Comments by the reviewer are *italicized*; our responses are in indented, normal font.

Manser et al.

Stable isotope evidence for long-term stability of large-scale hydroclimate in the Neogene North American Great Plains

EGUSphere

Comments of David Fox

Summary

The paper presents new d18O data from Miocene pedogenic carbonates from the Ogallala Group/Formation in the Great Plains region of the USA and, in conjunction with compiled published data, analyses the spatial pattern in relation to patterns predicted from climate data and moisture transport model. The paper is well-conceived and clearly written and is mostly thorough in its treatment of the data, the geological context, and the modern and Miocene climate system and implications of the data for our understanding of Miocene hydroclimate in central North America. I have a few mostly minor comments below and think this paper is ready suitable for publication after minor corrections and additions.

> We thank Dr. Fox for the thorough review and thoughtful comments. Addressing these concerns has certainly made the paper better.

General comments.

1. The lithostratigraphic terminology for the Ogallala is a bit complicated across the region. In some states (e.g., Nebraska) it is a Group with constituent formations and in others (e.g., Kansas) it is an undifferentiated Formation. I think Tedford et al., 2004 (full reference below) is the most authoritative source for the current terminology. I suggest that you address this complexity and explain your use and be consistent throughout (which you are now, but by treating it as a Formation, which is not correct everywhere in the study region).

> We have now modified section 3 to incorporate a more nuanced discussion of Ogallala lithostratigraphic terminology and to emphasize differences in the north-to-south extent of the Ogallala. We also continue to use Formation when referring to the Ogallala, but explain that, in the northern Great Plains, the Ogallala is considered a Group.

2. Include all of the data in a supplemental with this paper, both the previously published and your new data, rather than really solely on Dryad for the new data. I could not access the new data on Dryad, though maybe it is not yet accessible or maybe it is pilot error? Regardless, I think including all of the underlying data in a supplemental with this paper is critical even with the data available via Dryad as well.

We apologize that the data was not available and admit that this was due to the fact that we mis-understood the directions on Dryad. The data should now be available at the following doi: 10.5061/dryad.5hqbzkhc5; however, if it is not yet available, please use this link to access it: https://datadryad.org/stash/share/sY5SXxVH-Hfp104CZ4m-grrcnOh-RAdwdbCNrF1Avcg. Climate of the Past appears to strongly prefer that datasets are presented not as supplemental material.

3. Address explicitly the age assignments and uncertainties. You discuss age uncertainty as a factor in comparisons to published model results, but you do not discuss in any detail the uncertainties in your age assignments beyond listing maximum and minimum ages in Table 1. What is the basis for the assignment of each section and how did you treat the uncertainty? Given that you do not examine the data as time series, this is not so much an issue with your interpretations as a matter of completeness.

We now explicitly address how we assign age constraints to our samples in lines 221-236. In short, we rely upon published studies that have studied these sections in more detail. In some cases, there is dateable material that provides a constraint; in others, the studied section is well-correlated to a dated section that provides constraints. And in still other sections, there is only a lithologic correlation to the Ogallala and no dateable material. We therefore adopt the broadest possible age constraints for samples from these types of sections.

4. Do you plan a separate paper for the carbon isotope data? If so, then perhaps including the new carbon isotope data from carbonates here is not necessary. However, if you do not plan a separate paper on the carbon data, then I suggest that you include at least the new carbon data (and probably all of the published carbon data as well) in the supplemental table of data and add a few sentences summarizing how the new carbon data compare to the published data. I recognize the carbon data are not the focus of this paper, and you can qualify some brief comments by saying a detailed discussion is beyond the scope of this paper, but I worry that the carbon data will be orphaned and lost if you do not plan to publish them and do not include them here. The published Miocene data (my papers that you cite and Lukens and Fox (2022. Palaeogeography, Palaeoclimatology, Palaeoecology 586. doi.org/10.1016/j.palaeo.2021.110760) have a strong central tendency (-7.2 to -6.8‰ V-PDB) and low variance, so a simple comparison of your mean and s.d. to the published data would suffice.

We are working on a separate manuscript that will describe the $\delta^{13}C$ data in detail. The $\delta^{13}C$ values are listed in Table S1 as well (though we realize that this dataset was mistakenly withheld from the reviewers). However, the $\delta^{13}C$ data do add to our interpretation (see response to Reviewer Dr. Smith) and, in general, are useful to include in such a paper and we therefore now append the $\delta^{13}C$ data to Table 1 (main text). Because we are working on a separate paper, we do not discuss the patterns in the $\delta^{13}C$ data in detail.

All of our new Ogallala data (*i.e.*, excluding data from the Santa Fe Group) has a mean $\delta^{13}C$ of -6.14 ‰ ± 1.39 (1σ). However, when we exclude data from sites primarily in

New Mexico that may be younger than late Miocene (see discussion in response to Reviewer Dr. Smith) and that Frye et al. (1982) correlated to the Ogallala based only on lithologic or geomorphic relationships, then the mean $\delta^{13}C$ for our data is -6.69 ‰ ± 0.88 (1σ). Similarly, the mean $\delta^{13}C$ of the compiled data we present in our paper (mostly, but not exclusively compiled from Fox and Koch (2003)) is -6.74 % ± 0.83 (1σ). We view this similarity as sufficient to support our contention that—outside of the Frye et al. (1982) sites—all of our samples record late Miocene climate.

We now include these summary statistics of our $\delta^{13}C$ data in lines 351-362.

Specific comments (indexed by line number)

36. It might be worth noting the influence of longer timescale climate fluctuations here, particularly ENSO. As I understand, the onset of the Dust Bowl coincided with a particularly severe El Nino event, and the little Dust Bowl in the 1950s also coincided with a string El Nino. These longer frequencies in the climate system are obviously not the focus of your paper, but they are relevant in the intro it seems.

We have now included brief mentions of long-term climatic oscillations that have been invoked to explain extreme droughts and floods in the region.

39. Is Powell's work relevant beyond being antecedent? He is a somewhat complicated figure historically, perhaps less so than others, but the mention of him does not do much work here. He is traditionally treated as somewhat of a founder and hero in North American geology and geomorphology, but I am not sure everyone in North America views him so positively. That said, I don't think your treatment is problematic.

As you point out, this mention of Powell does not add to the manuscript, so we have eliminated this phrase.

48. Check on the regional nomenclature for the Ogallala. It is a Group in Nebraska with multiple constituent Formations, but I think used as an undifferentiated Formation elsewhere in the region, certainly in Kansas (see Ludvigson et al., 2009). My sense is that in Texas, different authors use it as either a formation or a group, but I am not sure which is currently formally correct. You should point this out and establish here a terminology that you will use throughout for the lithostratigraphic unit. For example, you could use Ogallala for the unit and specify "Ogallala Aquifer" when refering to the aquifer. I think Tedford et al. (2004) (see note for line 182) is the most definitive authority on the regional terminology.

We have now clarified the terminology that we will be using in this paper in section 3 (lines 188-191). We also have re-worked section 3 to provide a more thorough background on the regional differences in the Ogallala Formation (*i.e.*, that it is considered a group in the northern Great Plains, but an undifferentiated formation in the southern Great Plains).

51. To this list of references, thanks to my slow review, you can add Korus and Joeckel, 2023. Telescopic Megafans on the High Plains, USA Were Signal Buffers in a Major Source-To-Sink System. The Sedimentary Record 21. https://doi.org/10.2110/001c.89096.

Now included.

52. The Ogallala is not capped by a single, laterally continuous caprock. This idea was prominent in the early literature on the stratigraphy in the region, but is not correct.

We have modified the wording here to denote that this "caprock" is really only present in the southern Great Plains and that it appears in places there, but we do not mean to imply that it is regional extensive.

54. In Nebraska, the Ogallala also lies on top of formations of the Arikaree Group.

Noted and included now in the manuscript.

148. Should this be "between the land surface and the atmosphere"?

Yes, now fixed.

161. I am not sure either of these are the best citations for the orographic effects on precipitation amount and d18O (e.g., Rozanski et al., 1993).

We have now included additional references here.

182. You should cite Tedford et al. (2004) here as the most recent detailed synthesis of the mammalian biostratigraphy in the region for the study interval, and it includes more or less all of the reliably dated ashes to date. Tedford, R.H., Albright III, L.B., Barnosky, A.D., Ferrusquia-Villafranca, I., Hunt Jr., R. M., Storer, J.E., Swisher III, C.C., Voorhies, M.R., Webb, S.D., Whistler, D.P., 2004. Mammalian Biochronology of the Arikareean through Hemphillian Interval (Late Oligocene through Early Pliocene Epochs): Late Cretaceous and Cenozoic Mammals of North America: Biostratigraphy and Geochronology. Columbia University Press, New York, pp. 169–231.

We have now included this citation here as well as in additional locations in the manuscript.

183. See my earlier comment about lithostratigraphic nomenclature.

We now use consistent terminology throughout to describe the Ogallala Formation.

186. The citations here could include Tedford et al. (2004), but also more primary literature on each unit.

We have now added additional references here as well as Tedford et al. (2004).

189. "in Texas" needs to be moved as the Blackwater Draw Formation is only in Texas and only overlies the Ogallala in places there. You need to be clear and specific about this here as the Ogallala is overlain by high energy deposits in places elsewhere (i.e., the Stump Arroyo Mbr of the Crooked Creek Fm in SW Kansas and the Broadwater Fm in W Nebraska).

We have now modified these sentences to better describe the regional variability in the overlying sedimentary units.

192. The Ogallala includes multiple stratigraphically distinct cap rocks or mortar beds and not one regionally extensive or continuous one and not only one at the top of the section.

We now note that this prominent caprock exists only in the southern Great Plains that separates the Ogallala from the overlying Blackwater Draw Fm.

203. As suggested before, you should clarify the stratigraphic nomenclature earlier and make sure it is complete and accurate.

207. This is true almost everywhere and it is well documented that the caprock is not a single unit stratigraphically.

We have now clarified that, when we discuss the caprock, we are primarily referring to the "caprock" in the southern High Plains and do not mean to imply that it is a single bed nor formed at a single time in Earth history. We hope this language now clarifies that.

219. Could add Tedford, 1981. Mammalian biochronology of the late Cenozoic basins of New Mexico. Geological Society of America Bulletin 92: 1008-1022.

Added.

228. The references for the age assignments for each section should be given in Table 1 so that readers can evaluate the age assignments on their own. You need to state here how you assign a specific age to each section and/or sample given that the sections have age ranges. Are all samples in a section given the same age? Do you assume a sedimentation rate and assign ages in stratigraphic sequence, and, if so, how do you calculate sedimentation rate?

We now clarify in this paragraph (lines 221-236) how we assign ages to the samples. We have now updated Table 1 to also include the references that provide the age constraints. We do not, however, that our analysis strategy is not particularly sensitive to the precise ages of individual samples in our sections.

235. Be explicit about how the standards were used…Which was used to correct to the V-SMOW scale and which were used for runtime QA/QC? This needs to be reported clearly. What is the analytical precision and how was that determined?

This has now been spelled out in detail in the methods. We use the IUPAC-recommended equations in Brand et al. (2014) to convert our data from VPDB to VSMOW. No

standards are used for runtime QA/QC due to the consistently lower than 0.1‰ reproducibility of the standards, and this is standard operating protocol at the ETH Stable Isotope Laboratory (S. Bernasconi, personal communication, 2024).

295. Here and elsewhere, what is the basis for the order of citations? It does not seem to be alphabetical nor chronological.

Climate of the Past does not appear to have a preference for the ordering of citations.

331. The table of published data should include the original published C and O values and your calculated paleoprecipitation values.

We now include the original published carbonate $\delta^{13}C$ and $\delta^{18}O$ values, with the $\delta^{18}O$ values converted to VSMOW.

332. I see no reason not to include the full data table as a supplemental to this paper also so that the data are with the interpretation. I cannot access the data using the doi nor by searching on Dryad, though perhaps the data are not yet posted or accessible?

It appears that Climate of the Past strongly discourages supplements of this nature. We have now ensured that this data is publicly available via Dryad (please use this link if the doi remains unavailable: https://datadryad.org/stash/share/sY5SXxVH-Hfp104CZ4m-grrcnOh-RAdwdbCNrF1Avcg).

434. Higher is probably a better word choice here than greater.

Fixed.

469. Tedford et al., 2004

Added

537. One factor you do not discuss is the difference in water use efficiencies and evapotraspiration fluxes of woody vegetation vs. C3 grasses vs. C4 grasses. This is embedded in your consideration of land surface characteristics, but there is literature on this could be of use. The phytolith data suggest that grasslands were present throughout the Miocene, and the carbon isotope data have been interpreted as indicating a constant amount of C4 grasses throughout all of or almost all of the Miocene, so these patterns are consistent with your lack of a spatial signal in the d18O data.

Thanks for pointing this out. We now include a few sentences (lines 590-592) discussing this point.

545. data have

Fixed.

References cited in response to reviewer

Brand, W.A., Coplen, T.B., Vogl, J., Rosner, M., Prohaska, T., 2014. Assessment of international reference materials for isotope-ratio analysis (IUPAC technical report). Pure Appl. Chem. 86, 425–467. https://doi.org/10.1515/pac-2013-1023

Fox, D.L., Koch, P.L., 2003. Tertiary history of C4 biomass in the Great Plains, USA. Geology 31, 809–812. https://doi.org/10.1130/G19580.1

Frye, J.C., Leonard, A.B., Glass, H.D., 1982. Western extent of Ogallala Formation in New Mexico. New Mex. Bur. Mines Miner. Resour. Circ. 175, 41.

Kim, S., O'Neil, J., 1997. Equilibrium and nonequilibrium oxygen isotope effects in synthetic carbonates. Geochim. Cosmochim. Acta 61, 3461–3475.

Tedford, R.H., Albright, L.B., Barnosky, A.D., Ferrusquia-Villafranca, I., Hunt, R.M., Storer, J.E., Swisher, C.C., Voorhies, M.R., Webb, S.D., Whistler, D.P., 2004. Mammalian Biochronology of the Arikareean Through Hemphillian Interval (Late Oligocene Through Early Pliocene Epochs), in: Woodburne, M.O. (Ed.), Late Cretaceous and Cenozoic Mammals of North America. Columbia University Press, New York, NY, USA, pp. 169–231. https://doi.org/10.7312/wood13040-008