Thank you for addressing my comments and remarks. I believe that the revised version of the manuscript is much improved, and that most concerns have been well addressed and clarified. However, there are still some details that need to be resolved, and I therefore recommend minor revisions.

My main concerns are about the precipitation dataset and how the average isotope compositions of snow and rain were calculated. These aspects are important for the isotopic framework and were partly clarified in the first revision. However, some further justification and/or modifications are needed, as described below:

> Thank you for your feedback. In general response to your comments, many of the things that you suggest would indeed improve some aspects of the isotope framework. However, many of these suggestions lead to relatively small changes in the isotope framework, and overall if implemented would not change the overall interpretation of the isotope data and hydrology. Additionally, some of the changes you suggest using the Fritz et al. (2022) data are not possible because they do not provide event-weighted data as you suggest, and because the Fritz et al. (2022) snowfall data is likely less accurate to use versus the snowpack isotope data we collected, given any possible post-deposition fractionation.

• *How to calculate δP? You state that δP is the average of the two δR and δS values, but in that case the values don't seem to be correct? The average of -17.03‰ and -24.61‰ is -20.82‰ (not -21.24‰), and the average of -129.54‰ and -184.19‰ is -156.87‰ (not 160.1‰). Examining the data (https://doi.org/10.5683/SP3/AZE4ER), I don't understand how you arrive at your δR values, as I get different averages (-16.67‰, -128.36‰)? This also results in different δP values. Besides, is this the best way to calculate δP? Instead of calculating an average of the two δR and δS values, I suggest calculating a mean annual value using all precipitation values, which is perhaps better done using the Fritz et al., (2022) dataset? See the following comments related to this*

> Thank you for checking our values, it seems we had made some errors when excluding rainfall vs. snowfall and ice-covered vs. ice-free data. All of the data is corrected now in the tables and figures. While weighting the precipitation by total amount would likely garner a better estimate of the true δ_P, establishing δ_P halfway between δ_S and δ_R as we have done allows one to easily see which lakes are rainfall-dominated or snowmelt-dominated on Figure 3. We therefore decided to keep δ_P defined in this way. Again, while the Fritz et al. (2022) data would capture true δ_S (i.e. the average isotope composition of snowfall itself), however we believe it is better to use snowpack data that we collected to automatically amount-weight the data and account for the small amount of post-depositional fractionation that may have occurred.

• *Why not use the precipitation data presented by Fritz et al. (2022)? You mention that Fritz et al. (2022) published precipitation data from 2015-2018. Isn't it better to use this larger dataset (or the year preceding your sampling) to calculate δR, δS, and δP? By sampling the end-of-winter snowpack, you measured the amount-weighted snow isotopic composition, but you did not amount-weight the rain data? If using the event-based Fritz et al. (2022) data, the rain and snow data can be treated equally. Also, by amount-weighting the data when converting them into average rain, snow, and annual values (assuming they measured both precipitation isotopes and precipitation amounts?) you take the "amount effect" into account.*

We agree our values for δ_S, δ_R, and δ_P are not be as representative as the long-term average for Inuvik as Fritz et al. (2022). But, as we said in our previous reply, using their data makes very minimal difference to the overall isotope framework, and we already demonstrate this in the manuscript when we compare our LMWL to theirs. As mentioned earlier, the Fritz et al. (2022) data is not event weighted. Fritz et al. (2022) is also missing some of the early September rainfall in 2018 (their last data point is on 31 Aug 2018).

• *Difference between your and the Fritz et al. (2022) data? I agree with you that a reanalysis using the Fritz et al. (2022) data might not change the results much, but because there are more suitable data available, why not use them? You mention in the manuscript that the δS, δP and δR values found by Fritz et al. (2022) were within 0.6‰ (δ18O) of the values you found. How did you come to this value? Using the values you provided, δS differs by 0.81‰ (-23.8 -(-24.61)), and δP presumably more. Furthermore, I think it makes more sense to calculate an average δP using all data directly, than to calculate an average of two averages. I get non-amount-weighed δP of -19.2‰ (δ18O) and -148.8‰ (δ2H) when using all the Fritz et al. (2022) data, which differ notably from your values (-21.24‰ for δ18O, -160.1‰ for δ2H).*

With the corrected δ_P measurements our δ_P is now within 1.4 ‰ $\delta^{18}O$ of Fritz et al. (2022) unweighted. We had been comparing our 2018 data to their 2018 data to come to this comparison in our previous reply. As mentioned earlier, using the Fritz et al. (2022) data comes with a different set of uncertainties, making it unclear if it would actually be more accurate to use in our isotope framework. And as we mention earlier, our definition of δ_P is more as a diagnostic/reference point in the isotope framework to split between snowfall- and rainfall-dominated lakes. Since we define clearly how we calculate δ_P, and it is the typical way to do so in lake isotope hydrology studies, we believe that it is satisfactory to calculate it in this way.

• *Post-depositional fractionation? When answering to the question about post-depositional fractionation processes, you refer to Ala-Aho et al. (2021) to say that there is likely minimal change in isotopic composition after the snow is deposited. That is not what I read from the Ala-Aho paper. They suggest that post-depositional enrichment of heavy isotopes does occur over winter. A better argument is that the average snow value is close to the average by Fritz et al. (2022), who sampled event-based samples, or as I suggest, to use the Fritz et al. (2022) data instead.*

We had written that post depositional fractionation is minimal, which we would argue it is – looking at Figure 9 in Ala-Aho et al. (2021), one observes that the falling snow is within the range of the snowpack isotope compositions corresponding to that snowfall event. If it is your opinion that there was significant post-snowfall fraction that occurred, then it then makes sense to use the snowpack samples we collected which naturally account for post-depositional fractionation, and not to use the snowfall samples collected by Fritz et al. (2022) which do not account for post-depositional fractionation. Therefore, we don't agree that it would be better to use Fritz. et al. (2022) snowfall data over our own.

Please find additional comments in the attached pdf.

All pdf comments were addressed except those related to issues addressed here (e.g. using Fritz. et al (2022) data).