

Interactive comment on “A 70-yr record of oxygen-18 variability in accumulation from the Tanggula Mountains, central Tibetan Plateau” by D. R. Joswiak et al.

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Received and published: 25 November 2009

Response to major comments:

(1) You are not misunderstanding; you have pointed out confusing wording that was used. We meant to indicate the isotopic temperature dependence, since the isotope depletion depends on temperature. This wording was changed throughout the text.

(2) This shortcoming in the discussion and treatment of possible melt is acknowledged, and we clarified this question according to reviewer suggestions. Given the more rapid transformation of firn to ice associated with low net accumulation, visual detection of years with partial melt may have been insufficient. Although in situ temperature data

C947

are not available, previous studies and lapse rates suggest melt could occur. We have revised the manuscript to include additional geochemical data and discuss possible melt effects. We agree that the comparison to previous mass balance measurements should be included, and have revised the manuscript to include this in the results. Local precipitation variability may be expected given the variability of westerly- and monsoon-dominated moisture, relatively low annual precipitation in the central plateau, and the importance of localized convective precipitation in high relief mountain topography. Although these factors can contribute to high spatial variation, general agreement was found between the mass balance data and the TGL05 ice core accumulation.

(3) We also assume highest SO₄²⁻ concentrations are occurring during the cold, dry part of the year indicating major impact of dry deposition. A more complete understanding of the seasonal variability in SO₄²⁻ is needed in this local region. More discussion on the assumptions and validity regarding seasonality of ions was addressed in Section 3. The text was revised to include the proper soluble ionic species.

(4) The original works were cited

(5) This point was clarified in the results; the sampling intervals for major ions and isotopes were the same. The sulfate seasonality is addressed in the response to Anonymous Reviewer #3.

(6) The previous core data was added to the figure.

(7) We realize this is an important distinction, and have removed this citation which is not in support of the results.

(8) The degrees of freedom are indicated in the revised manuscript, and this error in reported probability was corrected.

(9) We agree the observed relationship between oxygen-18 values and N. India precipitation is indeed interesting. Analysis of precipitation records at the mentioned stations was performed and additional treatment of the meteorological data was achieved

C948

through comparison with the ice core accumulation. Although we compared only one core initially due to the geographic proximity, we acknowledge the necessity for additional comparison, especially with another core from a monsoon-dominated climate regime. We revised the manuscript to include further analysis and comparison with the previous Tanggula ice core, and with cores from Dasuopu and Everest.

(10) This point was also addressed by other reviewers, and a more complete description of circulation patterns was added.

In addition to these major comments, all specific comments were taken into consideration and addressed in the revised manuscript.

Interactive comment on Clim. Past Discuss., 5, 1929, 2009.