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# *Interactive comment on* "Siple Dome shallow ice cores: a study in coastal dome microclimatology" *by* T. R. Jones et al.

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This manuscript discusses new records of shallow ice core water stable isotopie composition from a north-south transect of 7 sites spanning about 60 km along Siple Dome, West Antarctica and covering between 1 and 3.5 centuries. The authors investigate two main features, (i) the spatial gradients of accumulation and isotopic composition between the different sites, and (ii) the different imprints of ENSO variability in the different shallow ice core sites, attributed to microclimate effects.

The manuscript in the present form is not suitable for publication and requires major revisions. While it provides an unprecedented effort to explore and quantify the inter-ice core variability around Siple Dome, key aspects are missing such as a precise description of the dating strategy and uncertainty, as well as an analysis of signal to

noise ratios. The logics of the presentation is also problematic. I recommend major revisions for this work to be accepted for publication.

Detailed suggestions :

- section 1.1 : you may want to explain and quantify using Rayleigh models how source and site temperature changes associated with ENSO and ASL changes are theoretically expected to affect water stable isotopes in Siple Dome ice cores.

- the order of sections is not logical. The site, measurements and dating methods should be introduced before a discussion of statistical methods (as in 1.2). I suggest to summarize the basis of isotope-temperature relationships in the introduction, and then have a section on Material and Methods.

- I cannot understand why the authors use a Rayleigh open cloud model to quantify isotope-temperature relationships. What is the local spatial gradient ? On which data is the unnumbered equation of line 20, page 2685 defined ?

- Page 2686 does not mention two key aspects : i) precipitation intermittency, and ii) wind scouring which both play a key role in signal to noise variability in shallow ice core records. The uncertainties on the isotope-temperature relationships from Jouzel et al (2003) are only valid for glacial-interglacial changes. Several studies have challenged the use of spatial gradients for temporal variations in coastal Antarctica.

- Section 3.2 should introduce the accuracy on deuterium excess measurements. The dating method should be described together with the associated uncertainty. This aspect is crucial for the rest of the paper.

- Table 2 should include error bars on average stable isotope and accumulation ratios, based on standard deviations. This would allow to check if differences between sites are significant or not.

- Section 3.3 is not properly placed. How much does the hypothesis of a constant phase with SOI affect the chronology ? The discussion of isotopic diffusion is useless



CPD

9, C1486–C1489, 2013

if this effect is not corrected for identifying annual layers and correcting measurements.

- The result section must be reorganised. I suggest to discuss first the spatial gradients and to combine Figures 8, 9, 10 into one single figure showing for the transect : (i) elevation, (ii) temperature, including temperature estimated from elevation effects only, (iii) accumulation rates, (iv) 18O (including 18O expected from a linear fit to accumulation) and (v) deuterium excess.

- A plot of excess as a function of 18O could be useful in order to identify if the points align or not on a single distillation line. The deuterium excess signals from the different ice cores should be shown somewhere (is there any coherency between the different shallow ice cores or not ?). On Figure 10, the authors show a decreasing trend, while I see two groups of points with different mean values ( like a step function).

- I am not convinced that the fits shown in Figs 8-10 do reflect spatial gradients. It seems that only site F is an Âń outlier Âż with respect to the other ones. Could dome shadow effects explain the differences ? Why are there two points Âń H Âż in the graphs ?

- Figures 3-4 bring no useful information. They could be replaced by a cross spectral analysis to look at the coherency between the various ice core records and the SOI, in order to check in which frequencies you have a common signal. If the dating uncertainty is 3.5 years as mentioned in the caption of Fig 5, then I am afraid that it is not possible to discussion the link with SOI.

- I suggest to replace Figs 5-6 by an analysis of the common signal in all the ice cores through a Principal Component Analysis, and then look at the link between the first PC and SOI. The signal to noise ratio could also be calculated and discussed.

- Sections 4.2 and 4.3 should be rewritten in order to investigate the spatial gradients in elevation, temperature, accumulation, their coherency and what they imply for isotopes. Then the isotope spatial gradients should be described with respect to the previous



CPD
9, C1486–C1489, 2013



information. In the last paragraph of page 2690, please check if such small temperature differences are significant or not.

- Page 2695 : this is very speculative and not quantified. An option could be to select years with specific known climate anomalies (large Nina events, or large SST anomalies) and see if there is a specific isotopic spatial pattern for these given years, compared to the other years.

- Links between modes of variability and local micro climate : couldn't atmospheric reanalyses or regional models be used to estimate what spatial structures are expected in this area ? The discussion is very speculative, and gives the impression that the authors have a theory, and want the observations to be consistent with this theory, without really testing it. Figure 11 shows the large scale atmospheric circulation but not the magnitude of the climate signals associated with it. This Figure could replace the schematic representation of Figure 1.

- Section 5.2 is very speculative.

- From this paper, one expects to discuss the representativity of a given shallow ice core record in terms of spatial and temporal scale of climate signals which are archived. This is somehow missing and the conclusion is quite vague. It is a pity due to the huge analytical effort conducted here and the fact that very few comparable studies have been conducted. The results should be compared to similar works for Greenland (e.g. at GISP2) and Vostok (work by Aleksey Ekaykin using shallow ice cores and pits).

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Interactive comment on Clim. Past Discuss., 9, 2681, 2013.