

***Interactive comment on* “Interpreting last glacial to Holocene dust changes at Talos Dome (East Antarctica): implications for atmospheric variations from regional to hemispheric scales” by S. Albani et al.**

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Dear Hubertus,

Thank you for your invitation to review the manuscript from Albani et al. untitled “Interpreting last glacial to Holocene dust changes at Talos Dome (East Antarctica): implications for atmospheric variations from regional to hemispheric scales”.

This is a very interesting paper, which presents new dust-flux and grain size records spanning the last ~23 kyr BP from the Taldice ice core obtained at Talos Dome, North-

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ern Victoria Land, East Antarctica. Preliminary investigations on this core [Delmonte et al., 2010b] have shown that this site is characterized by dust inputs from local Antarctic sources (compare to central East Antarctic Plateau (CEAP) sites fed by remote sources, mainly from Southern South America).

In this present manuscript, the authors compare in great details Taldice dust and water $\delta^{18}\text{O}$ records over the LGM-to-Late Holocene period with similar records obtained at EPICA dome C (EDC). This enables the authors to highlight the distinctive features of the Taldice records: in particular a smaller glacial-interglacial variation of the dust flux, a peculiar dust minima at $\sim 13\text{kyr BP}$ (i.e., before the end of the Antarctic cold reversal (ACR)), as well as a decreasing trend of the dust during the late Holocene. Although some of these features have already been described in Delmonte et al. 2010b, the detailed records presented here allow, among other things, to confirm that the differences with the CEAP records are partly due to the large and less variable contribution (across the studied climate transitions) from coarser particles deriving from “local” sources. Another interesting outcome of this study is the fact that, unlike what has been observed at EDC, dust and temperature ($\delta^{18}\text{O}$) at Talos Dome show an apparent correlation during the Holocene. The disentanglement between coarse (local) and fine (remote) contributions to the dust flux records is then used by the authors to discuss implications for atmospheric circulation changes, at a regional spatial scale in particular.

One of the main interests of mineral dust in ice cores is that it provides unique evidences for past air-mass trajectories and their possible changes with climate. Also, the comparison between central and peripheral sites (therefore located at different altitudes) holds some fundamental clues for our interpretation of the ice core records, dust records in particular. So, such an effort by Albani and co-authors to infer information on regional- vs large-scale atmospheric circulation features based on the comparison of ice core records at Talos Dome and EDC is to be commended.

Overall the manuscript presents some high-quality data, which are soundly interpreted, and it is rather well written. The last part of the discussion, however, would benefit from

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some clarification, and some sections could be tightened up a little bit here and there I think, mainly to avoid excessive reiterations. I would therefore recommend publication in CPD after the authors have addressed the comments listed below, which should only require minor revisions of the present manuscript.

Main comments

Results:

P 150, line 5: it might be useful to indicate here that such coarse particles ($>5 \mu\text{m}$) have been observed at other peripheral sites such as Berkner Island [Bory et al., Multiple sources supply eolian mineral dust to the Atlantic sector of coastal Antarctica: Evidence from recent snow layers at the top of Berkner Island ice sheet, *EPSL*, 291, 138-148, 2010].

P 151, line 10: could Sr and Nd isotopic end-members help in quantifying remote and local contributions?

Discussion:

P 151, first paragraph: I wonder how relevant this discussion on possible Australian dust reaching Talos Dome (according to models) is? Especially as the main findings of this study derive from the coarse (“local”) signal, not on the fine (remote) one. I would therefore suggest to remove it (preferred), or to move it some place else (in this case maybe P 153 in the second paragraph when EDC and Taldice LGM fluxes are compared). In any case, the connection between the first sentence (“The starting point. . .”) and the paragraph is awkward and so I would drop this sentence (which is clearly not essential).

Section 4.1, second paragraph (line 9): this paragraph doesn’t involve EDC at all; I would suggest to rename the sub-section 4.1 “Taldice dust flux-D18O relationship” or something like this (and start the following subsection “Comparing TALDICE and . . .” P 152 line 27). But because this paragraph does not really include many elements

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of discussion (it consists in a detailed description of the data), I would rather suggest moving it to the results section.

4.1, line 22: as it is written the sentence is ambiguous (and may suggest that the $\delta^{18}\text{O}$ shows linear trend but dust flux does not): I would suggest to rephrase (“However, neither the dust flux nor the $\delta^{18}\text{O}$ show linear trends”). Also, I think it would be important to show smoothed profiles for $\delta^{18}\text{O}$ as well in order to compare dust and $\delta^{18}\text{O}$ trends more easily.

Section 4.1, second paragraph: since the significant dust flux- $\delta^{18}\text{O}$ correlation during the Holocene is an important outcome of the paper, I was surprised not to find any dust flux vs $\delta^{18}\text{O}$ plot, where the better correlation at Taldice (especially for the coarse fraction) may be apparent when plotted together with EDC data. Similarly, I would have expected to find on figure 3 or 4 the comparison between the correlation of Taldice dust (coarse mode in particular) and temperature with time and similar correlation at EDC (as in Figure 2 of Lambert et al., [2008] for instance). I am sure there must be a good reason why such plots were not shown, but I think it would be of interest for the reader if the authors provided a few words on this.

P 154, lines 4-7: already described in details P 152 lines 15-20; also, the early dust minimum during the ACR is one of the most peculiar feature of the Taldice dust record and should be discussed further (if only to acknowledge that there is not yet a clear understanding of what may have caused it).

P 154, lines 10-12: similarly, this is already stated P 152 lines 20-22. The authors should carefully look for unnecessary repetitions in this bridging paragraph (and throughout the manuscript).

P 154, line 13: “rather stable” seems somewhat contradictory with the statement that “no linear trend” is evident in the $\delta^{18}\text{O}$ during the early Holocene.

P 154, lines 19: it is unclear here when the authors talk about dust or when they refer

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to $\delta^{18}\text{O}$ here; also, if I understand correctly which is which, doesn't the early Holocene (temperature?!) "optimum" coincide with "high" (not "low") dust flux values? Please clarify/rephrase.

Section 4.2, P 155-156, bridging sentence: difference in transport altitude/mechanisms between coastal and high-elevation sites was also observed on the other side of the East Antarctic ice-cap based on flux and grain size measurements (see Figure 3 in Bory et al. [2010] comparing data obtained at Berkner and Dronning Maud Land). As rightly stated in the abstract, there has only been limited investigations in peripheral area of the Antarctic ice sheet and so I think it is all the more important not to forget to quote the few existing ones.

P 156, line 9: a brief description of what "barrier winds" would be welcome.

P 156, last sentence of the first paragraph (lines 13-16): this statement requires to be explained in greater details; the author may want to specify for instance that lower temperature are associated with higher dust flux, and also, for which period they consider this to be true (this is clearly not the case during the ACR). In any case, the link the authors make ("we can "THUS" consider the Taldice $\delta^{18}\text{O}$... as a proxy for dust transport") is not obvious and should be clarified.

P 156, line 24-26: this has already been mentioned (P 151, lines 7-11). P 156, line 27-29: as above ("pre-dating the termination of the ACR" is already mentioned twice (at least), P 152 line 19 & P 154 line 6); again, watch for reiterations, which dilute and therefore weaken the discussion.

P 157, lines 2-5: although this hypothesis may hold true when comparing the ACR and the early Holocene periods (as dust flux increases throughout the transition), it is not applicable to the LGM-Holocene changes; indeed, as acknowledged P 156 lines 22-24, there is little change in the 5-10 μm dust flux across the transition; there is even a reduction in the 5-10 μm flux between the LGM and the early Holocene. So, is there really a need for additional sources to explain the data? And if, as discussed in the

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text, more sources are indeed becoming available during the deglaciation (in addition to the one existing during the LGM), it should then be mentioned that this implies a weakening of dust transport from these local sources during the Holocene compared to the LGM.

P 157, line 18: is the long-term increase of Katabatic winds a hypothesis (in which case, this should be indicated by “hypothetical” long-term . . .) or a proved phenomenon in the late Holocene (in which case, the author should provide a reference).

P 158, line 5: are deglaciation events (P 155, line 25: “the retreat of the Ross Ice Sheet margin [. . .] was completed by ~8kyr BP at Terra Nova Bay”) really “coveal” to the decreasing trend (8-2 kyr BP)?

P 158, lines 7-10: the link between the retreat of the Ross Ice Sheet and a longer and cooler distillation pathway (that would occur AFTER) seems counterintuitive (to me at least) and should thus be explained in further details.

P 158, lines 10-14: an increase in the $\delta^{18}O$ during the late Holocene seems coherent with a reduced ice-cover in the Ross Sea area ; however, how the Ross Ice Sheet retreat and subsequent reduced ice-cover in the area throughout the late Holocene may then lead to reduced dust transport to Talos should be explained better; or, if there is no explanation, this should then be clearly stated. Overall, the discussion in the paragraph bridging P 157 & 158 is difficult to follow (the first sentence, for example, which indicates that air masses trajectories bringing moisture to Talos today were hampered during the LGM seems to suggest that there were less efficient transport inland at that time. . . which seems contradictory with the fact that more dust was transported from local sources during the LGM).

P 158, last paragraph of the discussion: as above, the link between changes in the Ross sea area (throughout the deglaciation and the Holocene) and the reduced aeolian deflation of ice-free areas in Northern Victoria Land is unclear. I suggest the authors rewrite the last couple of paragraphs of the discussion in order to make clear what

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the hypothetical links with atmospheric transport patterns are, or to make clear that although some connection is supported by the important changes documented in the Ross sea area, the mechanisms are not yet understood.

Figure 4: this plot shows that there are remarkable similarities in the variability of the dust flux at EDC and Taldos during the late Holocene (similar ups and downs between 6 and 2 kyr BP); this seems to support the evidences pointing to possible changes in atmospheric pathways during the late Holocene (P 157, lines 21-23), but this may also suggest that the changes that affected transport from local sources may have been connected to larger scale atmospheric features (as evidenced by the variability observed at EDC). I didn't see any mention of it in the text (maybe I just missed it), but I think this an important point that should clearly be discussed. Similarly, a connected point that should also be examined is the fact that most of the decrease in dust flux during the late Holocene is due to the decrease in the 1-5 μm fraction (Figure 3). Two possibilities: much of it is locally derived (which would imply the connection mentioned above between large scale atmospheric features and regional ones in Northern Victoria Land) or most of it is from remote sources as in EDC and then the decrease must tell something about reduced transport efficiency during the Late Holocene with respect to EDC where there was only little changes throughout the Holocene.

The smoothed profiles in figure 4 seems somewhat less smoothed that the ones in Figure 3 (c, d): were identical running means calculated in both figures?

Minor points

Results:

P 150, lines 27-29: I would replace “resembles” (which seems to me a little too strong here considering the significant differences between the two records) by “bear some resemblance to” or equivalent. Alternatively, the authors may want to replace the sentence by something like “Although the main climate transitions are clearly visible in both the 1-5 and the 5-10 μm dust records, there is only minor variations in the magnitude

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of the coarse mode flux throughout the LGM-Holocene period compared to the fine fraction”.

P 151, line 3: definition of “metric”?

Section 4.1: I think it would be easier to follow this part of the discussion if Taldice and EDC were compared chronologically, i.e., if paragraph 4 (LGM) came before para 3 (LGM-Holocene transition).

P153, line 27: one might expect a “provenance” reference here (Delmonte et al. [2010] for instance), instead of Lambert et al. [2008].

P154, line 11: replace “preceded” by “followed”.

P 154, last paragraph, line 21: I would suggest to start the sentence with “As a summary, ...”

Figure 4 caption: (a) is showing EDC data, not Taldice data, which are shown in (c).

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