First, we would like to thank referee No.2 for the contribution of constructive comments to this work. They are addressed below in order and corresponding changes will be included in a revised version of the manuscript. Note that most issues are addressed in detail in the reply to reviewer No.1

1. We did not attempt to explain the survival of the 2.7yr signal in $\Delta^{17}O(NO_3^-)$ at Dome C and South Pole. Any speculation before building a quantitative post-depositional model for isotopic fractionation would be premature. However, it should be noted that at sites of very low annual accumulation the annual signal in most chemical parameters is lost, including major ions or the stable isotopes of water. One of the factors is physical redistribution of surface-near snow by wind, which can lead to mixing of layers or the loss of an entire annual layer (roughly 10 cm of snow at DC). The result is smoothing of the atmospheric signal.

2. We consider fractionation in NO_3^- occuring after deposition in the upper snowpack and compare to fractionation processes known from gas phase chemistry. We note that something unusual occurs in the snow.

3. A discussion of evaporation is essential for the interpretation of post- depositional fractionation. We updated our model (see reply to reviewer No.1) and rewrite stating where the assumptions still need experimental validation. We concur that the question evaporation vs. photolysis is not entirely settled yet and will change our conclusions accordingly.

4. We expect isotopic substitution to make a difference in the UV absorption spectrum of NO_3^- and therefore think that PHIFE is a valid first modeling approach for ε of $^{15}N(NO_3^-)$, noting that there are uncertainties in the absorption spectrum estimates and ε. Lab experiments needed to confirm model predictions will be part of future work (see reply to reviewer No.1).