Reply to comment by Martin F. Miller on "Record of δ 18 O and 17 O-excess in ice from Vostok Antarctica during the last 150,000 years"

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[1] *Landais et al.* [2008] concluded that recent Antarctic precipitation has ^{17}O-excess with respect to ocean water and that this excess was lower in glacial than in interglacial times. We further suggested that the change in ^{17}O-excess indicates smaller effect of kinetics signifying higher normalized humidity in the oceanic source region during glacials. In his comment, *Miller* [2008] questions the validity of these conclusions on three grounds. First, he emphasizes that the measurements are reported with respect to the VSMOW standard and not with respect to ocean water and therefore ^{17}O-excess with respect to the ocean may not exist. We note, however, that this point was raised by us [*Landais et al.*, 2008, p. 3] and we emphasized the need for precise calibration with respect to seawater. Recently, we [*Luz and Barkan*, 2008] have made precise calibration and reported that the triple oxygen-isotope composition of VSMOW is similar to seawater. Therefore, we conclude that Antarctic precipitation unambiguously contains ^{17}O-excess with respect to the ocean.

[2] The second criticism, based on simple modeling by *Angert et al.* [2004], is about the conclusion of smaller effects of kinetics and higher normalized humidity in glacials. As an alternative, *Miller* [2008] suggests that temperature change between glacial and inter-glacial as a possible driving mechanism. We note however, that in recent Antarctic snow the ^{17}O-excess is conserved over a wide temperature range from the edge of the continent to the continental interior. Therefore, there is no basis for the claim that temperature change affects the ^{17}O-excess. We also point out that precipitation over Antarctica is a complicated process involving kinetic isotope effects in addition to equilibrium ones. As shown by *Landais et al.* [2008, Figure S1], modeling using the new fractionation factors from *Barkan and Luz* [2005, 2007] and proper choice of modeling parameters (e.g., supersaturation) yields a flat trend that fits our observations very well. *Angert et al.* [2004] did not have any observations on ^{17}O-excess in precipitation. In addition, because correct fractionation factors were not available to them, their modeling was less realistic and showed dependence of ^{17}O-excess on temperature.

[3] The third criticism is about possible effects of Mass Independent Fractionation (MIF) enrichment of stratospheric water that might reach ground levels in polar winter. However, the effect of stratospheric water on ^{17}O-excess in tropospheric vapor in Antarctica must be negligible according to the available data. Indeed, according to *Franz and Röckmann* [2005] the MIF effect in stratospheric water is 0 ± 1.8‰ over Antarctica with a large scattering in the data. *Yang and Tung* [1996] estimated the gross flux of stratospheric vapor into the troposphere as 1070 $kg.km^{-2}.yr^{-1}$. The water flux over our transect is minimal around the Dome C region with a precipitation rate of 3.10^7 $kg.km^{-2}.yr^{-1}$. Therefore, the maximum variations in ^{17}O-excess by the stratospheric water input would be observed around Dome C within ±0.06 per meg (1800 per meg*1070/30000000) and thus negligible. Even considering possible local and seasonal increase of the stratospheric flux over the polar region by a factor of 10–100 [*Karpechko et al.*, 2007], the dispersion of the calculated anomaly is not significant compared to the dispersion of our transect data. We also note that our recent measurements of meteoric water [*Luz and Barkan*, 2008] show that the Antarctic ^{17}O-excess is of the same magnitude as in meteoric water at low latitudes.

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