

Review of revision: “Impact of changes in climate and CO₂ on the carbon-sequestration potential of vegetation under limited water availability using SEIB-DGVM version 3.02”

General comments

General comments from my previous review:

In this manuscript, the authors perform simulations with the dynamic global vegetation model SEIB-DGVM to explore the impact of historical changes in climate and atmospheric CO₂ concentration on potential carbon sequestration in live vegetation. Intriguingly, they look not just at total biomass, but also “aboveground” vs. “belowground” biomass (although those terms are misleading; see below). This allows the authors to examine how plants have shifted their growth strategies over the last century to maintain a competitive edge under environmental change.

The results show that both biomass pools have increased, but with “belowground” increasing more than “aboveground” on a relative basis. Factorial experiments reveal that atmospheric CO₂ increase is unsurprisingly the dominant driver of potential biomass increase in most of the world, but temperature and other factors are more important at latitudes above 60°N. The results also show that “aboveground” and “belowground” responses to environmental change differ along an aridity gradient, as well as from each other.

The authors designed a suite of experiments well-suited to explore how plant individuals and communities have changed their growth strategies to deal with environmental change. However, the manuscript needs substantial rework. Most importantly, while the Introduction briefly mentions previous findings regarding shifts in above- and belowground allocation under environmental change, this should build up to a set of hypotheses that are then tested with the model experiments. It is also unclear why this was submitted to *Geoscientific Model Development*. Perhaps if it were more focused on comparing SEIB-DGVM biomass to observations it would fit as an evaluation paper, but the work performed is much more high-level than that. I thus think it would be more appropriate to move to *Biogeosciences*.

The authors decided not to move journals, which is fine. They have made significant improvements in terms of explaining how water stress theoretically affects allocation, as well as tying their results back to this theory. However, I still have some significant questions about the methodology and confusion about the interpretation of results. As such, I again suggest this manuscript be ***reconsidered after major revisions***.

Specific comments

Methods: Hydrological regions

The aridity zones were determined based on a 115-year average, but it’s possible that they could see long-term trends. For example, a gridcell classified as “semi-arid” on average might have been arid at the beginning of the simulation and sub-humid at the end. This is potentially a very important confounding factor, and might explain the sometimes-large variation around $\Delta=0$ and the resulting weak trends in Figs. 9–10. If a lot of gridcells see shifts like this, it might be necessary to restrict analysis to grid cells that didn’t change in terms of aridity, or didn’t change much.

Another option might be to let gridcells shift their classification over time. This would require the classification to be based on a rolling mean of the previous, say, 15 or 30 years of climate. So, e.g., a bin in Figs. 9–10 would be “[mean 2015 L/WVBC value for all cells that were in this bin in 2015; i.e., whose 2001–2014 climate fit into this bin] minus [mean 1916 L/WVBC value for all cells that were in this bin in 1916; i.e., whose 1901–1914 climate fit into this bin].” Fig. 11 would show, at each year Y in each subfigure F, “mean Y L/W ratio for all cells that were in class F in Y; i.e., that qualify as class F based on climate in years Y–15 to Y–1.” This would be necessary in order to avoid spurious interannual switches in classification, as well as to minimize the effects of a lag in vegetation community response to changing conditions.

Methods: Pasture exclusion

I appreciate the authors having made this change to their analyses, as I suggested. However, I’m a bit unclear as to how they actually did it. L338-42 are confusing, but I interpret them as saying: “To minimize confounding effects of livestock grazing, we excluded grassland grid cells with pasture fraction greater than 1%.”

If my interpretation is correct, it’s hypothetically possible for a “forest” gridcell that’s 51% forest and 49% grassland, with LUH2 saying “49% of the area in this gridcell is pasture,” and it wouldn’t be excluded. This doesn’t seem right. Instead, the authors should exclude *any* gridcell with a significant pasture fraction, whether it’s classified as “grassland” or not. 1% might be too strict, though—maybe something like 10% instead.

Also, make it clear that this exclusion only applies for the NPP comparison.

Unimodal pattern in Fig. 10

In my initial review, I raised the question of why $\Delta WVBC$ decreases along the aridity axis from semi-arid to humid regions. The authors have not actually addressed this, despite an attempt at L556-9. “Drought mitigation promotes the growth of WVBC”—okay, sure, that explains why it initially increases from left to right. “humid region with high light competition limits root growth”—but this figure is about WVBC, not LVBC. Why would high light competition in humid regions lead to decreased $\Delta WVBC$ relative to semi-arid regions? (My thinking is it’s because vegetation in these regions is light-limited: They’re not seeing any alleviation of that limitation with climate change.)

Other issues with interpretation: Results

Some of these suggestions might seem like they’re better suited for a Discussion section than a Results section, but I think it would not be good writing to simply list a set of observations with no context, then explain them somewhere later in the paper. The current Discussion section is well structured in the sense that it provides a general overview of the results and then compares to previous literature.

- Figs. 9-10, A6-7

- L461-2: It's not that plants stop growing; they're still alive! It's that they don't end up increasing their carbon stocks—i.e., it's so dry that they can't take advantage of higher CO₂.
- L462-3: What does it *mean* that there's no obvious difference among the slopes?
- L472-483: What do all these factorial results *mean*? It is not sufficient to just saying how big the difference is between the minimum and maximum experiment. What do they imply regarding the drivers of partitioning according to your hypotheses? You approach this for LVBC at L475-7, but you don't actually connect the results back to the hypotheses. (And you don't do this at all for WVBC.) Guide the reader!
 - L472-4: I'm not sure where these numbers come from.
 - L473: "changed" should be "ranged", I think. You're not comparing a *change*, you're comparing across a *range* of aridity classes. Unless I'm misunderstanding—as I said, I don't know where the numbers came from.
- Fig. 11 (L484-97)
 - Again, guide the reader. Here you've done a good job of phrasing the results in a way that connects back to hypotheses ("Under the synergistic effect of drivers and water stress, ... there is a larger proportion of biomass allocated to, and stored in, light-gathering vegetation organs."), but it's unclear how that is evident from the figure. Is it because the blue line is so much higher than the other lines? But why only in sub-humid and humid zones? And there, what does it mean that the ratio goes back down when additional factors are added after S3?
 - L488: What does "variation range of ratio between LVBC and WVBC" mean? Interannual variation? Variation among factorial experiments? Neither seems to match the trend mentioned.

Other issues with interpretation: Discussion

- L503-29: LVBC and WVBC trends overall
 - This is very confusing and disjointed. L514-7 and L527-9 make it seem like plants are tending to shift their allocation from WVBC to LVBC, but then L519-20 ("LVBC... dominates the long-term trends") and L525-7 seem to suggest the opposite. What's correct? Here's a hint—focus on the *ratio*. The absolute numbers I think are not very informative about allocation changes, because wood biomass is always so much higher than fine root biomass.
 - What does "Compared with WVBC" mean here? Does it mean "116.18 ±2.34 Pg C" is Δ LVBC minus Δ WVBC?
 - L520-2: Yes, but then why is increasing Δ WVBC concentrated in high latitudes? The difference here might be something that partitioning theory could explain, or maybe not—maybe it's just a relaxation of climate limitations in the high latitudes that low latitudes never experienced (as possibly suggested at L545-7).
- L531-47: Factorial experiments
 - This needs to be cleaned up a lot. For one thing, just listing these numbers feels much more like something for the Results rather than the Discussion. Here, you should be focusing on the implications of your results for scientific understanding,

and comparing your results to previous literature. For another, the presentation of results is really confusing. For example, radiation doesn't "dominate" the trend at any latitude band. It *explains 20.67% of the global variation*, though. Explain where these numbers are coming from (Fig. 8b/d, adding the (-) and (+) numbers at the bottom of each). But then maybe I have it wrong—it doesn't make any sense for any of these "fraction of variation explained" numbers to be negative, as they are for, e.g., precip → WVBC (-2.76%). Do you mean instead that the net influence of precip on WVBC is negative?

- L531-2: Is this conclusion drawn from Fig. 8? If so, I would rephrase to talk about the amount of variation explained, rather than the amount of increase. Always mention what figure(s) your assertions come from.
- L535-540: Still need to do a better job of tying these results back to the hypotheses and/or to other explanations. The sentence at L539-40 might be directly relevant to optimal partitioning theory, depending on what it ends up saying after the authors clean up the section.
- L578-90: Caveats
 - L587-90: This sentence could be interpreted as "We didn't vary N deposition over the experiment," when in reality no N deposition was included *at all*. This means that the simulated ecosystems would have an incorrectly *low* amount of N input, leading to incorrectly *high* amounts of N limitation, leading to an *underestimate* of CO₂ fertilization (because if they're N-limited, they can't take advantage of higher CO₂ levels). This is the opposite of what the authors seem to conclude.

Technical corrections and minor suggestions

- To avoid confusion (such as I exhibited in my first review), aridity index axes on all figures with them should include (something like) "drier" at 0 and (something like) "wetter" at 1. Or at least an explanation of this should be in the caption. The axes should also include labels indicating what the ranges are for the different classifications (humid, semi-arid, etc.).

Sect. 1: Introduction

- L50: "are" should be "is"
- L56-8: Citation?
- L76: "region" should be "regions"
- L82-83: The two parts of this sentence seem to contradict each other.
- L84: "predictable" should be "predicted"
- L104-6: It's not really a *proxy*; this is just how you define it. (Which is fine, of course!)

Sect. 2.1: Forcing data (minimal changes; no comments)

Sect. 2.2: "Overview of modeling concept in SEIB-DGVM"

- Good clarification of model timesteps
- L175-6: "doesn't include" should be "isn't included"

Sect. 2.3.1: Allocation in SEIB-DGVM

- L207: Define “soffit” (or, ideally, use a simpler word, like “layer”).
- L220-1: Not a complete sentence. Suggest changing the period to a comma and deleting “This”.
- L221: “NSC” not previously defined (should happen at L187).
- L221-2: Clarify that these organs are not explicitly simulated (unless they are!), and instead are represented as a flux to litter.

Sect. 2.3.2: Description of partitions

- L254: Delete “in PFTs”, maybe? What does it mean?
- L257-8: GVBC should be LVBC.
- L259-63: “wood” should be “woody vegetation”.
- L257-263: W_{mass} should be renamed, e.g. to T_{mass} (T for “tree” [or “trees and shrubs”] instead of W for “woody vegetation”), to avoid confusion with other use of W for “water-gathering”.

Sect. 2.4.1: Run setup (minimal changes; no comments)

Sect. 2.4.2: Factorial simulation scheme

- L274-7: The “remarkable effects” comment about wind and relative humidity is a result; don’t include it in Methods. Deleting that comment will allow wind and RH to replace “other factors” in the previous sentence.

Sect. 2.4.3: Non-parametric test methods

- L294: “A2, 3” should be “A2–3” or “A2 and A3”
- Figs. A2–3: Caption should include experiment labels S2 etc.

Sect. 2.4.4: Hydrological regions (no technical corrections, but see Specific Comments above)

Sect. 2.5: Observational data

- L328-9: “It was defined as vegetation grid that the land cover type of this grid is” should be “We defined vegetated grid cells as those whose largest component was”. This grammatical correction turns out to also be a simplification, as it then allows the deletion of the sentences at L332-4.
- L331-2: Replace this with a simpler sentence along the lines of “Other grid cells were excluded from our analysis.”
- L334: Start a new paragraph here, as you’re talking about something new.
- Fig. A4: There should be a clear break in the color bar at whatever threshold you end up using (currently 1%). Also, red-green axes should be avoided, because red-green colorblindness is relatively common.
- Fig. A5:
 - “NNG (no natural vegetation)” should more accurately be something like “NI (not included).”
 - “END” should be “ENF”

Sect. 3.1:

- Fig. 4: Pixels that were excluded based on land cover should be colored gray, to distinguish from included pixels with low correlation.
- Fig. 5: This is much better than the previous bar graph version. My only suggestion is to delete “Dynamic of” from the Y-axis label.

Sect. 3.2

- Fig. 6:
 - (a): Delete “Dynamic of” from right Y-axis label
- Could also refer to Fig. 7 for extra support in this section.

Sect. 3.3:

- L385: “while **they** declined”
- L387: “decrease” should be “decreasing”

Sect. 3.5

- Figs. 9-10, A6-7
 - Figs. 9-10: Are Y-axis units *per year*? It would be easier to relate to other figures if they were total over the simulation.
 - L460: “enhance” should be “increase”.
 - L460-1: That’s not really from the factorial simulations; it’s obvious from the historical simulation.
 - L461: “water pressure” should be “aridity”.
 - L462: “different” should be “difference”
 - L476: Should be “matches”, not “match”
- Fig. 11 (L484-97)
 - L490: “Positive influence” is unclear, and “allocate” should be “allocation”

Discussion

- L503-29: LVBC and WVBC trends overall
 - L520: For clarity, say “annual **change in** LVBC”
- L549-76: Aridity zones
 - Great improvements here with regard to comparison to other literature and tying back to theory.
 - L552: “region” should be “regions”
 - L550-2: Refer to figures supporting this (presumably Figs. 9 and 10).
 - L565: Start a new paragraph here, since you’re switching from analyzing your results to comparing them with previous literature.
 - L567-8: Description of Madani et al. (2020) is too vague. “Variable”? In what way, and how does it compare to your results?
- L578-90: Caveats
 - L584: I would say “**apparent** underestimate,” as the numbers from SEIB-DGVM aren’t *wrong*—they’re measuring something different.
- L592-601: Conclusion
 - L594: “to” should be “vs.” or “and”