Dear Dr. Dominé,

We appreciate your recommendation, and are grateful to the two anonymous referees and Dr. Hastings for their comments.

We have made point to point responses to the two anonymous referees, and think we have addressed their concerns by adding more details on how the wavelength-dependent e-folding depth was computed in the model, as well as a summary table presenting all necessary parameters used to constrain the model, etc. Details can be found in our point-to-point response to these referees.

Regarding to Dr. Hastings's comments, apparently there is a strong disagreement between the two groups (us vs. Dr. Hastings), and we appreciate the questions raised here as it offers an opportunity to discuss with details. First, we thought Dr. Hastings may have misunderstood the purpose of this manuscript as well as the TRANSITs model that we used. The manuscript and the model were aiming to predict the **changes** of isotopes of nitrate from its primary deposition to final preservation, and to what degree the magnitude of the observed seasonality can be explained by post-depositional processing. Keeping this in mind, we did not ignore any relevant observation and the model agreed with observations just well. Second, we don't agree with Dr. Hastings' previous conclusion that there is little to no changes caused by post-depositional processing. Because neither surface snow nitrate alone can be used to determine the degree of post-depositional processing, nor can O-isotopes be used to quantify post-depositional processing. These, however, were what Dr. Hastings's previous work relied upon to draw their conclusions (Fibiger et al., 2013; Fibiger et al., 2016). We have made specific rationales in our point-to-point response to Dr. Hastings to elucidate the above-mentioned points.

We are looking forward to hearing from you, and any additional suggestions/comments improving this manuscript would be highly appreciated.

Best regards,
Authors