Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC1
https://doi.org/10.5194/bg-2022-164-RC1, 2022

Comment on bg-2022-164

Anonymous Referee #1

Referee comment on "Assessing carbon storage capacity and saturation across six central US grasslands using data-model integration" by Kevin R. Wilcox et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-164-RC1, 2022

This is an interesting paper, examining the potential carbon storage capacity of grasslands. Overall, it is well-written, and provides interesting and important results. That said, there are many limitations.

First, the number of abbreviations, acronyms, and symbols make it almost unreadable. I understand that the authors are working to present efficiently, but it's too much. The paper would benefit from a major re-writing, using real words instead of writing sentences and even research questions and interpretations about CSAT, Xp, XC, TE, etc. Most of us know NPP, MAT, and MAT, but still, these others in long complicated sentences make the paper impossible to really understand without having the glossary at hand.

The list of "questions and associated hypotheses" is sloppy. The first one has been tested many times over (and they are very short on citations about this); the second is not even close to a hypothesis (they should either use questions or hypotheses instead of garbling them up together); the third doesn't have a causal explanation, necessary for a hypothesis (a prediction with a causal explanation). As I understand it Csat is the proportion of current carbon storage relative to the potential carbon storage, and the explanation doesn't relate to this. That third one is an example of an idea that a) doesn't make sense, and b) is impossible to interpret given the density of acronyms/symbols.

It's quite difficult to tell from the Methods which of the values were modeled and which were using the measured data from the sites. The explanation of "formal model validation" states that they validated the vegetation components. Notably, vegetation does not comprise the major pools of C in these systems. There are some problems (line 195) with this method of estimating carbon storage with depth, as this varies so much by soil profile and location - it's not terrible, but a caveat should express the limitaitons of the approach.

I don't understand the "normalization" nor which slopes the authors are referring to (line 210).

For a number of these systems, a large proportion of the carbon stored is in recalcitrant soil pools. IThere needs to be MUCH more citation and analysis - and probably reconsideration of these residence times. Previous authors have shown that a good portion of the ecosystem carbon in these grasslands turns over on thousand year time scales, not time scales of 20 or so to 50 years. This alone gives me a great deal of concern about the paper. The paper should at least note that their estimates are orders of magnitude less than others have published.

The paper misses alot of literature about carbon storage, NPP, and decomposition across the region - it is almost shocking. There are very solid papers on the trends in soil carbon storage of grasslands vs. croplands (disturbed systems) across the central grasslands region, and on the trends in NPP and decomposition (k values) tested against mean annual precipitation and mean annual temperature that are never cited, in addition to other papers addressing mechanisms of C storage across the grasslands gradients in the region, in large scale databases and in a very original and key modeling paper for the region - this latter paper seems like a seriously important progenitor and the gap in citing it is pretty egregious. The paper does not really address the key issues about the effects of most disturbances (ie the distance between Xp and Xc, ack) on soil carbon storage, or what really happens that reduces C storage. It's quite theoretical, which is ok, but ungrounded in other literature on carbon storage either from empirical work or from modeling work in these grasslands. . There's no citation of where the effects of temperature on decomposition came from, from the empirical literature, used in the modeling (fig. A4 showing the modeled relationship between temperature and decomposition) - it's as though the authors made up these relationships and values out of their heads, instead of from empirical data or others' work.

The conclusions are broad, and don't really present new insights. One reason that the hot and dry grasslands may have more C than they think is "their capacity" (the gap between the Xp and Xc) ould be that the model is not representing the systems well - that should be clearly state - it may not actually be a reflection of system dynamics. Finally, the conclusion spends a lot of time on burning, a relatively rare disturbance in some of the systems studied, and consideration of other disturbances should be included.