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

Review by anonymous reviewer 1:

This paper presents an analysis of oxygen isotopes in modern water isotopes and ancient paleosols from the Great Plains. The authors apply a vapor transport model to see whether this simple 1-D model can explain observed gradients of isotopes (the model does relatively poorly), and also analyze whether these gradients have changed in the past. The abstract and paper text are well written, and it is overall appropriate for the journal. Some conceptual considerations:

> We thank the reviewer for their thoughtful comments.

Table 1 is helpful, but it would be helpful to have a map figure where sites are color-coded by their lower age and/or upper age.

> All of the samples presented are Miocene in age. In this study, since we are not comparing the sections across different stages of the Miocene (and to keep the figure readable), we stay with the current figure (though see changes in response to Reviewer Dr. Smith). All ages are available in Table 1.

The mismatch between the reactive transport model and observations is quite dramatic, and therefore I would like a greater discussion of the sources of mismatch between the reactive transport model and the data. Rainout is definitely a factor that can affect isotopic gradients, but what about storm statistics and the changing location of certain types of storms (see papers below)?

Sun, C., Shanahan, T.M., DiNezio, P.N., McKay, N.P. and Roy, P.D., 2021. Great Plains storm intensity since the last glacial controlled by spring surface warming. Nature Geoscience, 14(12), pp.912-917

Maupin, C.R., Roark, E.B., Thirumalai, K., Shen, C.C., Schumacher, C., Van Kampen-Lewis, S., Housson, A.L., McChesney, C.L., Baykara, O., Yu, T.L. and White IV, K., 2021. Abrupt Southern Great Plains thunderstorm shifts linked to glacial climate variability. Nature Geoscience, 14(6), pp.396-401.

> Here, rainout and the effect of changing storm statistics/storm locations on $\delta^{18}O$ are simply different sides of the same coin – the climatological pattern of rainout is the time integration of many storm events, each characterized by its own rainout pattern. We feel that extending our interpretation to infer specific storm-scale features (rather than the more general diagnosis of rainout) would go beyond what our data reasonably permit.
>
> In any case, comparisons with the Last Glacial Maximum are potentially not useful in understanding geologic shifts in $\delta^{18}O$ for a couple of reasons. First, both papers demonstrate that changes in the ice sheet extent (particularly the Laurentide ice sheet) affect the intensity of the Great Plains Low-Level Jet (GPLLJ). In turn, changes in ice sheet extent between now and the late Miocene do not affect GPLLJ intensity. Second,

Sun et al. (2019) show that springtime insolation—modulated by precession—impacts GPLLJ intensity via changes in the zonal pressure gradient. Precession is a large unevenly distributed forcing change on the Earth system; in contrast, changes between the modern and the late Miocene are more likely driven by CO_2, which provides a more globally uniform forcing and is therefore not directly comparable to orbital changes. Plus, our Miocene samples likely integrate signals across orbital timescales, given the timescale of formation of authigenic carbonates (Berner, 1968). Thus, we do not expect to see orbitally driven variations in $\delta^{18}O$ in our dataset.

Another thing I noticed is that the paleosols in this study extend to roughly 40-45 N. If you look at the climatology over the modern GPLLJ, in the modern climatology the jet counts (e.g. calculated on daily data) extend to roughly 45 N:

Helfand, H.M. and Schubert, S.D., 1995. Climatology of the simulated Great Plains low-level jet and its contribution to the continental moisture budget of the United States. Journal of Climate, 8(4), pp.784-806.

Would you actually need sites that are even farther north to detect poleward extensions/expansions of the GPLLJ, especially in past warm climates? It seems that the latitudinal range of samples in this study would be most appropriate for detecting contractions of the jet's intensity or northward extent? Are there past changes that could not be detected by the current dataset? However, I do agree with the overall conclusion about air masses, since if North American topography was high in the Miocene, we would expect a general pattern of mixing between the low level flow and midlatitude air masses.

While having additional spatial data will always help improve interpretations of spatial patterns in $\delta^{18}O$, we are not aware of any data to the north of the Ogallala Formation during our study time frame. With that said, the core of the jet, according to Helfand and Schubert (1995), lies within the middle of our dataset (between ~33° and 43° N) allowing us to resolve changes in GPLLJ intensity.

Dynamically, future changes in the GPLLJ have been linked to changes in the position of the North Atlantic Subtropical High, and this literature should be discussed in the manuscript (another paper by this team is already cited in the MS). I may be wrong, but Zhou et al are specifically references the westerly jet position over the Atlantic and its relationship to future changes in the GPLLJ, not necessarily the upstream jet over the west coast

Zhou, W., Leung, L.R., Song, F. and Lu, J., 2021. Future changes in the Great Plains low-level jet governed by seasonally dependent pattern changes in the North Atlantic subtropical high. Geophysical Research Letters, 48(4), p.e2020GL090356.

We thank the reviewer for this reference and have included this reference as well as a short discussion of changes in the North Atlantic Subtropical High and links to the GPLLJ (lines 566-567).

References cited in review:

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