

The manuscript "How Can Grazing Increase Grassland Carbon Storage: A Mechanistic Modelling Approach" makes interesting points about modeling grazing effects on carbon stocks in a parsimonious way, so that mechanistic conclusions can be easily derived. It is a nice, well-written paper that I enjoyed reading. I have some comments, questions, and suggestions below that I hope are useful to strengthen the paper towards final publication.

Major comments:

1. Probably my main comment is related to some of the simplifications made in the model. I understand the authors want to keep the model parsimonious, but simplifications like water limitation, or the fact that the model does not represent root:shoot partitioning or plant growth, may introduce strong feedback loops that undermine or constrain the current interpretation of the model simulations in mechanistic terms. For example, the conclusion that “the bell-shaped response is the exception rather than the rule” might hold only under the current model's assumptions, but I can imagine that some of the processes named above might also influence the bell-shaped response reported in other studies. Since I believe the authors may not want to build a different model, it would be helpful to see at least some reflection on, or acknowledgment of, the limitations of what the model can actually answer. This matters because the authors are trying to answer mechanistic questions, and some of the results read more like sanity checks of the model's internal consistency than mechanistic findings. For example, the authors can indeed claim that grazing intensity drives SOM increase via root exudation, but they cannot explain why and when this is happening, as root exudation in the model is just a linear function of h (grazing intensity) and not related to real mechanisms such as allocation shifts towards roots or compensatory growth, etc., so I wonder: can we really talk about mechanisms, or shall the authors temper the text a bit and limit it to what the model can actually do?
2. This comment goes hand in hand with the previous one, as it might have consequences for some feedback loops on other processes. I found the asymmetric treatment of the FOM and SOM decomposers interesting. For the first ones, the authors nicely represent them explicitly and show the consequences in the system when they are, for example, N-limited. I wonder why this is not the case for the SOM decomposers, especially if the base model SYMPHONY the authors used explicitly represents SOM decomposers. I believe this is an important point worth reflecting on and giving proper justification, as the fact that decomposers are just a fixed fraction of the SOM might mean that these organisms might not respond to priming effects in the way the authors are interpreting, as there is no saturation and

response towards priming might be overestimated. Also, where does the fixed fraction parameter (δ) come from?

3. The manuscript does not show much of a calibration exercise, which is a choice, but rather presents equilibrium scenarios and chooses equilibrium 4 to develop further. I just wonder how this equilibrium or stability, as mentioned in the text, was validated, as I do not see any Jacobian/eigenvalue analysis. The authors might want to add these justifications or rephrase the stability jargon.
4. I am a bit worried that the authors took the SYMPHONY parameters that were calibrated in what I understand to be a system including grazing treatments. That means that although SYMPHONY does not include the role of herbivores, the calibrated parameters implicitly include the grazing effect, meaning the SYMPHONY parameters later used in the model presented in this study might double-count grazing effects or at least introduce confounding. The authors should clarify whether this is the case. A complementary sensitivity analysis may be needed to clarify this point.
5. There is a claim in the discussion about persistent soil carbon: “As a consequence, SOM formation by FOM decomposers and root exudation both increase, ultimately promoting the accumulation of persistent soil carbon (Fig. 7)”. I feel the authors cannot really discuss anything related to persistent forms of soil carbon, as they do not really describe either of the two carbon pools they have as persistent. If they meant SOM to be the persistent form, that would be an error, as they packed inside SOM root exudates that are, by their own definition, low-molecular-weight compounds that are easily degradable, i.e., labile. So the gains they observed in their simulations might just be transient labile forms.

Minor comments:

1. Figure S2 is cited in the text twice, but it does not appear in the supplement.
2. The η parameter appears in the main text, but it is called v in Table 1 and in Eq. 12.
3. The double appearance of h in Eq. 8 (defoliation loss vs. exudation cost) is mathematically correct but could confuse readers. It is worth adding a clarifying sentence about what each term corresponds to.
4. Likely typo: $D_n = D_c/p_P$ (line 168) should be D_c/p_D .
5. Likely typo: " $FOM_n/FOM_c > r_D$ " (line 200) should reference p_D , not r_D .
6. Likely order-of-magnitude typo in section 3.3: $h = 0.0001125 \text{ day}^{-1}$ for "high" grazing intensity (1.5 LU) should probably be 0.001125.
7. Table 1 lists "m" for plant mortality; main text uses m_P throughout.
8. Supplement text refers to "(Table 3)" where it should say "(Table S3)".

9. It is unverified whether all plotted (h , be) combinations in Figures 3-6 fall within Eq. 4's valid existence domain.
10. The distinction between NPP as a flux and Pc^* as a standing stock, explained in section 4.2, would be easier to follow if flagged earlier, e.g. where NPP results are first reported in section 3, since an increase in NPP can occur even while plant biomass itself declines.
11. Please double-check the Chertov et al. (2022) citation at line 125. Its title suggests this model explicitly incorporates the rhizosphere priming effect, which appears to contradict its use here as an example of a model lacking this mechanism.