This has been rephrased to make it clearer – particularly focusing on the definition of equilibrium.

68. Page 17. Eqn. (54). In the absence of its units, I am not sure if the term dz_n is a single variable or do you mean Δz_n. And, I have no clue, what z_n is at this point in the manuscript.

We have rewritten this equation and text: Leaching is now done in a process-based manner, where the inorganic N is transported through the soil profile by the soil water fluxes. For any given soil layer n of thickness δz_n, the inorganic N flux (N$_{flux,n}$) of layer n is given by:

\begin{equation}\label{eq:layer_leach}

N_{flux,n} = \beta \delta z_{n} \frac{d}{dz} \left(W_{flux,n} \frac{N_{in,n}}{\theta_n} \right)

\end{equation}

where θ_n(z) is the soil water content of layer n in kg m^{-2} and $W_{flux,n}$ is the flow rate of the water through soil layer n in kg m^{-2} s^{-1}. Multiplying by δz_n gives the change in N content for each layer, n. The total leaching is then the sum of all nitrogen that leaves the soil by lateral runoff or out of the bottom soil layer.\\

69. Page 17. Line 483. "... is then the sum of all nitrogen that leaves the soil by lateral runoff ...". Does the lateral runoff from each layer mean that JULES is capable of producing runoff based on slope of the ground? Please clarify what exactly lateral runoff means.

We have removed the more specific details of how the water leaves the soil as I think it complicates further an already complicated paper. However, JULES has a version of TOPMODEL which can be switched on an generate lateral flows (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2004GL020919)

70. Page 18. Lines 501-502. "They were spun up by repeating the time period 1860- 1870 ...". This is confusing. Please consider rewording as "The models were spun up by using the meteorological data for the period 1860-1870 repeatedly..."

Changed

71. Page 19. Lines 522-524. "The main difference is the present-day NPP which is 12% higher in JULES-C than in JULES-CN. This is a direct consequence of nitrogen limitation which restricts the ability of the plants to utilise all of the carbon". No this is the direct consequence of JULES-CN reducing NPP. I don't think, it is necessary to spin this in a more biological way.

Changed

72. In Figure 4, it would be really useful to see separate estimates for mineralization and immobilization. In its current form, only net mineralization is reported.

This figure has been updated and now includes both immobilisation and mineralisation.

73. Page 20. Lines 580-582. "This [CUE] represents the capacity of the plants to allocate carbon from photosynthesis to the terrestrial biomass". I don't think this sentence is entirely correct. Since CUE is the fraction of GPP converted to NPP, it is a measure of autotrophic respiration.

Changed to: Plants with a higher CUE have a lower autotrophic respiration and allocate more carbon from photosynthesis to the terrestrial biomass and vice-versa.

74. Page 20, line 582-583. "In the model nitrogen limitation restricts the ability of plants to allocate carbon and reduces the carbon use efficiency". Here again, the "restriction of ability of plants to allocate carbon" appears as if carbon is there but some how plants can not allocate it. In contrast, as JULES-CN is designed, there is simply less carbon to be allocated. I don't think, JULES' allocation module has been changed in JULES-CN to limit how much C flows to different components.

Changed to: In JULES-CN there is less carbon available to be allocated because it is constrained by the amount of N present. This reduces the carbon use efficiency.

75. Page 21. Line 596. " ... by structural changes in the vegetation in particular ...". Please clarify if structural changes refer to changes in vegetation height, LAI, and rooting depth.

This is mainly the vegetation distribution – this been made clearer in the text.

76. Page 22. Lines 626-628. "There remains a significant underestimate of NEE in the years following the Pinatubo volcanic eruption ...". Please make it explicit in which year Pinatubo erupted since it's not marked in Figure 12.

Pinatubo erupted in 1991 – this has been added.

77. Page 22. Line 646. Please change "tome" to "time".

this has been changed.

78. Page 23. Line 656. "In this model, nitrogen limitation affects NPP and how the carbon is allocated ...". As mentioned above, I think, it's more appropriate to say how much C is allocated since the underlying C allocation module has not changed between JULES-C and JULES-CN.

this has been changed.

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