Response to comments from Reviewer 1

Authors' replies in blue. All references to figures, equations or lines in the paper refer to the initially submitted paper.

The general model and math underpinning the updated calculations for diffusion length estimates for low-resolution, discrete data make sense as presented. However, I do have a major concern that needs to be addressed before this paper could be published. Hopefully, this concern can be addressed with only minor revisions.

My concern is centered on the white-noise power spectra that are used in the modeling. As a common example, the power spectra of ice-core water-isotope data typically exhibit white noise at periodicities less than multi-decadal scales (excluding the effects of diffusion, which results in a pseudo-red spectrum), but at periodicities greater than multi-decadal scales the spectrum is red. (This is a very general summary of the shape of the spectra, but good enough to make my point). The authors need to address this red noise at periodicities greater than multi-decadal scales. If an ice core site has a significantly large diffusion length in deep ice, is the model still valid? Is it valid to fit a white noise spectrum in the model for deep ice with diffusion lengths of 30 cm? I think there are instances where a white noise consideration would not be valid, and that large amounts of diffusion would not only alter the white noise part of the spectrum but also the red noise at periodicities > multi-decadal scales. The red noise portion of the spectrum is not the same across ice cores, thus the fitting of spectra to estimate diffusion lengths would have to account for these differences, otherwise an additional aliasing could occur. Similarly, if diffusion has significantly affected the red noise portion of the spectra, there is no way to know what the original red noise was (i.e., the power law to describe it). I would like the authors to produce a number of examples using varying ice core sites with varying outlier characteristics (low accumulation, significant thinning at depth, and significant solid-ice diffusion) to demonstrate that this method is generally valid for deep ice. As of now, the authors are not clear for what cases this model may be valid or not valid. If the method is not valid for certain sites, please explain why. If the method is valid for all sites, explain how. If there should be certain considerations to determine if the model is valid for a particular ice-core site, explain what those considerations would be. If I have misunderstood something about the model or the approach, please explain.

We thank the reviewer for their comments and well-made point regarding the red power spectrum observed for timescales greater than multi-decadal. Indeed, this is a crucial consideration for deep ice cores and one which we address in our model choice. The diffusion length estimation method applied in all cases in this paper is an adapted version of that proposed by Shaw et al. 2024, a publication which specifically addresses the power-law complication of estimating diffusion length from deep ice cores spanning millennial timescales.

The novel approach takes the foundation of the conventional estimation model but replaces the white-noise assumption with a red power-law representation of the isotopic record before diffusion, $P_{X0}(f) = \alpha f^{-\beta}$, whose amplitude and slope are estimated jointly with the diffusion length and the measurement noise rather than being fixed in advance. We would like to stress that no white-noise assumption enters the theory of Sect. 2 either: the Gaussian diffusion filter and the sampling terms of Eq. (21) are applied to whatever form $P_{X0}(f)$ takes, and the derivation nowhere separates a white from a red part of the spectrum. We will add a statement to this effect in Sect. 2 and clarify in Sect. 4 which parameters are free in each experiment.

As correctly identified by the reviewer, the challenge with this new red noise approach is deriving the correct power law from strongly diffused data. Where a large diffusion length has removed power well into the longer timescales, few frequencies remain from which the power law can be derived. Since different ice cores show a considerable variety in the slope and amplitude of the red portion of the spectrum, the method we applied from Shaw et al. (2024) uses less-diffused data from further up in the same ice core, taken from time periods with similar climate states. By using Bayesian inference, the model fit accounts for the potential differences between these climate states while still informing on the likely structure of P_{X0}. For MIS 19 in Dome C, using MIS 1, 5 or 9 individually as the reference period changed the diffusion length estimate by less than the estimation uncertainty (Shaw et al., 2024, their Table 3). This method is therefore valid for any ice core where P_{X0} can be inferred from other time periods or even other proxies. We would add that this consideration applies in equal measure to the conventional estimator (Eq. 5) and to the estimator proposed here (Eq. 21), so that the comparison shown in Fig. 3 is unaffected by it.

The focus of this paper was on the discrete sampling effects and how they can bias diffusion length estimations. Since all of the records in this paper are simulated, we knew the exact power-law underlying all the deep ice records, and so provide accurate, informative priors for P_{X0} and measurement noise. However, for the diffusion length prior, we chose a very weak prior with a mean 50% greater than the true diffusion length, to reveal when the model fails to fit the data and defaults to the mean of the priors.

The new adaptation to the model which accounts for the sampling effects is equally viable for sites with very small or very large diffusion lengths. For sufficiently large diffusion lengths the impact of discrete sampling is negligible, as the additional aliasing and spectral leakage effects act on timescales where nearly all the power has been lost due to diffusion, so the power spectrum on those frequencies is dominated by measurement noise. The method used in this paper correctly models this. The estimate additionally requires the sample interval to be small enough that the diffusive roll-off falls within the resolved frequency band; where it does not, the diffusion length and the power-law slope are no longer separable.

The authors do appear to partly address my concerns in Figure 3 by using a power-law deep-ice record. There is not much said about how this particular case was chosen ($P_{X0}(f) = 0.15f\^{-2.2}‰^2m$ and σ = 30 cm). Please explain why you chose this particular case.

This power law for the deep ice case was taken from the Dome C composite spectrum from Shaw et al., 2025, similar to the firn case but from the longer-timescale regime instead. Selecting data-derived values provided a representative power spectrum of a deep ice core isotope record. The diffusion length of 30 cm was chosen from the diffusion length estimated by Shaw et al. 2024 from the deepest section of the Dome C ice core. We will add this to the text to make clear the origins of the chosen values.

Other power law slopes and diffusion lengths would similarly demonstrate the effectiveness of the new model at removing the sampling-induced bias; we simply chose two edge cases (firn and very deep ice) as examples. The relevant parameters are the diffusion length, the ratio of sample interval to diffusion length and the slope of the background spectrum, while the site characteristics named by the reviewer (low accumulation, strong thinning at depth and significant solid-ice diffusion) are captured entirely by these parameters. We are happy to add a sensitivity experiment spanning a range of power-law slopes should the reviewer consider it useful.

Once this matter is addressed and explained more in depth, the paper would be suitable for publication. I do not have any concerns with the writing or grammar. It is worth mentioning again, I may have misunderstood something about the model or approach, and if so, please explain the misconception.

Thank you for the nice paper for considering these changes.

We thank the reviewer again for raising this point. It prompted us to state the treatment of the underlying spectrum explicitly rather than leaving it implicit in the reference to Shaw et al. (2024) and these changes will make the scope of the method clearer to readers.