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Geoscientific Model Development Discussions

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

William Wieder (Referee)

wwieder@ucar.edu

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Wiltshire and co-authors nicely document their additions of a nitrogen cycle and vertically resolved soil biogeochemical model to the JULES model for use in UKESM1. The offline simulations include documentation of simulated vegetation and soil carbon and N pools and fluxes and their change over the historical period. A comparison with some observations is provided for model evaluation

Major concerns

My major concerns aren't that substantial, but stem from contradictions in what's expected from the paper and what's actually delivered.

The paper sets off comparing the C only, CN and CN_Layered implementation of the

model, but a number of display items omit results from the CN_Layered configuration. Specifically, Fig. 10-12 & Table 2 do not show results from the layered model, why? Should these effects of vertical soils also be discussed in 5.3? Because these results are not presented, I think major revisions are warranted.

Are there meaningful differences in plant distributions simulated with the new N enabled or CN_layered models?

The multi-layered canopy model is introduced in section 3, but never really discussed in section 5. Should it be? Are there any interesting insights enabled by this new feature of the model?

Minor and technical concerns: These are more numerous, but intended to clarify and improve the paper.

I like the high-level overview of the main findings summarized in the abstract, but I wondered if more quantitative results should also be provided (pending length requirements for the journal)?

Paragraph starting on line 70. I appreciate how clearly model assumptions are laid out. For example, the approach here looks at the "large-scale role of nitrogen limitation on carbon use efficiency", but I wonder if there's evidence to support this common assumption made in models in real ecosystems? What is the assumed impact of N limitation on NEP? The net results it that is dampens

Can paragraphs around lines 60 & 90 effectively be combined? Both paragraphs seem to have a common purpose of documenting the model connections and history. It's also not really clear how JULES fits into UKESM (also called UKESM1) vs. HadGEM2

Is section 2 subheading really warranted? Maybe just combine subheadings for 2 & 3 into one longer section?

There are some redundancies in the text (section 3) where sentences are repeated at different points.

Line 162. I'm confused why "These stoichiometric functions already exist in the model" for MR fluxes. This suggests the new work here is just to explicitly represent the N pools that were being implicitly assumed in the carbon only model? Separately, is it worth documenting the source for vegetation stoichiometry (presumably used in Cox et al. 2011)?

Fig 1: The assumption that 'roots' in the model have a lower (or equal) C:N than leaves seems surprising to me, buth this but seems contradicted by 'Ratio of root to top leaf nitrogen' (Table 1), please clarify. Roots have wide variation in C:N (Iversen et al. 2017), but if anything I'd assume they should have a higher C:N ratio than leaves (Kattge et al. 2011).

Table 1: "Top leaf nitrogen concentration": listed twice

Line 175, this statement doesn't seem to be true for grasses, which have declining C:N with height (Fig 2).

Section 3.1.1, oh no, why define N fixation (which should limit NPP) as a function of NPP in the model?! This isn't the first modeling group to make this assumption, but a brief discussion and literature review seems warranted (see Vitousek et al. 2013; Thomas et al. 2105; Wieder et al. 2015; Meyerholt et al 2016)

Section 3.1.1- I think inputs from N fix lead off these details of the CN model because that's where the N cycle 'starts', which seems logical, but putting it under a "Vegetation carbon and nitrogen" (subheading 3.1) seems odd, especially since Nfix contributes to the soil N pool (not plants). Maybe different names for the higher level subheadings (3.1 and 3.2) would be warranted? Alternatively, use Fig 1 to group these fluxes together.

Line 182 What is potential NPP? (eq. 9). How does this different than the 'actual' NPP? If not discussed here, please reference where this is described (3.1.4).

Line 225 where is the multi-layer canopy approach included in these simulations? I'm assuming with with CNlayered, but this isn't clear in section 4 (line 495)

What is 'spreading' in the model (section 3.1.4)? Is this prescribed by some land use time series dataset or prognostic (more like a DGVM)? Text on page 11 makes me think it's the later.

The assumptions made in the phenology and allocation section are thoroughly defined, but it's hard to understand for readers not familiar with TRIFFID how N limitation is implemented in the model. It seems like it's an instantaneous down regulation of NPP, with extra carbon respired by plants that are N limited? With that N limitation calculated by the tissue and pft specific stoichiometry defined in the model?

Eq. 25-28. I don't really understand how the soil model is wired based on these equations. If R_DPM and R_RPM are the respiration terms from litter pools, how do some of these fluxes go back into the BIO and HUM pools, which themselves are respired (and also simultaneously included as inputs to BIO and HUM)? It seems that soil respiration fluxes to the atmosphere are actually R_tot*B_R, if so, the R_* fluxes should be some kind of soil turnover term (not respiration).

It seems like B_R is a critical number here, as it controls the soil carbon use efficiency and the amount of N required during litter decomposition (eq. 35). Is this parameter value defined somewhere?

Eq. 29-32 do the N fluxes need to include I_DPM + I_RPM?

Line 355, as above can this be called potential turnover, not "potential respiration"?

Eq. 33. I'm trying to wrap my head around the vegetation controls over decay rates and how that may feedback to a CN model that has vegetation with very different stoichiometry and N demand (woody vs. grass pfts; Fig 2) but that allows for plant competition (on a single soil column). I assumed the maps of nutrient limitation (Fig. 6) reflect differences in vegetation N demand (per unit of C), but are decay rates also slower for grasses (increasing the N limitation in these ecosystems)?

Eq. 36, Is this still a potential decomposition rate, as it's 'limited' by N availability?

Line 385, what are ‘these two pools’? I think it should be DMP and RMP, but it’s not clear in the text?

What happens to wood in the soil CN model? How is it allocated to the pools described?

Eq. 39, is there anything that prevents this flux from being negative? Are there times when immobilization > mineralization?

Eq. 39, Should the N loss description go into 3.3 (inorganic N) instead of the soils section (3.2)?

Where does N_turnover flux (eq. 46) go in the model, the atmosphere? How large is this tuning flux relative to other loss terms?

Eq. 46, where does N_gas (eq. 39) fit into the N budget summarized here?

Section 4, How does the model handle agricultural fractions of grid cells?

Section 5.1 I’m used to fluxes and pools being roughly proportional in models like this. If NPP is 11% lower in the CN model, why are the vegetation stocks roughly equal in the C and CN model? Similarly, if the vertically resolved model has a similar NPP to the CN model why are vegetation C pools so different?

Fig 4, 8 and others, Since the text is organized with C, CN, and CN_layered should the display items be similarly organized?

Fig 4 what is the ‘N-loss’ flux supposed to represent? As drawn, I think this is a gaseous N loss, but as labeled it’s not clear how this connects with N_gas and N_turnover fluxes (see above).

Fig 4, how deep are the soils being represented, this is especially important to consider in the vertically resolved model and should likely be described in methods (3.2.1)

Line 535, doesn’t this just mean the model is at equilibrium as it should be given your

spinup procedure?

Section 5.2 seems out of place, as the extent of N limitation should be preceded by a more thorough comparision of the model states and fluxes. One suggested organization could be comparing the 1) Spatial distribution of present day stocks / fluxes and residence times (e.g. Figs 4, 10, 7, 9, & 8 in that order) and 2) Temporal evolution of relevant stocks / fluxes over the historical period (e.g. Fig 11 & 12) 3) N limitation (Fig 6, 5) as diagnosed by NPP_Potential / NPP and its evolution over time (Fig 11b).

The title for Fig 5 (and associated text) implies that you conducted a N fertilization experiment (see Wieder et al. 2019), but I don’t think this is accurate. Instead you’re calculating a N limitation diagnostic (NPP_pot/NPP) and comparing that to results from an observational synthesis.

I’d suggest flipping the order of Figs 5 & 6, as they both show the same information, but Fig 6 is less processed model output, with 5 serving to summarize biome-specific information and related it to observations.

Fig 6, Line 553. It seems like the model is more strongly limited in grasslands, which have much higher N requirements / unit of C (Fig 2). This doesn’t really show up in results for ‘tundra’ or ‘grasslands’ (only for Savannah). I wonder why?

Should multi-paneled figures be labeled (‘a’, ‘b’, ‘c’) and accordingly described in the figure caption?

Fig 7, can legends be smaller or moved into the figures (as in Fig. 9) so the data are easier to read?

Fig 9, should the bottom panel be labeled soil C residence time and also include data from the C-only model?

Fig. 11b what is the time series of ‘response ratio anomaly’? Is this the change in NPP_Pot / NPP_act that used to diagnose N limitation shown in Figs 5 & 6? If so, is this what you’re calling ‘progressive N limitation’ (line 599), in which case this should be clarified on and expanded in the text.

Fig 12 & section 5.2.3 The low bias in NEE (~0.5 Pg / y, roughly 25%). This would lead to an underestimation of the land carbon sink of about 25 Pg over the period from 1960-2010 (or about 12 ppm CO_2 in the atmosphere). Thus, while the IAV of NEE looks better here, that overall magnitude of the land sink may be too low with the CN version of the model. This isn't a deal breaker for the paper but time implications of the low bias with the CN (and CN_layer) model should be discussed in the text, especially since JULES_CN is included in UKESM1.

Section 5.3. Is it just frozen soils that are causing this? it seems the differences in Veg C pools extend down to ~ 40 degrees north (Fig 7). Is this somehow connected to assumptions about the fraction of N that plants have access to in the vertically resolved model (e.q. 51)?

Line 611, as noted in Fig 12, the low biases in land C uptake seems notable if you're trying to capture changes in the atmospheric CO_2 growth rate.

Line 671 what is "climate-induced mineralization" I'm assuming this has something to do with accelerated decomposition from climate change increasing N mineralization rates?

Is there a data availability statement required for the journal? References: Iversen, C. M., McCormack, M. L., Powell, A. S., Blackwood, C. B., Freschet, G. T., Kattge, J., . . . Violle, C. (2017). A global Fine-Root Ecology Database to address below-ground challenges in plant ecology. New Phytologist, 215(1), 15-26. doi:10.1111/nph.14486

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