

Biogeosciences Discuss., author comment AC1
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Reply on RC1

Kevin R. Wilcox et al.

Author 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-AC1>, 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.

Thanks. And we feel confident in our ability to address the limitations of the manuscript outlined below.

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.

In the manuscript, we tried to balance efficiency with readability. However, based on the comment here and that from the other reviewer, it seems like we leaned too far to the efficiency end. If invited to submit a revision, we will address this issue.

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.

This is another instance where brevity was our goal. Instead of having separate sections for questions and hypotheses, we decided to combine them. This sometimes resulted in questions without associated hypotheses (e.g., Q2) when we felt the current understanding was not sufficient to posit meaningful hypotheses. We agree that Q1 about relationships of NPP, $\square\square$ and $\square\square$ with MAP and MAT has been the focused on in previous studies. Yet, we argue that there is value to having both questions that test current ideas along with questions that test entirely new ideas, thus grounding novel work with more established patterns. This being said, we agree that we could execute the questions, hypotheses, and predictions better and will do so if invited to provide a revised

manuscript.

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.

We apologize that the Methods were unclear about the sources of the values used in the paper. Wherever possible, we used empirical data to inform our analyses. However, much of the time, empirical data did not capture the fullness of information necessary for calculating holistic variables, such as carbon potential. In a revised version, we will be more explicit about where each piece of information came from. For reference, we summarize this here. GPP and NPP were obtained from the model (L118-120), which was forced using environmental data from the sites (L123-126) and vegetation components were calibrated using observations of ANPP and BNPP, as well as abiotic characteristics such as soil texture and soil moisture dynamics (L128-131 and Appendix D).

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.

We agree. This, along with difficulties present in measuring C cycling parameters in the field (e.g. slow C turnover), is why we used the data assimilation approach to estimate the processes that drive soil C pools. We used high temporal resolution to estimate 15 parameters associated with the C submodel (Fig. A1-4, Table B1), which we feel is in itself a valuable contribution to the field. We argue that this approach is a much more robust method of estimating C cycling pools and processes than simple model calibration and validation, since there could be many ways to achieve the observed carbon flux values (e.g. fast inherent turnover times, high temperature sensitivity).

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 limitations of the approach.

The reviewer makes a good point about difficulties estimating carbon storage by depth. However, we do think it is important to estimate soil C from 0-20 cm to ensure that simulated and observed C pools match in the layers of soil they are estimated in. We will include caveats to these estimates in the text if we are invited to resubmit.

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

We mean that the values were scaled by the standard deviation and centered around the mean, which we will explain better in the text.

For a number of these systems, a large proportion of the carbon stored is in recalcitrant soil pools. There 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.

We appreciate this comment and will incorporate more citations on this subject (e.g. Conant et al 2011 GCB). We would like to note, however, that these carbon residence time estimates incorporate all carbon substrates including both those that turn over

quickly and those that turn over much more slowly. If we only focus on the recalcitrant substrates from our study here, we see inherent turnover rates more in line (167-194 years) with the comment here by the reviewer, albeit not quite thousands of years. The discrepancy here may be due to our focus on relatively shallow soil layers. We propose to include two additional exercises that we think will provide a solution to the issue brought up by the reviewer. First, we will run a simulation where we manually set the inherent turnover time of the passive pool to some of the slower rates found in the literature and report what effects this has on carbon residence times. Not only will this provide insights to how much control these passive turnover pools have on total residence time, but it may also be incentive to better represent these processes into models. Second, we will expand the current section discussing some of these issues (L402-409), which will qualify our findings to the less recalcitrant soil substrates.

The paper misses a lot 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.

Originally, we had not included text (or associated references) for comparisons of croplands versus non-tilled grasslands since soil processes differ substantially in tilled versus untilled soil and by crop type. However, we recognize the value in this since much of the historical extent of grasslands is now cropland. If invited to revise, we will include discussion and literature on this topic such as Chambers et al 2016 J of soil and water cons., Smith et al. 2005 GCB, 2004 Eur J of Ag.

We do include articles associated with the mechanisms and gradients of decomposition rates (Brandt et al 2010, Garcia-Palacios et al 2016), and NPP (Sala et al. 2012, Huxman et al. ,2004). However, we will expand these citations to include additional studies such as Zhou et al 2009 Ecosystems, Bontti et al 2009 GCB, Yahdjian et al 2006, Burke et al 1997 Ecology, Parton et al 1993 GCB, and others.

With regards to addressing previous literature stemming from modeling results and large scale databases, we assume that the reviewer is referring to the LIDET data set and associated papers (e.g., Bonan et al 2012 GCB, Adair et al 2008 GCB, Bontti et al 2009 GCB, Harmon et al 2009 GCB, Parton et al 2007 Science). This was an oversight on our part - we appreciate the reviewer pointing this out and we will include these citations in a revised version.

The paper does not really address the key issues about the effects of most disturbances (ie the distance between X_p and X_c , 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.

In the simulation of soil carbon capacity, the effect of disturbance is primarily through the loss of NPP inputs due to losses during disturbance, which we could expand on in the text (currently L394-400). A couple of references we could add would be Ojima et al Biogeo 1994, Lorenz and Lal C Seq in Ag Eco 2018).

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 temperature effect on decomposition (Q10) is shown as a density distribution obtained from the data assimilation process we conducted in our study. The bounds for the parameter were obtained from density distributions presented in Shi et al. (2015 Ecosphere), but the distributions shown in Fig A4 are obtained from the Markov chain Monte Carlo simulations comparing model output with observations.

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 X_p and X_c) could 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.

We respectfully disagree with the reviewer on this point. We think that this study presents major conclusions that are important for addressing critical issues in the future. Although the model-data fusion approach makes assumptions that may not perfectly mirror the carbon cycle in all ecosystems, it allows for estimating ecosystem attributes that are difficult or impossible to estimate without very long and detailed observational records of carbon pools and fluxes – namely, it allows us to estimate future tendencies of ecosystem to gain or lose carbon. First, the finding here that fast carbon turnover rates in drylands without increased NPP inputs may cause future C loss is important for guiding land management and future research priorities. These findings are corroborated by recent empirical findings in drylands showing increased C losses in arid grasslands linked with wetter and productive years likely through increased microbial activity (Hou et al 2021 Biogeosciences). The effect of fire for carbon potential is another important finding from our study. In many ecosystems fire enhances NPP and is used as a common management tool. There is uncertainty surrounding the balance between increases in NPP and decreases in conversion of ANPP to soil C – our study provides evidence that the losses of ANPP may eventually outweigh the increases in total NPP leading to C loss. All of this being said, we would include additional discussion about uncertainties associated with these findings in a revised version.