The study by Brashear et al. shows how stable water isotope interannual variability on the Greenland ice sheet changes throughout the Last Glacial, being stronger during stadials than interstadials, with peaks preceding D-O events by hundreds of years.

They adjusted minor irregularities in the text and further
· show the robustness of their results towards diffusion correction,
· discuss possible uncertainties of the diffusion estimates related to systematic density changes,
· show in a graphic how accumulation rates coevolve with the high frequency isotope variability.
With this, they address some of the issues we raised in the first review. We therefore think that the paper is in a good state for publishing.

We have just a few comments left to revise:

Two minor comments:

L.100: "*decouple from the drivers of mean local climate (e.g. temperature, accumulation, etc.)*" – use either "e.g.", or "etc." - Also, how does this study show that the drivers of high frequency isotope variability are temporally decoupled from accumulation? It does not, as it does not include any accumulation rate analysis.

Fig. 8A; Thanks for adding this graphic. In the review answer you state: "As expected, declines in variability lead accumulation shifts by hundreds of years for most D-O events. This suggests precipitation intermittency and stratigraphic noise during cold stadial phases cannot account for the early shifts in 7-15 year variability, relative to D-O warming". As changes in stratigraphic noise and precipitation intermittency with time cannot be quantified, while higher accumulation rate changes might facilitate signal preservation, (which do seems strongly correlated to high frequency variability), noise changes could still be a reason for variability changes, which should still be stated in the text as one (counter?) hypothesis.

And our comment on the method description was not solved and still needs to be adressed

Review Round 1:

- **185: The method description is too short to be reproducible. If I understand it right, it needs to assume / assumes that 1.) P0(f) is not frequency dependent and the fit takes only place on frequencies lower than a manually chosen fc to ensure that the spectrum is dominated by the diffusion signal in this range of frequencies and measurement noise can be ignored.**
  o The authors feel the description accurately and succinctly describes the methods used in this study and is consistent with prior studies (Jones et al., 2017b; Jones et al., 2018; Jones et al., 2023; Kahle et al., 2021)
  § Po(f) is still frequency dependent based on its definition in equation 3

§ It is unclear what the variable ‘fc’ is in reviewer comment, but it is correct that the correction fit is placed on frequencies affected by diffusion and not analytical noise

While we acknowledge that the specifics of the diffusion correction do not alter the results, we insist that the method description must be comprehensive enough to ensure full reproducibility, as this is standard good scientific practice.

Currently, it is unclear in which frequency range the fit is performed or how this range is determined (e.g., manually for each depth or using a single range). Additionally, it is unclear whether the fit is applied to P or $\ln(P)$, as suggested in line 190: “Diffusion length, σ_a, can also be quantified as the slope, m, of a linear regression, y, fitted to $\ln[P(f)]$ versus f^2 of the diffused interval.”

The references cited use different approaches. If I read it right, Kahle (2021) accounts for the red CFA spectra by fitting two Gaussian distributions, whereas Jones et al. (2018) uses only one Gaussian “fits to the frequency at which there is a distinct slope break in $\ln(PD)$.” Therefore, simply citing these references does not provide the reader with a reproducible method.