Clim. Past Discuss., 4, S114–S117, 2008
www.clim-past-discuss.net/4/S114/2008/

Climate of the Past Discussions

Interactive comment on "Response of regional climate and glacier ice proxies to El Niño-Southern Oscillation (ENSO) in the subtropical Andes" *by* E. Dietze et al.

Anonymous Referee #2

Received and published: 24 April 2008

This paper examines the ENSO impacts on both temperature and precipitation interannual variations in the western part of the subtropical Andes from different meteorological datasets. As climate at the Cerro Tapado drilling site appears to be ENSO-sensitive, the authors seek for ENSO signatures in the different Cerro Tapado ice records. The initial objectives of this paper are thus valuable. However, this study mainly relies on correlation analyses between meteorologial data and both ENSO indices and Tapado records that are not reliably conducted and thus not convincing. As a consequence, the major and interesting conclusion on the significant influence of ENSO on major ion concentrations in Tapado ice does not seem to be robust. Thus, I do not recommend this manuscript for publication in Climate of the Past in its present form and I list below

some important remarks that should be addressed in a revised version of this paper.

- Before computing any EOFs and correlation analyses, it is important for the reader to show and to discuss the raw data. Please, show (and compare) the time series of meteorological datasets (for each station), as well as the raw major ion concentrations records (not only PC1) along with isotopes and net acumulation records as a function of the depth.

- The works dealing with post-deposition processes at the Cerro Tapado site (Ginot et al., 2001, JGR; Ginot et al., 2006, CP and Stichler et al. 2001, JGR) have clearly shown that important changes of both water stable isotopes and chemical records are induced by sublimation and snow melting at the surface. In the manuscript, the authors do not discuss the potential impacts of those important effects and the related caveats for both the interpretation and the dating (see below).

- Regarding the dating of the ice core, a number of information are missing and I have two important comments: (1) The dating is only done by counting the annual layers on the first PC of major ion concentrations and on the isotpic composition of the ice: a- I do not see any seasonal cyles in the isotopic composition of the ice that could correspond to the PC1 high frequency variability. And, I am not sure to reliably distinguish sesonal cycles in PC1. b- I think that this method (using an EOF analyses) leads to important (higher) errors than a classical multi-proxy dating especially if some of the chemical proxies are altered by pot-deposition effects or in case some years are missing (how the authors estimate an error of plus or minus 2 years on the dating ?). I strongly suggest that the authors show, as a function of the depth and in a separate figure, some of the raw chemical profiles (calcium and sulfate for example) and point out on the latter and on the isotopic record the seasonal cycles. Please, also show the entire tritium profile if available. (2) How the authors are able to differentiate a chemical peak corresponding to a seasonal mineral dust input from a chemical peak due to a sublimation effect ? To show the link between seasonal cycles in isotopes and chemical markers could be a way to answer that question.

CPD
4, S114–S117, 2008

- A more reliable work has to be done about the meteorological records quality. How many stations are used in the Willmott dataset for the region of Cerro Tapado ? Does it dataset use homogenized data ? Are the precipitation and temperature records from the 4 meteorological stations consistent in terms of variations, amplitudes and absolute values (after altitude corrections) ? Could the authors show those time series ? Please, detail in section 3-1 the comparison between the Willmott dataset and the NCEP-NCAR reanalyses and the 4 meteorological series. The authors only give correlation coefficients without mentioning anything about the timeperiod used for the comparison and the comparison of absolute values and amplitude (slope of 1 ?). I do not see any correlation analyses in table 2 as mentioned on page 180. On page 180, "Missing values….the appropriate long term monthly mean". What does this mean exactly ? Is it correct to do that ? "Missing data in the Mendoza Observatory…. Mendoza airport". Same questions. Is the comparison between the two stations at Mendoza correct for all parameters and for different timescales ?

- The authors explain that at Cerro Tapado site, both precipitation and temperature could be explained by ENSO variations (figures 4, 5, 6) (although the maximum for the variance is of 30%). Then, the authors compare Tapado ice core records (PC1, oxygen 18 and net accumulation) with both temperature and precipitation to explore the link with ENSO (figure 8). It will be more direct to compare Tapado records with ENSO indices. In that case, what kind of ENSO signatures are expected on the isotopic composition of precipitation at the drilling site and on the major ionic concentrations ? After answering that question, please show the ENSO influence on the raw time series. I do think that if a strong ENSO influence exists on chemical records, as mentioned on page 186, there is no need of EOFs calculations and indirect comparisons to see it.

- There is a general problem in the discussions about correlation analyses. Actually, most of the time, correlations are significant but very low. For example, the authors point out that Tapado oxygen 18 record is correlated with summer temperature (figure 8), however r is of 0.3-0.4 which means that no more than 16% of the oxygen 18 signal

CPD
4, S114–S117, 2008

can be explained by summer temperature (so, this is not a "strong" influence as written at the beginning of section 4-2). Again, in case a real correlation exists, please show it on raw temporal series.

- Given my above remarks, section 4 seems to me highly speculative and not convincing. I would recommend to combine a new and robust section 3 including some possible climate mechanisms explaining the link between Tapado records and ENSO because in this actual form, section 4 is very hard to follow in places.

Other comments

- The authors use and discuss the net accumulation but I am wondering what are the controls of net accumulation ? - At page 179, the authors write "At higher latitudes, air temperature during precipitation dominates the isotopic fractionation of snow". This phrase has to be reformulated as it puts the reader in trouble. Whatever the latitude is, the isotopic composition of the condensate is mainly controlled by the amount of precipitation which is formed. At high latitudes, this amount is intimately related to the air temperature, mainly explaining the relationship between isotopic composition and air temperature. The impact of temperature on fractionation is a minor parameter. The authors can calculate an isotopic distillation at constant temperature to be convinced or see for example the very clear figure 18-23 in Cuffey and Brook, 2000, Ice sheets and the ice-core record of climate change. - Is the "Mediterranean Chile" (page 175) a common name for the studied region ? - On figure 2, please check the running average calculation on oxygen 18 that seems to be shifted compared with raw data in black.

Interactive comment on Clim. Past Discuss., 4, 173, 2008.